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EDITOR

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SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

ASTRONOMY. By W. M. Smart, M.A., D.Sc., Observatory, Cambridge.

In the *Annales de l'Observatoire de Strasbourg*, Tome II (1er fasc.), M. A. Danjon presents a comprehensive research on astronomical photometry. A critical comparison of the great photometrical catalogues of Oxford, Potsdam, and Harvard leads him to the conclusion that these catalogues are affected by systematic errors exceeding a tenth of a magnitude and dependent on the magnitudes and spectral types of the stars concerned, and that the accidental errors are of much the same order of magnitude. Astronomical photometry has not yet attained the accuracy with which laboratory investigations of a similar nature can be undertaken; the difficulties associated with atmospheric absorption are the main hindrances to accurate and consistent determinations of stellar magnitudes. M. Danjon has used several photometers, with artificial stars as standards, in which filters of different colours play an important part. Although the author considers it yet premature to indicate the details of a careful plan for the compilation of a standard photometrical catalogue, he insists on the following principal points: (i) special study of atmospherical absorption, with the compilation of a special catalogue of stars for this purpose; (ii) observations of catalogue stars at constant altitudes; (iii) the use of colour filters for all observations. M. Danjon then proceeds to discuss differential photometry and gives a description of a new photometer suitable for this kind of work. In the second part of the volume, he describes several years' work on the magnitude determinations of several typical variable stars: δ Cephei, β Lyrae, β Persei. It is noteworthy that M. Danjon obtains from his visual photometrical observations of β Persei the small secondary minimum first detected by Stebbins from photo-electric observations of this eclipsing binary.

Volume X (Zone $+55°$) of the *Catalogo Astrografico* from the Vatican Observatory (Director, Father Hagen) completes the share—declination zones $+55°$ to $+65°$—undertaken by

this observatory in the vast international undertaking of photographing the heavens. The volume contains the rectangular co-ordinates (on the photographic plates) of 75,673 stars, together with the measures of the diameters of the stellar images. The photographs were taken and measured at the Vatican Observatory and the ensuing calculations were made at Oxford University Observatory under the direction of Professor H. H. Turner. To celebrate the completion of the work the Pope had special medals struck, and it is very gratifying to learn that Professor Turner and Mr. and Miss Bellamy of the Oxford Observatory were singled out as worthy recipients of this honour. Of twenty observatories scattered over the globe, eight have completed their programmes, seven are nearing the end of their labours, while the remaining five have still a considerable way to go, being hampered mainly by lack of funds for the printing of the volumes of the catalogue.

The first publication (Memoir No. 1) from the Commonwealth Solar Observatory at Mt. Stromlo, Canberra, Australia (Director, Dr. W. G. Duffield), is dedicated to the generous benefactors who have made valuable gifts, to the generous contributors to the foundation fund of the observatory, and to "those leaders of scientific thought in many countries who for so many years gave their influential support and encouragement to the movement to found this observatory." The memoir deals with the luminosity of the night sky, observed with a Rayleigh photometer, during the years 1926 and 1927. In this instrument—supplied to the observatory by Lord Rayleigh—light from the night sky is admitted through an aperture and by a simple optical arrangement compared with the standard illumination of a self-luminous uranium salt. Three coloured filters are introduced in succession across the aperture; one filter transmits chiefly red light, the second a part of the spectrum including the auroral radiation λ 5577, while the third transmits blue light. The general illumination of the night sky has a background of continuous spectrum due to scattered sunlight or moonlight or to the integrated light from the stars, and one feature of the investigation is the elimination of the continuous spectrum effect so that the auroral radiation can be deduced. The curves obtained from the observations show that the auroral radiation increases very markedly in April and May. A pronounced auroral display and the zodiacal light were observed with the photometer.

In *Meddelanden från Lunds Observatorium*, Ser. II, No. 52, Dr. W. Gyllenberg evolves a mathematical theory which, in conjunction with counts of stars of different magnitudes in adjacent nebulous and non-nebulous regions, enables the

distances of the obscuring nebulæ to be estimated. Consider first a non-nebulous area of the sky in which counts of stars have been made. If the results are plotted with magnitudes (m) as abscissæ and the logarithms (for convenience) of the numbers of stars brighter than given magnitudes as ordinates, the resulting curve slopes upwards as m increases. Consider now a nebulous area in the same way. The curve in this instance will be similar to the first curve up to a certain value of m; thereafter, the obscuring effect of the nebula comes into play and the curve will cease to rise. In developing the mathematical theory, Gyllenberg adopts two hypotheses: the first is that of Seeliger to the effect that the density of stars, $D(r)$, at the distance r is given by $D(r) = Ar^{-k}$; the second is that the frequency distribution of the absolute magnitudes of the stars is given by the normal Gaussian error function. For the second hypothesis, the value of the mean absolute magnitude is necessary, and Gyllenberg adopts the value arrived at in the work of Kapteyn and van Rhijn. In the application of the theory the author uses the star-counts (to the seventeenth photographic magnitude) obtained by Professor M. Wolf in the two nebulous regions near ξ Cygni and S Monocerotis. His values of the deduced parallaxes of these nebulæ are 0·″006 and 0·″013 respectively, in good agreement with estimates made by other investigators using different methods.

Lick Observatory Bulletin, No. 408, is devoted to an interesting paper by N. T. Bobrovnikoff on the disintegration of comets. Recent investigations have established the fact that the brightness I of a comet varies according to the formula $I = I_0\rho^{-2}r^{-n}$ where ρ and r are respectively the comet's geocentric and heliocentric distances. I_0 is defined by the author as the absolute brightness. Converted into the scale of magnitudes, the formula becomes $H_0 = H - 5 \log \rho - 2{\cdot}5\, n \log r$, in which H_0 and H are the absolute and observed magnitudes. This is the equation used in conjunction with the observed magnitudes to determine H_0 and n. From a study of two hundred comets, Vsechviatsky had, a few years previously, concluded that the value of n is about 4 and that the absolute magnitudes of the comets studied increase with the semi-major axis a. The author points out that no consideration was given to the eccentricity e which is generally close to unity, and that it is evident that two comets with the same a but different e will be subject in a different degree to the disintegrating influence of the sun. In the first formula quoted the part of the comet's brightness due to reflection of sunlight is proportional to $\rho^{-2}r^{-2}$; the remaining factor r^{2-n} is due to excitation by the solar radiation. As the fluorescence of comets is accompanied by the continuous loss of matter ejected from the nucleus to

form the coma and the tail, the author takes (for $n = 4$) the dissipative factor producing the secular decrease in brightness to be Ar^{-1}. A simple formula then enables him to calculate the change in magnitude after one revolution. As a working hypothesis he then assumes that comets came into existence about the same time and that they were equally bright at the beginning of their careers. These hypotheses are then tested by the available statistics pertaining to ninety-four comets. For example, there is found a definite correlation between the present absolute magnitudes H_0 and a dissipative function derived from theoretical considerations, which seems to indicate that the hypotheses are statistically probable. The author then refers to the cosmological inferences suggested by this work. It is believed that the disintegration or fading away of Encke's comet proceeds at such a rate that its absolute magnitude increases by about one magnitude per century. From estimates of this character, the author infers that the upper limit of the age of the cometary family is about one million years—a view that is inconsistent with one of the alternative theories that comets were born at the same time as the planets. According to the author, there is evidence that comets are true members of the solar system and not chance captured visitors from interstellar space. His view of the family of comets is that they were born as the result of the sun's passage through a diffuse nebula at a time when the sun was some seventy light-years distant from its present position in the general direction of Orion, where agglomerations of nebular and meteoric matter are suspected.

No. 373 of *Contributions from Mt. Wilson Observatory* (*Astrophysical Journal*, Vol. 68) contains a very interesting account of a conference held at Mt. Wilson concerning the famous Michelson-Morley experiment. Lorentz, Michelson, and Miller took part in the proceedings, and the latter described the results of a series of recent ether-drift observations, which are claimed to indicate that the absolute motion of the solar system is at least 200 kilometres per second towards a point close to the pole of the ecliptic. Since the conference, Michelson has repeated his experiments at Mt. Wilson with a view to testing Miller's results, but is unable to observe the effects predicted by Miller.

In No. 365 of the *Mt. Wilson Contributions*, Miss C. E. Moore and Professor H. N. Russell calculate the wave-lengths of iron lines occurring in multiplets and not observed in the laboratory. A search in the solar spectrum has resulted in the identification of 289 of those calculated with previously unidentified solar lines.

At the last meeting of the International Astronomical

Union at Leiden in 1928 the committee on Solar Physics decided to publish a quarterly bulletin dealing with solar phenomena. Thirteen observatories scattered over the globe are co-operating, the headquarters being at Zürich Observatory. The first number deals with the first quarter of 1928. The bulletins, of which three have now been published, give daily " characteristic figures " which denote the intensity of various solar phenomena ; this information is expected to be of great assistance in the search for relationships between happenings on the sun and terrestrial phenomena (magnetic storms, etc.). The characteristic figures relate to sunspot activity, intensity of ultra-violet radiation, calcium flocculi, bright and dark hydrogen flocculi.

PHYSICS. L. F. BATES, B.Sc., Ph.D., F.Inst.P., University College, London.

American Contributions.—A very interesting application of the new quantum mechanics to the theory of radioactive disintegration is given in a paper by R. W. Gurney and E. M. Condon (*Phys. Rev.*, vol. 33, p. 127, 1929). For a long time α-particles have been considered essential constituents of the nucleus, and on the basis of classical mechanics we have supposed them to be located in regions of low potential energy inside the nucleus. How, then, could they escape from the nucleus without a supply of energy from external sources ? The answer is now given, that, whereas on the basis of classical mechanics we could not understand how an α-particle could pass from a region of low potential energy through a kind of high potential barrier to leave the nucleus, yet on the new quantum mechanics the α-particle, regarded as equivalent to a group of waves, possesses a definite probability that it will be able to leak through this barrier in a given time. The application of this conception is shown to lead directly to the well-known law of radioactive decay. Gurney and Condon also succeed in providing a theoretical explanation of the Geiger-Nuttall relation between the rate of disintegration and the energy of the α-particle emitted during the radioactive process. They also show that the reason why no α-particles of range less than 2 cm. are found, is that on the new quantum mechanics, the predicted rate of decay would be so small that it would lie beyond the limits of experimental detection. It is extremely interesting that they conclude that the law of force between the emitted α-particle and the remainder of the nucleus is almost the same for all radioactive atoms, even though their rates of decay differ by a factor of 10^{21}. The experimental results of Rutherford and Chadwick on the behaviour of uranium are cited as

direct evidence of the correctness of the above views. It will be remembered that Rutherford and Chadwick found that the scattering of fast α-rays by uranium nuclei indicated that the inverse square law of repulsion held down to a distance of $3{\cdot}5 \times 10^{-12}$ cm., the closest distance of approach. The nucleus of uranium must therefore have a radius less than this, and yet the velocity with which an α-ray is expelled from a disintegrating uranium nucleus indicates that the nucleus must extend to a distance of $6{\cdot}5 \times 10^{-12}$ cm. In other words, two equally reliable methods lead to very diverse results for the size of the nucleus. The discrepancy is easily surmounted by assuming an attractive force to act at small distances and a repulsive force at large distances from the nucleus, which is the same as assuming the existence of a high potential barrier such as is considered by Gurney and Condon. Interesting remarks bearing on this problem were made by Sir Ernest Rutherford and Dr. Gamow at the Royal Society discussion on the structure of atomic nuclei on February 7 (cf. *Proc. Roy. Soc.*, vol. 123, p. 373, 1929). It should be mentioned that Gamow had independently arrived at results similar to those above (cf. *Zeit. für Phys.*, vol. 51, p. 204, 1928).

English Contributions.—At the meeting of the Royal Society on February 21 three papers of very great interest were read. The first was read by P. Kapitza, and dealt with the change of electrical conductivity of metals when placed in very intense magnetic fields (cf. *Proc. Roy. Soc.*, vol. 123, p. 292, and p. 342, 1929). To those familiar with the skill with which this worker designs and obtains complicated and expensive apparatus, it will perhaps not come as a great surprise to learn that he has collected some thirty-five different metals, the majority of a purity of over 99·9 per cent., for the purpose of these new investigations. The experiments deal with fields up to 300 kilogauss. It will be remembered (cf. *Sci. Prog.*, vol. 23, p. 205, 1928) that the earlier experiments showed that the resistance of bismuth increased in proportion to the square of the field when the latter was small, and in direct proportion to the field when the latter was intense. This behaviour of electrical resistance at room temperature, at the temperature of liquid carbon dioxide and at the temperature of liquid nitrogen, has now been proved to be general for all the metals examined. The linear portion of the graph of resistance against field, which is usually found with fields above 60 to 100 kilogauss, appears in most cases to be independent of the crystalline state of the metal and to depend upon the position of the element in the periodic table. The experimental procedure was that

employed in the previous work (*Proc. Roy. Soc.*, vol. 113, p. 358, 1928), with the addition of certain refinements necessary for dealing with metal specimens in the form of short rods, wires, or liquid threads. The purity of the materials was examined by spectroscopic methods, and it was found that in order to obtain trustworthy results, the materials had to be not less than 99·9 per cent. pure, a 1 per cent. impurity being sufficient to render the results useless as a basis of discussion. Special precautions were therefore taken when the metals were drawn or extruded to form wires. It was found convenient to plot the relative change of resistance, $\Delta R/R$, against the magnetic field in kilogauss, and the following formulæ were found to hold :

$$\Delta R/R = \beta \cdot \frac{H^2}{3H_k}, \text{ for } H \lesseqgtr H_k$$

and

$$\Delta R/R = \beta\left(H - H_k + \frac{H_k^2}{3H}\right), \text{ for } H \gtreqqless H_k,$$

where β is the slope of the assymptote to the graph at high values of the field. The two formulæ, of course, represent a continuous line, and it may be shown that these formulæ can be deduced by strict mathematical reasoning from the theoretical considerations given below. Before passing to these considerations, however, we ought to mention one or two points of exceptional interest revealed by the experiments. For instance, the state of hardening of copper specimens had a profound effect on the change of resistance in the field, harder specimens giving considerably higher values of the critical field, H_k, but approximately the same values of β; similar results were noted with the elements, silver, gold, cadmium and aluminium. The changes observed in the cases of iron and nickel were very small and complex, and have not yet been completely investigated. Their complexity may be due to impurities or to the superposition of phenomena of ferromagnetic origin upon the usual increase of resistance in a magnetic field as observed in other elements. It is interesting to note that the suggestion made by de Haas (*Proc. Roy. Amst. Acad.*, vol. 16, p. 1110, 1914), that strongly diamagnetic substances should exhibit the largest changes of resistance, was not confirmed. The transition elements, which are strongly paramagnetic, certainly showed small changes, but chromium, molybdenum, and tungsten showed very large changes. The influence of impurity was very marked ; an impure specimen of silver, for example, showed about half the change observed with a pure specimen.

The interpretation of these phenomena given by Kapitza

is rather simple and attractive. He assumes that below the critical field H_k, the appearance of the linear increase of resistance with field, which he regards as the true law of change of resistance, is in some way hindered. He assumes that the hindrance is an initial disturbance randomly distributed in the metal, and that the effect of this disturbance is similar to that produced by an external magnetic field. If an external magnetic field is applied to the metal, the increase of resistance observed is due to the vectorial sum of the initial and applied disturbances, and when the disturbance due to the external field becomes sufficiently large, we have the gradual approach to a linear law. A mathematical treatment based on these assumptions leads to expressions for the change of resistance of the nature demanded by experiment. The treatment leads to the conception that the metal possesses an ideal resistance, R_i, when no internal or applied disturbances are present, and that the ordinarily observed resistance is therefore the sum of the ideal resistance and an increment ΔR_0, due to the initial disturbance. The quantity ΔR_0 may easily be obtained from the point of intersection of the assymptote with the axis of change of resistance. An examination of the experimental results showed that the ideal specific resistance of different specimens of the same metal (such as copper, silver, etc.) was practically the same for all the specimens, in agreement with the above assumptions, whilst the value of the additional resistance, ΔR_0, was much affected by the physical state of the specimen; for example, the additional resistance of a specimen of hard drawn copper was about two and a half times as great as that of the same specimen when annealed. In agreement with the above assumptions, the experiments showed that the ideal resistance is the same when the magnetic field is parallel or perpendicular to the direction of the current in the specimen. It was concluded that to a first approximation ΔR_0 is unaffected by temperature, but the ideal resistance diminishes rapidly with decreasing temperature. Now, in the well-known experiments of Kammerlingh Onnes, it was found that many materials when cooled to temperatures in the neighbourhood of absolute zero, possessed a resistance which was more or less constant in this region, which was called the residual resistance. The extremely important fact which emerged from Kapitza's results was that ΔR_0 and this residual resistance were identical. The magnitudes of the two quantities for a large number of metals were practically the same. Hence there now arises the interesting question of the behaviour of the supraconductors, mercury, thallium, tin, lead, and indium. They all obeyed the general law of increase of resistance with magnetic field, although they gave small values for β. It

was found, however, that the values of ΔR_0 were about the same as the values of the resistance just above the threshold temperature. This indicates that supraconductivity really means the disappearance of the additional resistance ΔR_0, and, moreover, that all metals would be supraconductors if only we could get rid of their chemical and structural imperfections. In the light of these remarks we may understand how an external magnetic field, or a magnetic field due to a current in the metal itself, may cause a supraconductor to acquire a finite resistance even when it is below its threshold temperature, and we understand the observations of Kammerlingh Onnes (*Leiden Comm.*, No. 50*a*, p. 15, 1924) that strains actually lower the threshold temperature in the cases of tin and indium; but this, of course, tells us nothing about the mechanism which produces the additional resistance.

When we come to discuss the bearing of these experiments upon the theory of metallic conduction, we see that we have to explain why a magnetic field produces a change of resistance at all, and why there exist ideal and additional resistances. The linear change of resistance with magnetic field is not predicted by the older theories of J. J. Thomson and Gauss or by the more recent theory of Sommerfeld (cf. *Sci. Prog.*, vol. 23, p. 21, 1928), which are based on the deflection of free electrons by the magnetic field. The initial disturbance in some cases is very large, *e.g.*, often over 100 kilogauss, which is too large to be attributed to an internal field of true magnetic origin. We cannot, however, easily assume that the field is likely to change the number of free electrons or to produce an irregular deformation of the crystal lattice, and Kapitza is inclined to the view that the magnetic field affects the scattering power of individual atoms. It should be emphasised that these researches only aimed at a general survey of the phenomena over the available elements of the periodic system, and not at a detailed study with a few elements. It is noteworthy that the valuable results so far obtained may be adequately explained in terms of an initial disturbing field, and it appears to the writer of this article that such a disturbance is immediately established when a difference of potential is applied to the ends of the conductor, if the atoms may be looked upon as dipoles.

The second paper read at the Royal Society on February 21 dealt with the problem of long range α-particles from the active deposits of radium and thorium. These particles are very difficult to investigate, mainly because of the large number of hydrogen particles which are emitted by the radioactive sources, and Nimmo and Feather (*Proc. Roy. Soc.*, vol. 122, p. 668, 1929) are to be congratulated on the success

with which they have eliminated this source of confusion. They examined the particles emitted by thorium C by the Wilson expansion chamber method, and the 1,200 photographs they obtained showed 563 long range tracks, of which 9 had ranges between 12·5 and 17 cm. in standard air (air at 15° c. and 760 m.m. pressure). The remaining tracks fell into two groups of ranges 9·9 and 11·7 cm. (*cf.* Meitner and Freitag, *Zeit. für Phys.*, 37, 481, 1926), and the curves showing the distribution of the particles with range, indicated clearly that they were due to α-rays. With radium C, 461 long-range tracks were observed, and a much more complicated state of affairs was found to exist than previous experiments had led us to believe (*cf.* Bates and Rogers, *Proc. Roy. Soc.*, vol. 105, p. 97, 1924, and Rutherford and Chadwick, *Phil. Mag.*, vol. 48, p. 509, 1924, *Yamada, Journ. de Phys.*, vol. 6, p. 380, 1925). A group of particles with range just over 9 cm. was found, and there was strong evidence for the presence of other groups with ranges of 8·1 and 11·0 cm., and possibly another group of range 10·0 cm. In fact, the possibility of a continuous spectrum of α-particles from radium C should not altogether be ruled out. In addition, some particles of range over 12 cm. were found. Are these α-particles really emitted as groups of constant energy from the nucleus? The evidence at present is not sufficient to enable us to answer this question, but if the particles from radium C form a continuous spectrum, then we must face the problem of the source of energy of the long-range particles. Phillip and Donat (*Zeit. für Phys.*, vol. 52, p. 759, 1929) have also examined the particles from radium C by the expansion chamber method, but their results do not appear to be so complete as those of Nimmo and Feather, for they find two groups of ranges 9·2 and 11·0 cm., and a few particles of range greater than 11 cm.

The third paper read on February 21 was very aptly described by the President, Sir Ernest Rutherford, as a striking example of the success obtained by the application of intelligence to the solution of an old and familiar problem. C. R. Burch (*Proc. Roy. Soc.*, vol. 123, p. 271, 1929) has certainly achieved success in his attack on the problem of vacuum distillation. He has carefully considered the ways in which modern vacuum technique may be applied to the fractional distillation of organic substances in general, and, in particular, to the fractionation of petroleum and its derivatives, and he has managed to distil these substances of very high molecular weight under conditions which are practically equivalent to evaporation into a perfect vacuum. The efficiency of his still is due to the presence of a cold condensing surface a short distance above the surface of the liquid being distilled, so

that a molecule of vapour may reach the condensing surface without suffering a collision, even although the pressure of mercury from the diffusion pump may be as high as 1 microbar. Explosive ebullition and frothing must clearly be avoided when the condensing surface is so close to the liquid, and dissolved gas must therefore be removed slowly by reducing the pressure by stages, the condensation pump being started when the pressure is about 50 microbars. A speed of about 10 drops per minute is regarded as a maximum speed desirable in fractionation, although in some cases a speed of 60 drops per minute was obtained. Once decomposition sets in, the distillation must be stopped, owing to the violent splashing which occurs. To the physicist the importance of these experiments becomes very clear when it is mentioned that the author has produced greases and oils with such minute vapour pressures that they may be freely used to lubricate ground glass joints, etc., in high vacuum systems. Moreover, it has been proved that these oils may replace mercury in condensation pumps and do away with the use of liquid air-traps in many cases, such as in the evacuation of thermionic valves.

A paper of interest to all teachers of advanced physics and those interested in dielectric measurements is that by Hartshorn and Oliver (*Proc. Roy. Soc.*, vol. 123, p. 664, 1929). The object of their research was to obtain the absolute value of the dielectric constant of at least one standard liquid with unquestionable accuracy, and benzene was chosen for the purpose. A simple Wheatstone network system of measurement was employed. The effects of lead capacity were most carefully eliminated, mainly by the use of a particularly well-designed condenser of large capacity. The only disadvantage of this condenser was that it required about 500 cc. of liquid to fill it, and as this is much more than is usually available in a state of great purity, absolute measurements were made with great accuracy on commercial benzene (A.R. grade). The dielectric constant of the liquid thus standardised was compared with highly purified samples by the usual Nernst comparison method, *i.e.*, by measuring the capacity of a smaller test condenser when filled with air, when filled with the standardised liquid, and when filled with pure liquid. The dielectric constant of pure benzene at 20°c. was found to be $2{\cdot}282_{5\pm5}$ (vacuum = 1), the probable error being given as ± 2 parts in 10,000. The dielectric constant decreased linearly with rise in temperature.

At the Glasgow meeting of the British Association, E. Griffiths gave a preliminary account of experiments which J. H. Awbery and he had made on the measurement of flame

temperatures. A complete account of this work is now available (*Proc. Roy. Soc.*, vol. 123, p. 401, 1929). They have employed two new methods of measurement. The first, called the "wire method," is based on the fact that an electrically heated wire placed in a flame may lose heat by radiation, by conduction to its supports, and by transfer to the hot gases in the flame, whereas the same wire heated in vacuo will not suffer heat losses by the third mode of transfer. If the temperature of the flame is the same as that of the wire, then the wire again will not suffer heat losses by the third mode of transfer. Hence the "wire method" of measurement consists in finding that temperature at which both the heating current and the temperature of the wire are the same in the flame and in vacuo. The temperature of the wire is found by an optical pyrometer. The heating current and temperature are plotted both for the wire in the flame and the wire in vacuo, and the intersection of the two graphs gives the required temperature of the flame. Wires of different diameters give the same values of the flame temperature, which agree with that found by the second method. The second method is rather more accurate, and is known as the "spectral line reversal method." Its basis is Kirchoff's observation that the dark lines in the solar spectrum correspond to the spectra of various elements. The condition which decides whether the lines appear dark or light is the relative temperature of the source and the intervening material. This would suggest that when the lines just fail to be reversed, the temperature of the source is equal to that of the absorbing medium. In the measurements now described, the glowing sphere of a "pointolite" lamp is sharply focussed upon, and to more than cover, the slit of a spectrometer. A meker-flame is supplied with salt by a Gouy spray and placed between the "pointolite" and the spectrometer. The current in the lamp is adjusted until the sodium lines just merge into the continuous background, and an optical pyrometer reading then gives the temperature of the glowing sphere, which is also the temperature of the flame. Flames containing lithium and sodium were also used, and it was found that the two sets of lines reversed simultaneously. The thickness of the flame had very little influence on the results, but greater sensitivity was obtained with thick flames. The authors also used the "reversal method" to investigate the maximum temperature in gaseous explosions. The explosion tube contained powdered sodium carbonate, and by successive approximations the temperature of the "pointolite" sphere was found when the D lines just failed to reverse at the instant following the explosion.

Ditchburn and Arnot (*Proc. Roy. Soc.*, vol. 129, p. 516,

1929) have applied the method of investigation of the m/e ratio of positive ions, initiated by Smyth (*Proc. Roy. Soc.*, vol. 102, p. 283, 1922, and vol. 104, p. 121, 1923), to the examination of ions produced in potassium vapour by various means. In one set of experiments the ions were produced by photo-electric ionisation, in which case the only stable ion formed was K^+, and evidence is given to show that this ion was produced from K_2 molecules. In another set of experiments, a beam of K^+ ions from a Kunsman filament was accelerated into the vapour, so that ions might be formed by their attachment to neutral atoms. In this case, stable and unstable K_2^+ ions were found, the number of K_2^+ initially formed being about ten per cent. of the number of the incident K^+ ions. They could have been formed either by the attachment of K^+ to neutral K atoms or by impact ionisation of K_2 molecules by K^+ ions, the first mode of formation being the more probable. There was some evidence that K_3^+ ions were formed, and that these later dissociated with the formation of K^+ and K_2^+ ions. Some of the K_2^+ ions dissociated on entering the magnetic field, forming K^+ and K, and this appears to support the evidence of Franck and Grotian (*Zeit. für Phys.*, vol. 6, p. 35, 1921) and Hogness and Lunn (*Phys. Rev.*, vol. 26, p. 786, 1925, and vol. 27, p. 732, 1926) that unstable ions are affected in an unknown manner by a magnetic field and caused to dissociate. In a further set of experiments, ions were formed by electron impact, and K_2^+, K_3^+ and K_3^{++} were found. The results depended very markedly on the velocity of the impacting electrons, the results indicating that K_2^+ ions formed by the impact of high velocity electrons were in a more highly excited state than those formed by low velocity impacts. Negative potassium ions were found but were not completely investigated. The authors give a valuable discussion of their results and their correlation with the band spectra of the alkali metals and other optical data. They give a qualitative explanation of their results, in which, it may be mentioned, they assume that the moment of inertia of the K_2 molecule and that of the K_2^+ ion are always increased on excitation.

German Contributions.—All teachers of physics who have suffered under the limitations of the method of Clement and Desormes for the determination of the ratio of the specific heats of a gas, will be interested in the description of a new and simple method by Rüchardt (*Phys. Zeit.*, vol. 30, p. 58, 1929). It is now possible to obtain commercially, from Leybold, a glass tube about 55 cm. long and diameter 1·6 cm., whose internal cross-section is so uniform that a precision spherical steel ball of the same diameter will slide airtight in the tube. If such an open tube is placed vertical, the

ball will, of course, fall, but if the lower end is suddenly closed, the ball is thrown into rapid vibration and will fall only very slowly. If the tube passes through a rubber stopper into a bottle of 5 to 6 litres capacity, provided with a tap, and the ball is allowed to fall, then on closing the tap, the vibrations set up are only slightly damped and have a period of about one second. The process of compression and rarefaction of the gas in the apparatus is adiabatic, and a simple calculation shows that the ratio of the specific heats of the gas in the apparatus is given by the expression $4\pi^2 \cdot m \cdot V / q^2 \cdot p \cdot T^2$, where m is the mass of the steel sphere, V the volume of gas in the apparatus, p its pressure, q the area of cross-section of the tube, and T the observed period of vibration. The experimental values appear to be limited only by the accuracy with which the period is determined.

A very neat method for the determination of very small displacements by means of a photoelectric cell or thermocouple is described by Cristescu (*Phys. Zeit.*, vol. 30, p. 24, 1929). Suppose we take a point source and by means of a convex lens obtain a parallel beam of light which may be brought to a focus on a photoelectric cell or thermocouple by means of a second lens. Two similar gratings are placed parallel to one another between the lenses and perpendicular to the beam. It is obvious that the maximum deflection of a galvanometer connected to the cell or thermocouple will be recorded when the transparent spaces of the two gratings are so placed that the maximum amount of light passes through them. If, now, one of the gratings is displaced in a direction perpendicular to the parallel beam, the intensity of the light falling on the receiving apparatus will be reduced. The sensitivity of the galvanometer may be arranged to give a deflection proportional to the intensity of the light, and thus a small deflection may be accurately measured. With gratings of only five lines per m.m., displacements of 0·1 mm. could be arranged to produce galvanometer deflections of about 10 cm.

Among other German contributions, to which we can only briefly refer, is a paper by Kluge (*Ann. der Phys.*, Ser. 5, vol. 1, p. 1, 1929) who describes an apparatus for producing frictional electricity in vacuo. He shows that when platinum, gold, and palladium are rubbed with untreated silk in high vacuo, the sign of the charge is always reproducible. The magnitude of the charge thus produced depends on the surface of the metal; and degassing causes an increase in the charge, which is decreased when air is again allowed in contact with the surface. Incidentally, platinum which has been heated white hot during degassing, shows an initial reversal of sign

when rubbed with untreated silk. This appears due to a layer of powdered platinum, for further rubbing produces the usual negative charge on platinum in high vacuo.

A paper by Buchner (*Ann. der Phys.*, vol. 1, p. 40, 1929) deals with the phenomenon which was considered by Glaser (cf. *Sci. Prog.*, vol. 20, p. 203, 1925) to be an anomalous effect in diamagnetic gases. Buchner shows by means of a model apparatus, that the apparent anomaly may be produced by convection effects which vary with the pressure of the gas, and result in the temperature of the paramagnetic rod used in Glaser's work being non-uniform even when the apparatus is enclosed in a thermostat.

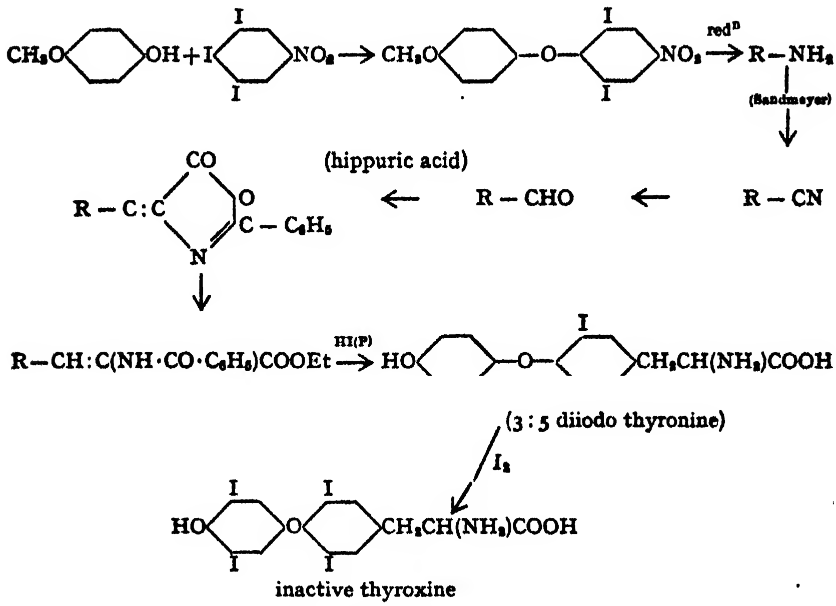
Rupp (*Zeit. für Phys.*, vol. 53, p. 548, 1929) describes experiments carried out in order to determine whether the number of electrons of moderate velocity twice reflected from the surfaces of single crystals of copper depends on the angle which the surface second makes with the direction of the initial electron beam, and whether a homogeneous magnetic field has any influence on the reflection. In other words, on the assumption that the electron may be treated as a group of waves, the experiments were designed to determine whether electron waves are polarised, and whether there is any connection between their polarisation and the magnetic moment of the electron (cf. *Wolf*, *Zeif. für Phys.*, vol. 52, p. 314, 1928). The experiments gave no information on these points of interest.

BIOCHEMISTRY. By P. Eggleton, M.Sc., University College, London.

The Active Principle of the Thyroid Gland.—The advances made by Harington toward the synthesis of the active principle of the thyroid gland have been followed in earlier reviews in this series (Science Progress, Jan. 1927, p. 387). The work is now completed from the chemist's point of view, by the synthesis of i-thyroxine and its resolution into the d- and l-isomers, the latter being identical with the active principle of the gland. Harington and Barger (*Biochemical Journal*, 1927, **21**, 169) achieved the synthesis by the route shown on the next page.

In view of the similarity of the two phenyl groups in the molecule this seems a rather indirect route, but the lability of iodine substituted benzenes foiled all attempts at the more obvious synthesis from 1-5-iodophenol derivatives. The resolution of the optically inactive product was not found to be feasible: it was necessary to return to the penultimate stage of 3:5-diiodo thyronine. The formyl derivative was resolved by fractional crystallisation of its l-α-phenylethylamine salt. The dextro-rotatory isomer gave on decomposition formyl-l-3:5diiodo thyronine, from which the formyl group was removed

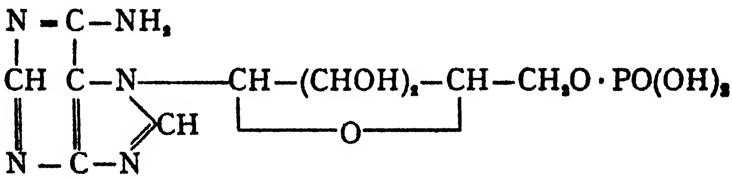
by hydrolysis with 15 per cent. HBr. Iodination in ammoniacal solution gave l-thyroxine. The dextro isomer could not be obtained pure from the same preparation, but the use of d-α-phenylethylamine in the same set of processes resulted in a good yield of d-thyroxine. The lævo compound, which is the naturally occurring isomer, has three times the physiological potency of its optical enantiomorph (*Journal of the Society of Chemical Industry*, 1928, **47**, 1346).



Ammonia Production in Muscular Activity.—A new turn has been given during the last year to the biochemical study of muscular activity by the discovery that ammonia is liberated in an isolated muscle when it contracts. It will be remembered that this field of biochemistry has undergone three rather startling changes recently ; much of what was spoken of as the " inorganic " phosphate of voluntary muscles has turned out to be organic—in a combination so labile that customary analytical manipulations brought about its hydrolysis—and much of the " organic " phosphate looks in a fair way to becoming inorganic, since Lohmann has adduced strong evidence for the presence of pyrophosphate to the extent of nearly a half of the so-called " acid-stable organic phosphorus." And, as if this were not enough, the elusive entity " lactacidogen," identified in 1921 as a hexosediphosphoric ester, has now become a hexosemonophosphoric ester, consequent upon the discovery that the di-ester is absent from normal muscles

(Embden and Zimmerman, *Zeitschrift für Physiologische Chemie*, 167, **114**, 1927).

The confusion is not lessened by this new study of muscle ammonia, for again it appears that phosphates are concerned. Embden and his colleagues (Embden *et al*, *Zeitschrift für Physiologische Chemie*, 1928, **179**, 149–237) extending the preliminary reports of Parnas, and their own initial studies, find evidence that the source of the ammonia liberated in fatigue is the purine derivative adenylic acid (adenine nucleotide):



Under the influence of a specific deaminase, the presence of which in muscle has been demonstrated by Gerhardt Schmidt (*loc. cit.*), this substance gives rise to hypoxanthine nucleotide (inosinic acid) and ammonia.

It appears that resting frog muscles, killed with every precaution to prevent traumatic changes, contain 10–20 parts per million of ammonia. If the muscle is first fatigued, or if taken from a fatigued frog, this amount may be trebled; if allowed to die and pass into rigor, or if cut up and incubated in a bicarbonate buffer solution, it is found to contain 200–300 parts per million—an amount which probably measures the quantity of the ammonia precursor in the muscle. These amounts are considerably smaller than the changes in lactate or even orthophosphate which occur simultaneously. In extreme fatigue, for example, the lactic acid production is usually 20 millimols per kilogram; the orthophosphate, 15–20; the ammonia, 2–3. It is unlikely, therefore, that this system contributes appreciably to the energy supply of the contracting muscle. Embden regards it rather as a chemical "trigger."

Parnas and his co-workers, who have studied the ammonia production following injury, have demonstrated (*Klinische Wochenschrift*, 1928, **7**, 1423) that although a fresh, rapidly killed muscle contains about 20 mg. of adenine nitrogen per 100 gm., an injured muscle contains only 5 or less, whilst the amount of hypoxanthine is correspondingly greater. This would account for the production of 30–40 parts per million of ammonia.

It must be admitted that although the ammonia produc-

tion is well substantiated, the evidence for its adenine-hypoxanthine parentage is as yet not strong, but whether this story is ultimately modified or not the impetus to research afforded by it is bound to be considerable. Hypoxanthine is a close relative, chemically speaking, of uric acid, and ammonia is easily converted in the animal body to urea,

$$2NH_3 + CO_2 \rightarrow CO(NH_2)_2 + H_2O.$$

The possible linking of these constant constituents of urine to muscle metabolism is obvious—but there are difficulties in the way. Exercise does not lead to any noteworthy increase in the excretion of these substances, and so far no changes have been observed in the urea content of an isolated muscle recovering from fatigue, although it is stated that the ammonia formed in activity disappears again in rest. The production of ammonia and the oxidation of purine bodies to uric acid by dying tissues is an old observation (the work of Jones and Partridge (*Zeitschrift für Physiologische Chemie*, 1904, **43**, 343) in this connection need only be mentioned); the novelty of these present observations lies in the fact that the phenomena are observed in surviving muscles capable of living for days if given adequate oxygen, and are to some extent reversible in the physiological, if not in the chemical sense.

The Curative Action of Liver in Pernicious Anæmia.—The disease of pernicious anæmia lost its sting two years ago when it was demonstrated by Minot and Murphy (*Journal of the American Medical Association*, 1926, **87**, 470; 1927, **89**, 759) that the feeding of large amounts of raw liver to patients suffering from this disease produced startlingly rapid abatement of its symptoms. Some information is now available as to the nature of the substance responsible: not enough, it is true, for us to form an opinion as to its structure, but enough to make possible fairly potent preparations of it for clinical use and further chemical study. The difficulty has been, and still is, that the disease cannot be produced experimentally in animals, so that all preparations have to be tested on hospital cases, which are none too common. Cohn, Minot, Alles, and Salter (*Journal of Biological Chemistry*, 1928, **77**, 325) have obtained very active preparations by extracting minced livers with dilute sulphuric acid, in the isoelectric range of the liver proteins; the protein free extract contains all the activity of the original liver, and if alcohol is added fractionally, the precipitate forming after the alcoholic concentration has passed 70 per cent. contains the principle. Basic lead acetate removed more impurities from the aqueous solution of this precipitate, and finally the active principle itself could be precipitated from solution by phosphotungstic acid.

The purest preparations, containing no carbohydrate, protein, or ether-soluble substances were curative in doses of rather more than 500 milligrams per day. Since doses of half a pound per day of raw liver pulp are necessary to obtain the same beneficial results, the therapeutical advance is considerable, even though the knowledge gained of the chemical nature of the principle is chiefly of a negative character.

The preparations contained as much as 17 per cent. of nitrogen although protein-free, but there are a number of known nitrogenous bases capable of accompanying the active principle through the series of steps outlined above, and it would be unwise at this stage to attribute much importance to this analytical figure.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., Ph.D., University, Glasgow.

Continental Drift, Isostasy, Tectonics and Igneous Activity.—The Wegener theory of continental drift still continues to excite much discussion and controversy. The most important contribution to hand is the remarkable symposium conducted by the American Institute of Petroleum Geologists, and edited by Dr. W. A. J. M. van der Gracht. For a review of this work see p. 153 of this volume.

In an interesting attempt to explain Wegener's continental drift by a new application of Joly's radioactivity hypothesis, which is, however, only at present available in abstract, Prof. A. Holmes concludes (*Geol. Mag.*, lxv, 1928, pp. 236–8), that there is now convincing evidence of the former occurrence of continental drift on the scale demanded by Wegener's theory. Admitting that, up to the present, no forces have been discovered adequate to produce the drift, he adopts a suggestion put forward by A. J. Bull, namely, that convection currents may be set up in the sima as a result of differential heating by radioactivity. Beneath the region of greatest heat output an upwardly-directed current would be produced which, exercising a powerful subsurface drag on the crust in two opposed directions, would lead to the formation of a geosyncline. A continental mass would be enabled to move forward by stoping the heavy ocean floor in advance of its movement. With setting-in of mountain-building, the directions of the currents would ultimately be reversed. In this way it is thought that the difficulties inherent in Joly's original hypothesis may be overcome.

Dr. F. Lotze has unfavourably discussed the Joly hypothesis for the explanation of cyclical geological events ("Die Joly'sche Radioaktivitätshypothese zur Erklärung der Gebirgsbildungen," *Nachr. Ges. Wiss. Gött., Math.-Phys. Kl.*, 1927, Heft 1, pp. 75–114) on the grounds that the physical bases of the hypo-

thesis are extremely ill-founded, and that the assumption of a periodicity of geological processes, and of a dualism (compression and tension) in tectonic forces, is not justified by the facts of geological history. He declares that the re-shaping and further development of the hypothesis by Holmes only makes it more complicated, and still more difficult to reconcile with the fundamental physical and geological facts.

With regard to the theory of the geological cycle, however, E. Kraus, in a paper on "Das Wachstum der Kontinente nach der Zyklustheorie" (*Geol. Rundsch.*, xix, 1928, pp. 353–86; 481–93), comes to a very different conclusion from that of Lotze. He attempts to answer a series of questions: Where are the recent geosynclines, and what is their nature? How do continents grow, and where does their material originate? Is continental consolidation final, or is there a reversion to oceanic conditions? The last question is answered by the conclusion that the process of continental formation is irreversible. Kraus claims that the recent geosynclines are to be found in present-day labile zones of the earth's crust, manifested by great variations of submarine relief, and by great seismic and volcanic activity. For the explanation of orogeny he resorts to the "magmatic streaming" theories of Ampferer and Schwinner. He distinguishes four stages in the life of a geosyncline: the pre-orogenic, deep-orogenic, high-orogenic, and post-orogenic. The continents grow by the accretion of successive orogenic belts around a primitive dome, and their materials originate by the still incomplete differentiation of a "Sal-sima" primitive magma.

The concluding chapter of Dr. N. L. Bowen's fine recent work on *The Evolution of Igneous Rocks* (Princeton University Press, 1928, 332 pp.) is entitled, "Petrogenesis and the Physics of the Earth." It deals penetratingly with topics similar to those discussed in preceding paragraphs. Dr. Bowen presents arguments to show that the actual heat loss from the earth's surface may be many times greater than that calculated from the thermal gradient, a point also made by Lotze. If this view be upheld by further data it would necessitate considerable modification of the present radioactivity tectonic theories.

Dr. Bowen also discusses the origin of basaltic magma, the existence of a supposed subcrustal layer of basaltic composition, and its physical state, whether glassy or crystalline. He points out that the remelting of a crystalline basaltic layer would be selective, and that the liquid thus produced would not be of basaltic composition. The selective fusion of peridotite, however, would provide material of the composition of plateau basalts, and Dr. Bowen prefers this method of obtaining original basaltic magma notwithstanding the great

difficulties admittedly involved. At the same time he emphasises the relative lack of data, and says that the problem of the origin of magmas is still far from approaching solution.

In an illuminating review of the interrelations of ore-deposits, magmatic activity, and the major tectonic structures and events of the earth, Prof. P. Niggli (" Erzlagerstätten, magmatische Aktivität, und Grosstektonik," *Zeitsch. f. Prakt. Geol.*, **36**, 1928, pp. 159–64 ; 185–91) is strongly of the opinion, especially with regard to the latest orogenic convulsions, that the tectonic reconstructions of Wegener, Argand, and Staub most closely harmonise with the characters and distribution of petrographic and metallogenic provinces.

The book by Prof. S. von Bubnoff entitled " Der Werdegang einer Eruptivmasse " (*Fortsch. d. Geol. u. Pal.* Bd. VII, Heft 20, 239 pp., 1928), is a typical production of what may be called the " magma-tectonics " school which has recently arisen on the Continent under the leadership of Prof. H. Cloos. The work attempts to correlate the tectonic and igneous events in the Variscian history of the Southern Schwarzwald. The main idea is that, in the affected region, the tectonic processes and the igneous activity begin at the same time and run concurrently. The nature of the igneous rocks injected at various stages depends not only on differentiation, but also on the stratigraphical and structural level on which the intrusions or extrusions take place, and on the tectonic processes that accompany them. The problems have been attacked by intensive field and laboratory study, with notable additions to knowledge on both petrological and tectonic sides.

The latest work of Prof. H. Cloos himself is entitled " Bau und Bewegung der Gebirge in Nordamerika, Skandinavien, und Mitteleuropa " (*Fortsch. d. Geol. u. Päl.*, vii, Heft 21, 1928, pp. 241–327). It deals with a large number of tectonic problems as illustrated in North America, Scandinavia, and Central Europe, impossible to summarise in this place. In this work he has made special investigation of tectonic movement which is going on at the present day. He has also an interesting section on the tertiary volcanic zone of Scotland and Northern Ireland, which he regards as having the same type of structure as the Palæozoic of the Oslo region of Norway. The individual fissures and faults strike N.W., although the whole zone strikes N.

The object of Dr. P. A. Wagner's valuable paper on " The Evidence of the Kimberlite Pipes on the Constitution of the Outer Part of the Earth " (*South African Journ. Sci.*, xxv, 1928, pp. 127–48) is to show that the xenoliths brought up by the kimberlite magmas of the South African diamond pipes afford direct evidence of the constitution of the sub-crustal

layers. The absence of any xenoliths that could be referred to a basaltic substratum shows that, in South Africa at least, this shell is missing. The abundance of the characteristic xenoliths indicates, on the other hand, that the granitic crust is underlain by a shell composed predominantly of ortho-amphibolites accompanied by gabbroid and granulitic rocks. The lowermost part of this shell is believed to be composed of a zone of gabbroid eclogite; and this, in turn, is underlain by the universal sima or peridodite zone, in which there are bands or lenses of pyroxenite and eclogite. All these types are represented among the xenoliths. In Wagner's opinion the amphibolite-gabbro-granulite shell is magmatically identical with basalt, and may constitute a vast potential reservoir of basaltic magma.

R. W. Goranson's excellent paper on "The Density of the Island of Hawaii, and Denisty Distribution in the Earth's Crust" (*Amer. Journ. Sci.* (5), xvi, 1928, pp. 89–120) provides a good statement of the methods of calculation of the theoretical values of gravity. After discussing the physical and geological restrictions which any hypothesis for computing isostatic compensation must satisfy, he adopts a modified form of the Airy hypothesis, which was found to agree well with that proposed by Heiskanen.

The results of the investigation show that a continent at sea-level is compensated at about 30 kms. depth with respect to the sub-Pacific crust, regions of higher elevation at correspondingly greater depths, and that the sub-Pacific crust differs from the continental crust in being uniformly femic down to a depth of about 60 kms. It is also shown that the volcanic islands of the Pacific are essentially uncompensated, and are subsiding loads on the sub-Pacific crust.

In his paper on "Isostasy and Mountain Building," C. O. Swanson (*Journ. Geol.*, xxxvi, 1928, pp. 411–33) claims that there is confusion between the broad fact of isostasy which almost every geologist accepts, and the various working hypotheses that have been constructed to explain the phenomenon. His object is to formulate a working hypothesis of isostasy which will be acceptable to both geologists and geodesists. To that end much of the discussion is devoted to the removal of two obstacles to the *rapprochement*; namely, the belief that geodetic calculation can be used to define the degree of crustal balance, and the claim that isostasy conflicts with the view that mountain folding is due to regional compressive stresses in the earth's crust.

In a paper entitled "Zur Einführung in die Phasen der palaözoischen Gebirgsbildung," Prof. H. Stille (*Zeitsch. Deutsch. Geol. Ges.*, **80**, 1928, pp. 1–26) discusses and tabulates the

mountain-making movements of the Palæozoic eras in Europe. As introductory to this work he discusses some tectonic questions, and defines such terms as "sub-phases," "phases," and "eras" of orogeny.

Prof. Stille's closely-packed memoir "Über Westmediterrane Gebirgszusammenhänge" (*Abh. Ges. Wiss. Gött. Math.-Nat. Kl.* N.F., Bd. xii, **3**, 1927, 62 pp.) can best be summarised by listing the sectional headings: I. Allgemeines über "Gebirgszusammenhänge" und über "Leitlinien"; II. Die Begriffe "alpidisch" und "vorlandisch"; III. Das Pyrenäenproblem; IV. Die Struktur der Celtiberischen Ketten; V. Parallelen zwischen der mitteleuropäischen und der iberischen Vorlandstektonik; VI. Sonderstellung des Pyrenäengebietes in Bezug auf die vormesozoischen Faltungen; VII. Balearische Probleme.

In a series of four papers ("Uber die Tektonik der jüngeren Faltung in Ostfinnland," *Fennia*, **50**, 1928, No. 16, 22 pp.; "Sur un problème de la stratigraphie du Précambrien," *C. R. Soc. Géol. France*, No. 14, 1928, pp. 239–41; "Über alpine Tektonik und ihre Anwendung auf das Grundgebirge Finnlands," *C. R. Soc. Géol. de Finlande*, No. 1. 1929, 5 pp.; "Stereogramm des Gebietes von Soanlahte-Suistamo (Finland)," *Ibid.*, p. 9) Dr. C. E. Wegmann applies the principles of Alpine tectonics to the elucidation of the later (Pre-Cambrian) folding in East Finland. He has been able to trace a great arc of folding of Alpine type from Lake Ladoga by Carelia and the Oulujärvi region to southern Finland. This chain is the product of the deformation of an ancient pedestal, deeply eroded, and covered by three thick, sedimentary series, the Ladogian, Kalevian, and Jatulian. These are members of the same orogenic episode representing the different facies of the same formation. The Jatulian covers the foreland and has a marked continental facies; the Kalevian and Ladogian schists represent the geosynclinal facies of the mountain chain for which Wegmann proposes the name of Carelides.

Mr. E. B. Bailey's powerful Presidential Address to Section C (Geology) of the British Association at Glasgow (1928, 21 pp.) dealt with the Palæozoic Mountain Systems of Europe and America. He enlarges on the crossing of the Caledonian and Hercynian folds in the neighbourhood of South Wales, and on the similar crossing of the same systems of folds in the Appalachians west of New York. With Laurentia and its flat-lying eastern border of Palæozoic rocks, corresponding to Baltica with its similar cover of undisturbed sediments, North America represents, broadly speaking, a magnified mirror-image of much of Europe. Mr. Bailey concludes with the expression of the view that these concordances on opposite sides of the

Atlantic involve a recognition of some type of continental drift.

The paper by E. B. Bailey, L. W. Collet, and R. M. Field on "Palæozoic Submarine Landslips near Quebec City" (*Journ. Geol.*, xxxvi, 1928, pp. 577–614) is an attempt to interpret the peculiar conglomerates of the Ordovician in the neighbourhood of the great thrust-zone of Quebec (Logan's line) as submarine landslips in the earlier stages of the development and deformation of a geosyncline. "Logan's slope" served as a limit between different types of sedimentation, and as a locus for submarine landslips long before it was reached by thrust movements. The conglomerates have derived much of their materials from limestones very little older than themselves, and in the upward stratigraphical direction still younger fragments are met with, not older as is the case with normal conglomerates. The slope on which the conglomerates were formed was renewed by intermittent hinged subsidence, partly monoclinal, and partly of block-fault type. The interpretation of the conglomerates as submarine landslips is pointed by reference to the huge submarine landslip which was proved to have taken place after the great Japanese earthquake of 1923.

The Presidential Address to the Geological Society of America by Prof. A. Keith (*Bull. Geol. Soc. Amer.*, **39**, 1928, pp. 321–86) deals with "Structural Symmetry in North America," and is a thorough analysis of the mountain systems and structures in that continent. We cannot do better than quote his main conclusions: "I am convinced it is reasonable to accept the theory of thrust against the continent from all of the surrounding oceans, and also the doctrine that the continental shape and size have been roughly constant from the present day far back into the Pre-Cambrian. Around its margins were deposited in the ocean thick lenses of sediment; from the shallows of the ocean margin long, narrow geanticlines arose in response to thrust from the ocean. These geanticlines grew higher and higher, thrust was repeated, and finally culminated in the Appalachian Revolution at the E. and S., the Caledonian and Devonian deformations at the N., the Sierra Nevadan and Laramide revolutions at the W. and S. Then the geanticlines were united to the mainland and a period of relaxation and tension followed." Finally Prof. Keith gives an instructive time-table of Appalachian and Cordilleran tectonic events.

A posthumously published work by the late G. K. Gilbert deals with Studies of Basin Range Structures (*U.S. Geol. Surv.*, Prof. Paper, No. 153, 1928, 92 pp.). It represents the results of Dr. Gilbert's return to the field to defend his inter-

pretation of the parallel ranges of the Great Basin of North America bounded by the Wasatch and the Sierra Nevada, against the views of Spurr, who inferred erosion where Gilbert and others had inferred faulting. This incomplete series of memoirs shows all Gilbert's old power of lucid expression and penetrating analysis of geological structure.

BOTANY. By E. J. Salisbury, D.Sc., F.L.S., University College, London.

The vegetation of the Massif of St. Croix is the subject of a paper by S. Dziubaltowski (*Acta Bot. Soc. Pol.*). The region studied had an annual precipitation from 1922 to 1926 of from 630 to 843 mm. with a snow-free period of about six months. The soils are mixtures of sand and clay with about 0·5 per cent. of calcium. The climax community would appear to be forests of *Abies alba* with *Fagus sylvatica*; such forests and those dominated by *Abies alba* alone occupy the greater part of the area, but forests of *Pinus sylvestris* or pine, together with *Quercus robur* and *Q. sessiliflora*, also occur. In the pure stands of Abies the total carbonates ranged from 0·02 per cent. to 0·6 per cent. with a soil reaction of the surface of from pH. 3·6 to pH. 5. The most characteristic (exclusive) species, e.g. *Galium rotundifolium*, *Blechnum spicant*, and *Aspidium lonchitis*, have a low constancy. The most constant members of the herbaceous flora are *Oxalis acetosella*, *Maianthemum bifolium*, *Luzula pilosa*, *Vaccinium myrtillus*, and various ferns and mosses. In the mixed Fir and Beech forests the carbonates range from 0·01 to 0·6 per cent. and the pH. from 3·5 to 5·9. The ground flora is similar, but *Asperula odorata* and *Impatiens noli-tangere* are characteristic and commonly present. The *Pinus sylvestris* woods have similar soils with *Juniperus communis*, *Vaccinium myrtillus*, *Pteridium aquilinum*, *Sieglingia decumbens*, and *Calluna vulgaris* as common and characteristic species. The Pineto-Quercetum chiefly occupies southern slopes with the two species of *Quercus* showing similar soil preferences to those which they exhibit in this country. Thus *Q. sessiliflora* is found at the higher altitudes on the lighter, drier and poorer soils. These mixed woods are characterised by the presence of *Daphne mezereum*, *Lathyrus niger*, *Anemone nemorosa*, etc., and it is noteworthy that several species which in Britain are associated with calcareous soils occur in these siliceous habitats, *e.g.* *Sanicula europæa*, *Paris quadrifolia*, *Polygonatum officinale*, etc.

The succulence of plants receiving high nitrogenous fertilisation is attributed by Ewing and Pearsall to the fact that under these conditions, although amino-acids tend to accumulate, there is a reduction of Hydrogen-ion concentration. The effect of the high content of amino-acids and the reduction

in real acidity is to permit increased swelling of the protoplasmic colloids (*Ann. Bot.* Jan.).

Woodhead has recently given a valuable account of the history of the vegetation of the Southern Pennines (*Journ. Ecology*, February 1929), in which he emphasises the considerable area which was ice-free even at the period of maximum glaciation and probably supported a considerable Arctic flora. In the Arctic conditions obtaining till late glacial times the vegetation was tundra, which with Birch-heath probably persisted till about 8000 B.C. *Pinus sylvestris* invaded the birch-heaths about 7000 B.C., followed by Oak, Alder, and Hazel, forest development attaining a climax about 6000 B.C. during the warm and dry continental climatic conditions of the early Boreal period. The Pine disappeared between 6000 and 5000 B.C., and with the advent of the moist warm oceanic climate of the Atlantic period (4000–3000 B.C.) the forest on the summit plateaus degenerated and cotton grass moors dominated by *Eriophorum vaginatum* took its place. With the partial return to dry conditions in the Sub-Boreal (2000–1000 B.C.) the cotton grass moors were partially invaded by ericaceous vegetation, whilst the moist and cold climate of the Sub-Atlantic extending to the historical period was accompanied by the present vegetation differentiation and increasing human influence.

The Pennine peats mark the areas formerly occupied by forest, some of which have disappeared since Norman times. At present this peat is degenerating partly through the natural cutting back of streams, partly through artificial drainage and burning.

An interesting survey of the relation between nitrogen content of prairie and woodland soils to mean annual temperature has been made by Jenny (*Soil Sci.*, March 1929), who finds that for the semi-humid, humid, and semi-arid soils alike, in the U.S.A. the average total nitrogen content exhibits an exponential decrease with the increase of temperature. In Louisiana (semi-humid region) with a mean annual temperature of 20° C. the total nitrogen amounted to 0·05 per cent., whilst in North Dakota with an average of 4°·4 C. the nitrogen in virgin soils amounted to 0·295 per cent.

In the same journal Bartholomew and Janssen report the results of experiments with various crop plants grown in culture solutions containing a constant nutrient supply including Calcium and Magnesium, but with very varying amounts of Potassium. Despite the very wide range of basic ratios involved it is noteworthy that the dry weights obtained show no correlation with this feature. Thus in the case of oats the growth with two parts per million of Potassium yielded

optimum results, but with 0·5 and 1·0, or with 25 and 50 p.p.m., the amount of growth was practically the same. Of the six species experimented upon only cotton showed any approach to a regular relation, although the concentration of Potassium at which the best growth occurred was markedly different for the various species.

H. C. Joseph (*Bot. Gaz.*, February 1929) has obtained some interesting results from experiments on the germination of Birch seeds. Freshly harvested seeds of *Betula lenta* even when subjected to the most favourable temperature, viz. 32° C., did not exceed 17 per cent., and was in some cases as low as 2 per cent. Seeds sown after one month of dry storage, however, yielded from 11 to 41 per cent. germination at 32° C., whilst with temperatures alternating between 15° and 32° C. from 39 to 48 per cent. was obtained. On an acid substratum (pH. 4·6) slightly improved germination was indicated, but moist storage for from four to ten weeks resulted in a much higher percentage germination, whilst an even more interesting feature was the reduction of the minimum temperature at which germination took place. Over eighty per cent. germination was obtained at 15° C. with seeds which had been after-ripened under moist conditions for four weeks, and after five to six months' germination will take place at 0° C. These results are confirmatory of those obtained by Weiss for *Betula papulifolia* (*Am. Jour. Bot.*, 737, 1926). In the same journal Joseph describes experiments with Parsnip seeds, which freshly harvested showed from 70 to 94 per cent. germination in twenty-one days at 15° C. About 60 per cent. of the seeds lose their vitality during the first three years, but artificially dried seeds kept in dry air may completely retain their vitality.

A. Zamelis (*Acta Horti, Bot. Univ. Latviensis*) has studied the andrœcium in a number of flowers of *Pyrola uniflora*, and finds that both diplostemenous and obdiplostemenous flowers occur. The stamens are most commonly in groups opposite the petals, viz. three opposite each of the inner petals, one opposite each of the outer petals, and two opposite the petal of which one edge is internal and the other external.

Some doubt has been cast on the obligate nature of the parasitism of Entomophthoraceæ by the success which has attended the culture of *Entomophora sphærosperma* and *Empusa* by Sawyer on artificial media prepared from potato, pork, or fish. The presence of proteins in the culture medium is apparently essential, and these are broken down by the fungi with the production of acids, ammonia, and nitrites The optimum reaction appears to be p.H. 6·5, the optimum temperature 21° C. and the maximum 34° C. (*Amer. Jour. Bot.*, pp. 87–121, 1929). The five genera of the family present an

interesting series from specialised types such as *Completoria complens* confined to fern prothalli, and *Massospora cicadina,* which infects the Locust *Tibicina septemdecim* and presumably passes over sixteen years of subterranean existence in its host, to definitely saprophytic types, easily cultivated on artificial media, such as *Delacroixia coronata* found on decaying gills of mushroom and *Conidiobolus villosus* found on decaying wood. Consideration of the family as a whole both with respect to their natural occurrence and their behaviour on artificial media lends considerable support to the origin of the parasitic habit from saprophytism.

Rayner (*Ann. Bot.*, January 1929) finds that fungal mycelium is present throughout *Vaccinium oxycoccus.* Up to 10 per cent. germination was obtained with untreated and 40 per cent. with etherised seeds. The best germination takes place in spring and early summer following collection.

An account of the genus *Sphæroplea* is given by Fritch in the *Ann. Bot.* for January. Five species are recognised. *S. annulina* has a wide distribution, and in this species the chloroplast takes the form of transverse rings. *S. wilmani* has similar chloroplasts, but has a more restricted distribution. In *S. africana* the chloroplast is a diffuse reticulum, and in this species alone the septa are incomplete. Two new species are distinguished. There are *S. tenuis* from Griqualand West with band-like chloroplasts, and in which both types of gamete are liberated so that the oospores are not formed within the oogonium as in other species. *S. cambrica* from North Wales has wing-like ridges on the oospores similar to those of *S. africana,* but the chloroplast structure is as yet unknown.

From the observations of Howland, published in the same Journal, it appears that *Trentepholia aurea* can withstand six months' desiccation, and the conclusion is arrived at that the water is mainly retained by the walls of the cells and in the capillary lacunæ between the algal threads. The maximum resistance to plasmolysis is attained in the spring and summer.

ENTOMOLOGY. By H. F. Barnes, B.A., Ph.D., Rothamsted Experimental Station, Harpenden.

General Entomology.—During the past few months several outstanding books have appeared and are noticed elsewhere in this issue. Besides these, F. Balfour Browne (*Insects, an Introduction to Entomology,* 80 pp., 1928, *Benn's Sixpenny Library,* No. 45) has written a very stimulating short account of insects. He attempts to bridge the gaps between structure, habits and relationship to man that are usually to be found in elementary books on entomology, this he does most admirably. E. Handschin has produced a good elementary textbook on

the morphology of insects (*Praktische Einfuhrung in die Morphologie der Insekten*. Berlin: Gebrüder Borntraeger, 1928. Pp. viii + 112 + atlas of 23 plates). The atlas, which is held in a pocket at the end of the book, is characterised by the clarity of the drawings, most of which are original. D. H. Robinson and S. G. Jary (*Agricultural Entomology*, Duckworth, 1929, xi + 314, with 149 figures) have compiled an elementary book on agricultural entomology suited for agricultural diploma students.

Of recent years it has been rather the vogue, at any rate among the younger generation, to belittle taxonomy and systematics in favour of experimental, ecological, and genetical studies. By such the description of species is often considered to be the essence of systematic entomology, in reality it is but a small part. Hitherto the information available regarding the true meaning of systematics has been negligible. G. F. Ferris has stepped into the breach with *The Principles of Systematic Entomology* (*Stanford University Publ. Biol.*, **5**, 1928, 169 pp., 11 figs.). The whole subject is discussed very convincingly in twelve chapters, which are: (1) The contribution of the systematist to biology; (2) the scope of systematic biology; (3) the principles of systematic entomology; (4) the segregation of species; (5) categories less than the species; (6) the morphological basis of systematic entomology; (7) the preparation of material; (8) entomological drafting; (9) the description of species; (10) classification; (11) nomenclature, and (12) the training of the systematist. All entomologists should read this important contribution to the subject. G. C. Crampton (*Canad. Ent.*, **60**, 1928, 284–301) has concluded his study on the evolution of the head region in lower arthropods and its bearing upon the origin and relationships of the arthropodan groups.

Metabolism in insects is attracting more attention and several review papers on this subject are now available. P. S. Welch (*Ann. Ent. Soc. America*, **21**, 1928, 476–88) has surveyed the present state of knowledge, while Miss M. H. Sayle (*Qtr. Review Biol.*, **3**, 1928, 542–53) has reviewed the field of metabolic experiments on insects. B. P. Uvarov has done a very valuable piece of work (*Trans. Ent. Soc. London*, **76**, 1928, 255–343) in summarising the literature concerning insect nutrition and metabolism. This paper deals with their food, the chemical composition of insects and their products, enzymes, metabolism, the influence of diet on growth and reproduction, and the bibliography which is brought to date to the end of 1927.

The relative value of parasites and predators in the biological control of insect pests is discussed by W. R. Thompson

(*Bull. Ent. Res.*, **19**, 1929, 343–50). He points out that predators are to be found in twice as many families as parasites and his theoretical conclusions seem to indicate that the value of predators has been underestimated and that they are worthy of more careful attention in the field. The biological control of cotton pests is reviewed by J. G. Myers (*Empire Cotton Growing Review*, **5**, 1928, separate 15 pp.). An interesting list of arthropods associated with coniferous leaders weeviled by *Pissodes strobi* (Peck) is presented by R. L. Taylor (*Psyche*, **35**, 1928, 217–25).

F. Zweilgelt (*Ent. Anz.*, Vienna, **8**, 1928, 93–4, 99–100, 107–14) has dealt with the effect on development of insect life of external conditions such as climate, temperature, etc. He has proved, for example, that climate is the positive and the soil the negative factor in the distribution of *Melolontha* beetles. W. Robinson (*Jl. Agr. Res.*, **37**, 1928, 743–8) has shown that when insects are pierced, cut or injured, an effect is produced on them that may be analogous to surgical shock in higher animals.

M. J. Godfrey (*Jl. Botany*, 1925, 33, and 1927, 350) showed how an Algerian orchid, genus *Ophrys*, was fertilised by a Scoliid wasp, which is deceived by the plant and performs operations of a copulatory nature on the flowers. Now, Mrs. E. Coleman and A. M. Lea (*Trans. Ent. Soc. London*, **76**, 1929, 533–9) have made observations on the Ichneumon, *Lissopimpla semipunctata*, whose male enters the flower of an Australian orchid. Having mated with the flower even so far as ejecting seminal fluid, the Ichneumon leaves, carrying off the pollen on the tip of its abdomen. It is suggested that probably the deception is due primarily to scent as the Ichneumons are attracted from some distance, and that after the initial deception the coloration and shape of the labellum may serve as a confirmation to the insect.

Orthoptera.—Locusts and Grasshoppers, a handbook for their study and control by B. P. Uvarov (viii + 352 pp., 118 figs., ix plates, 1928, Imperial Bureau of Entomology, London) is an excellent work containing accounts of the external morphology, anatomy and physiology, development and transformations, behaviour, ecology and distribution, natural enemies, periodicity of mass outbreaks, and the technique and organisation of control of this group of insects. In the special part (Chapters 10–18), which did not appear in the Russian edition published some time ago, the author deals with the grasshopper and locust problems patent to different countries. There is a good bibliography and an index to species.

R. K. Nabours (*Ent. News*, **40**, 1929, 14–16) gives a very brief statement as to the results of the late Dr. Hancock's

studies of inheritance in green and pink Katy-Dids. The main conclusion to be drawn is that the green and pink of *Amblycorypha oblongifolia* compose a pair of Mendelian characters with the pink colour dominant.

J. H. Bodine (*Biol. Bull.*, **55**, 1928, 395–403), after experimenting with the rates of oxygen consumption and the blood pH changes in grasshoppers under normal and anaërobic conditions, finds that during oxygen lack an oxygen debt is built up which is repaid, in addition to an increased rate of oxygen consumption, when the insects are re-admitted to oxygen. During anaërobiosis blood pH falls, returning slowly to normal upon recovery. He suggests that the chemical change responsible for the anæsthetic condition accompanying anaërobiosis is the production of an excess of acid, carbonic and lactic, and that recovery consists in their elimination.

Coleoptera.—In *Forest Products Research Bull.*, **2**, 1928, pp. 1–46, 27 figs., R. C. Fisher deals fully with *Lyctus* Powder-Post beetles. The increase and spread of these insects is shown to be due to the importation of infested American ash and oak. These beetles lay their eggs inside the vessels of the wood, never on the surface. S. H. Clark has found a definite correlation between the size of the vessels and the liability of different woods to *Lyctus* damage. Certain hard woods such as beech and birch which have small vessels have not been observed to be attacked. Miss N. M. Payne (*Biol. Bull.*, **55**, 1928, 163–79) has studied cold hardiness in the Japanese Beetle, attention being paid to cold hardiness due to both intensity and duration of cold. One important result is that she finds development of cold hardiness to the quantity factor of low temperature is associated with loss of cold hardiness to the intensity factor except in extremely dehydrated individuals.

Among the biological studies of beetles that have appeared, J. N. Oldham, in a paper (*Ann. Appl. Biol.*, **15**, 1928, 679–97) on the metamorphosis and biology of *Rhynchænus* (*Orchestes*) *alni*, gives a detailed description of the external morphology of the full-grown larva and of the pupa, and also a list of hymenopterous parasites, of which eight distinct species were reared. F. R. Petherbridge (*Ann. Appl. Biol.*, **15**, 1928, 659–78) describes the larval, pupal, and adult stages of the turnip mud beetles (*Helophorus rugosus* Ol. and *H. porculus* Bedel), and gives notes on their biologies.

Lepidoptera.—R. A. Fisher and E. B. Ford, in a study of the variability of species of Lepidoptera, with reference to abundance and sex (*Trans. Ent. Soc. London*, **76**, 1929, 367–79), find that mean variance in colour in both sexes is greatest in the abundant species and least in the less common ones. There are indications also that the species with wider range

are, in any one locality, the more variable. But the mean variance is considerably greater in females than in males, and it is suggested that the male hormones may inhibit the action of a number of the factors influencing the development of pigment. Goldschmidt's alternative suggestion that there exist pigmentation factors in the Y chromosome capable of interaction with autosomal factors to cause pigmentary differentiation may account for a few cases.

The digestive enzymes of both larvæ and the adult of the Oriental Fruit moth are secreted by the cells of the mid gut according to H. S. Swingle (*Ann. Ent. Soc. America*, **21**, 1928, 467–75). While invertase, lipase, trypsin, and erepsin were found in the digestive tract of the larva, only invertase was found in the adult. W. M. Davies (*Bull. Ent. Res.*, **19**, 1928, 267–70) deals with the feeding habits of *Habrosyne derasa L.* and shows how it can develop in captivity on a number of different plants. R. Pussard (*Ann. des Épiphyties*, **14**, 1928, 107–31) has studied the morphology and biology of *Gracilaria syringella Fab.* which attacks lilac.

An important paper on the systematics of the Formosan Pyralidæ has appeared by J. Shibuya (*Jl. Fac. Agric. Hokkaide Imperial Univ.*, **22**, 1928, 300 pp., 9 plates). For a well-illustrated account of the Saturniid moths of the East Indies one should refer to E. L. Bouvier's recent paper (*Bull. Hill Museum*, **2**, 1928, 122–41, 6 plates); descriptions of new species and varieties are included.

Hemiptera.—J. Davidson's work on the factors controlling migration and reproduction of *Aphis rumicis* has now been concluded. An important paper summarising his results has appeared (*Ann. Appl. Biol.*, **16**, 1929, 104–34). The normal sequence of the generations in the complete life-cycle and the occurrence of the parthenogenetic and sexual phases are first studied. In seeing whether the normal sequence could be experimentally affected by changing the environments and also the influence of such changes on the occurrence of alatæ and apteræ, he found that with suitable conditions the parthenogenetic alienicolæ generations may be maintained experimentally for long periods and the sexual phase may be induced or suppressed by the factors of light (length of day) and temperature.

P. Marchal (*Ann. des Épiphyties*, **14**, 1928, 1–106) gives a very full account, morphological and biological, of the woolly apple aphid, and in addition a very interesting section on immunity and resistance of host plant to attack.

C. J. George (*Q.J.M.S.*, **72**, 1928, 447–85) describes the morphology and development of the genitalia and genital ducts of *Philænus*, an Homopteron, and *Agrion*, a Zygopteron.

F. V. Theobald's monograph on the Aphididæ of Great Britain has been completed by the publication of *The Plant Lice or Aphididæ of Great Britain*, Vol. III. (vi + 364 pp., 213 figs., 1929). Headley Brothers, Ashford and London). The subtribes Chaitophorina, Pterocommina, and Saltusaphidina; the tribes Vacunini and Lachnini; and the subfamilies Eriosomatinæ, Hormaphidinæ, and Mindarinæ are dealt with in this volume. There is also included a note on some observations on the life-cycle of *Periphyllus testudinatus* Thornton and *P. aceris* Koch by A. W. Rymer Roberts (pp. 355–59).

Hymenoptera.—An important contribution to the biology of entomophagous Chalcids by H. L. Parker and W. R. Thompson has been published (*Ann. Soc. Ent. France*, **97**, 1928, 425–65).

R. N. Chrystal (*Bull. Ent. Res.*, **19**, 1928, 219–47) deals with the biology and forest relations of certain wood-wasps. A complete review of the classification and status of the Siricid wood-wasps occurring in Great Britain is given in addition. It is interesting to read that *Sirex cyaneus* cannot be considered a primary enemy of healthy green trees, for instance, at Tubney unsuitable soil conditions were the predisposing factor, while in N. Devon it was a root fungus, *Fomes annosus*. J. G. Myers (*loc. cit.*, 317–23) gives further biological notes on *Rhyssa* and *Ibalia* parasitising *Sirex cyaneus*.

W. M. Wheeler (*Jl. Expt. Zool.*, **50**, 1928, 165–237) summarises the present state of knowledge of *Mermis* parasitism and intercastes among ants. The *Histoire des Fourmis* (116 pp., 1928, Lechevalier, Paris), by de Réamur, which hitherto has lain as an unpublished manuscript in the Archives of the Academy of Sciences of Paris, has now been published as Vol. XI of the *Encyclopédie Entomologique* with an introduction by E. L. Bouvier and notes by C. Perez. P. Rau (*Psyche*, 35, 1928, 153–6) gives an acccount of trophallaxis in *Polistes pallipes*.

The third of G. D. Morrison's papers (*Q.J.M.S.*, **72**, 1928, 511–26) on the muscles of the adult honey bee deals with the chemistry of the muscles, appearance of muscle-fibres, under polarised light, effect of age and fatigue on muscle, which, however, could not be determined by the techniques employed, and theories of muscular contraction. D. M. T. Morland (*Jl. Ministr. Agric.*, 1929, 45–50), in discussing the feeding of bees, shows that there is little or no foundation at present for the prejudice against beet sugar feeding. No. **19** of *Faune de France* (1928, 205 pp., 232 figs.), by L. Berland, concerns the true wasps, together with certain related groups commonly united to form the family Bethylidæ.

Diptera.—No. **18** of *Faune de France* (1928, 174 pp. 275 figs.) by M. Goetghebuer, deals with the tribe Chironomariæ of the family Chironomidæ. H. G. Dyer (*Proc. Ent. Soc. Washington*, **30**, 1928, 110–12) gives a list of the water-bearing plants of Panama which harbour mosquitoes together with the species that inhabit them. E. Gabritschevsky (*Bull. Soc. Ent. France*, 1928, 75–9) gives a preliminary account of some experiments on the determination and reversion of the polymorphic larval characters of *Miastor metroloas*. Three forms of larvæ may arise from the pædogenetic mother-larvæ, (*a*) white larvæ with small eyes and white adipose tissue, (*b*) orange larvæ with large eyes and orange adipose tissue, and (*c*) white larvæ, characterised by presence of an anchor-process, which develop into adults. The factors controlling the appearance of these forms have been unknown till now. It is claimed that the artificial media used by Harris in his recent experiments give the same results as using pure water, there being no real food present. Gabritschevsky finds that, with a plentiful supply of food, constant humidity, and low temperature (5–15° C.), permanent reproduction by pædogenesis results. Dryness causes white mother-larvæ to become yellow and also causes larvæ inside the mother to change into the orange type. Starvation after birth causes young white larvæ to change into the orange type quicker in a dry atmosphere than one in which the humidity was maintained. If the pædogenetic white larvæ kept at a low temperature are suddenly moved to a temperature of 30° C. with abundant food supply, he finds that white larvæ of the third type which possess an anchor-process are produced very quickly. Thus he shows that the appearance of the sexual cycle is suppressed by low temperature. The orange larvæ are adapted to submersion by water, dryness, the action of acids, etc., and are also a migratory form.

There has recently been published an important contribution to our knowledge of island fauna in the shape of a small volume by the late Mario Bezzi (*Diptera Brachycera and Atherica of the Fiji Islands: based on material in the British Museum* [*Nat. Hist.*], pp. viii + 220. London: British Museum [Nat. Hist.], 1928). The volume consists in the main of technical descriptions of new species, but the author also discusses the affinities of the fauna, coming to the conclusion that the Fijian diptera are typically endemic of Austro-Malayan origin, with a small number of imported elements. In vol. 9 of the *Encyclopédie Entomologique* (*Mouches parasites* 1, *Conopides, Oestrides et Calliphorines de l'Europe occidentale*, 251 pp., 300 figs., Lechevalier, Paris, 1928), E. Séguy deals with the systematics of the Conopidæ in the first part, while

in the second, which is more general, he discusses the morphology and biology of the Oestridæ and Calliphorinæ. The structure of the head and mouth parts in the Nycteribiidæ is the subject of a paper by B. Jobling (*Parasitology*, **20**, 1928, 254–72).

W. W. Alpatov and R. Pearl (*Amer. Nat.*, 63, 1929, 37–67) have shown that *Drosophila* reared at 18° C. is distinctly larger than when reared at 28° C., and that the females always live longer than the males, although, as the temperature in imaginal life increases, regardless of the temperature during development, the difference in length of life between the sexes decreases. Also the relative influence of temperature upon body size and duration of life was the same order of magnitude, thus confirming the theory that the rate of energy expenditure during life is an important factor in determining the duration of life.

D. F. Miller (*Jl. Expt. Zool.*, **52**, 1929, 293–313), in a study of the effect of temperature on locomotion of fly larvæ, has found that the rate of locomotion varies directly as the temperature and that the rate of contraction increases directly with temperature. He used the larvæ of *Lucilia serricata* Mg. J. G. H. Frew (*British Jl. Expt. Biol.*, **6**, 1928, 1–11) describes methods for the cultivation of tissues of the Blow Fly larvæ and concludes that a successful technique must depend on the rearing of bacteria free larvæ for which a method is briefly described. A. Moutia (*Bull. Ent. Res.*, **19**, 1928, 211–6) has shown that *Stomoxys nigra* is the principal vector of surra in Mauritius. A closely allied species, *S. calcitrans*, had previously been proved to be a vector in other countries.

Other Orders.—J. V. Pearman's biological observations on British Psocoptera have now concluded (*Ent. Mo. Mag.*, **64**, 1928, 241–3, 263–8). These sections deal with hatching, ecdysis, sex behaviour, and miscellaneous observations.

C. E. Abbott (*Psyche*, **35**, 1928, 182–5) gives a short account of some experiments on the nervous physiology of dragon fly larvæ.

The Thysanoptera of India is the subject of a paper by T. V. Ramakrishma Ayyar (*Mem. Dept. Agric. India*, **10**, 1928, 216–316).

W. M. Davies (*British Jl. Expt. Biol.*, **6**, 1928, 79–86) has found, from some experiments on the effect of variation in relative humidity on various species, that Collembola with no tracheal system are very susceptible to dry conditions, whereas *Sminthuris viridis*, which possesses a tracheal system and a well-developed ventral tube, is much less susceptible. It was also suggested that the ventral tube is used for conveying

droplets of water from the hairs of the body to the mouth as well as being a "cleaning" organ.

PREHISTORIC ARCHÆOLOGY. By L. J. P. Gaskin, Librarian to the Royal Anthropological Institute, London.

Journal of the Royal Anthropological Institute, Vol. 58, Part 2. Father Gardner writes on excavations in a Wilton Industry at Gokomere, Fort Victoria, Southern Rhodesia. The implements found include scrapers, crescents, saws, knives, borers, beads, arrow-points, bone, and ivory spikes. The Wilton Culture is usually associated with the Middle Palæolithic Period (comparable to the Mousterian of Europe), and is chiefly remarkable for the fact that it is almost always composed of pygmy tools. The article is well illustrated.

Antiquaries' Journal, April 1929. Miss Chitty writes on Twin-Food-vessels preserved at Aqualate Hall, Staffordshire. Miss Chitty points out that these vessels have been wrongly listed as "Roman," and, in reality, belong to the second phase of the Early Bronze Age (*circa* 1700–1400 B.C.), corresponding to Abercromby's type, Ia, which is nearly always associated with unburnt burials. It is assumed that the pots were originally formed separately and joined together while the clay was still pliant. There is an excellent illustration of the vessel.

In a well-illustrated article on a Chellean Hand-axe from the Cromer Forest Bed, Mr. J. Reid Moir states that the implement, which is devoid of cortex, has a butt-end provided with an edge for chopping at right angles to the upper blade-like portion. Mr. Moir thinks that this feature is connected with the form of rostro-carinate implements from which the Chellean Hand-axes were evolved.

Mr. S. E. Winbolt reports on a late Pleistocene Flint Point found in October, 1928, in Peper Harow Park, Godalming. Mr. Reginald Smith and Mr. J. Reid Moir assign to it a proto-Solutré origin. This discovery serves as one more example of the presence of Upper Palæolithic Implements in the open in England.

Bulletin de la Société préhistorique Française, February 1929. M. Ferdinand Ydier writes on the Neolithic Pottery of the Sables-d'Olonne (Vendée). More than sixty pieces of fragments of decorated pottery were found at the site of "Bar des Roches." M. Ydier makes a careful examination of some typical specimens (of which he gives illustrations), and comes to the conclusion that they belong to the last period of the Neolithic Era.

M. André Renard makes a detailed survey of the Prehistory of the valleys of l'Estrigueil, l'Esves, and La Ligoire

(Indre-et-Loire). Objects from the Palæolithic and Neolithic Age were discovered. Of particular interest are the *Coups-de-poings* from Mareuil, La Brissonière, Fosse Laureste, and Saint-Senoch.

Památky Archeologické, Tome 35. Dr. A. Stocký contributes a well-illustrated article on the pottery of the *Aunjetitz* Culture in Bohemia. This Culture, named after the great cemetery at Prague, belongs to the original Bronze Age Civilisation of Central Europe. The pottery shows affinities with the jugs of the bell-beaker period, and the old Danubian forms, such as the tripod bowl. Prof. V. Gordon Childe regards the pottery as a direct outcome of the preceding epoch—the *Marschwitz* Culture—and cites as example the typical *Aunjetitz* mug which is just a *Marschwitz* pouched jug made angular.

In the *Illustrated London News* of March 2, Baron von Miske and Dr. von Bandat describe a prehistoric Iron Mine and associated cultural remains at Velem St. Vid, Hungary. They date the mine as belonging to the early Hallstatt period, and give it as their opinion that Velem was the first place in Europe in which Iron was used. The mine, which was sunk to a depth of 200 feet, is associated with bronze and iron furnaces, and with a large number of broken bronze objects of Italian, Silesian, Swiss, and Scandinavian origin, showing that changes from the use of copper to iron were effected here on a large scale. The article is exceptionally well illustrated, and the plates of the Silesian implements and the bronze sword are particularly attractive.

Wiener Prähistorische Zeitschrift, Vol. 16, Part 1. Herr Julius Caspart describes the finds from a Prehistoric Cemetery at Neunkirchen am Steinfeld (Austria). The most interesting objects came from Grave 1, bronze fibula ; Grave 2, iron sword, chain, lance-head, and vessel ; Grave 7, an iron knife. Herr Caspart dates the culture as the middle of the La Tène period. The illustrations to this article are particularly good. Herr Heinrich K. Michna reports on a prehistoric settlement near Mödling, Vienna. Objects from the Neolithic, Bronze and Iron Ages were found. Herr Michna states that the settlement was founded in the late Neolithic period.

Archæologica Hungarica, Tome 4. Dr. J. Hillebrand contributes a long and well-illustrated article on an Early Copper Age Cemetery at Pusztaistvánháza, Hungary. Examination of the graves proved that orientation was practised, and pottery, copper swords and daggers, and pearls were among the objects found associated with the burials. Dr. Hillebrand correlates the culture with that of Bodrogkeresztúr, which he classifies as Early Copper Age I. The culture is remarkable

for the prevalence of the " Milchtopf " type of pottery, and the primitive type of square copper dagger.

Antiquity, March, 1929. Contains a very interesting article on the Magic Origin of Prehistoric Art. The author, Count Bégouen, considers that " Art was born of Magic," and in support of this theory advances the following assertions. The general inaccessibility of prehistoric drawings, and, in particular, the famous clay bison of Tuc d'Audoubert (758 yards from the entrance of the cave), the consequent darkness, and, therefore, the probability of some magic ritual accompanying the act of drawing. The fact that the drawings were made in parts of the caves not used for habitation, which suggests some sort of shrine ; that many animal figures are represented as pierced by darts at vulnerable points (such as those at Font-de-Gaume), and that there is a direct connection between this practice and that in use by the pygmies of Central Africa to-day, who use a magic ritual of piercing the representations of animals before entering upon the chase ; that at Cabrerets and elsewhere many animal figures have been drawn which lack both eyes and ears, purposely drawn thus for magical reasons, to deprive animals of their two essential senses and so place them at the mercy of Man. Finally Count Bégouen asserts that when Man passed from the Hunting to the Agricultural phase of existence he forgot Art. There are many arguments which might be advanced to controvert Count Bégouen's theories, and a new book by Prof. Baldwin Brown, entitled *The Art of the Cave-Dweller*, in which very different views are held, is worthy of consultation.

Dr. Wheeler reports on the recent excavations at Brentford, where a discovery was made of a Romano-British pile-dwelling, the first of its kind recorded in the British Isles. Dr. Wheeler points out the similarity existing between this pile-dwelling and the lake-village of Glastonbury. The pottery has been classified as belonging to the Hallstatt period (*circa* 1000–500 B.C.). The article is well illustrated and contains a schedule of the finds.

Proceedings of the Prehistoric Society of East Anglia, Vol. 5, Part 3. In her presidential address, Miss Garrod reviews the present position of research in Prehistory. She points out that since research is spreading far beyond the confines of Europe the sequence of culture periods from Chellean to Magdalenian cannot be applied in entirety to all regions. In relation to the Palæolithic Cultures of Europe she suggests that the Upper Palæolithic is divided into two branches—the Capsian and the Aurignacian, regarded as two offshoots from a common stock as yet unknown, and that Palestine is the centre of dispersal. Similarly, while one branch of the Aurignacian fused

with the Solutrean of Central Europe, the other developed into the Magdalenian. The recently discovered Palæolithic Culture of China may be explained (the centre of dispersal being Central Asia) if it is regarded as related to the West by descent from a common stock. Miss Garrod concludes her article with a plea for extended exploration in extra-European areas. Mr. J. P. T. Burchell reports on the final investigations carried out at Lower Halstow, Kent. Finds in the following cultures were discovered : Palæolithic, an early Mousterian Hand-axe ; Epi-Palæolithic, an Epi-Palæolithic factory site, represented by many implements ; Neolithic, arrow-heads ; Early Iron Age, a piece of decorated pottery ; Romano-British Period, broken pottery fragments and a bronze phalera. The illustrations which accompany the article are excellent.

Man, February 1929. Mr. C. E. Vulliamy reports on an unrecorded Long Barrow in Wales. The Barrow is situated on ground belonging to Little Lodge Farm, Glasbury, on the Breconshire border. It is built of slabs and blocks of red sandstone with a certain proportion of small glacial and water-worn boulders. The chambers are of Megalithic type. Excavation produced the remains of five adult males, an old woman, and two children. Mr. Vulliamy gives it as his opinion that the burial can be ascribed to the close of the Megalithic Period.

Man, April 1929. Messrs. Sandford and Arkell write on the relation of Palæolithic Man to the History and Geology of the Nile Valley in Egypt. The authors survey in detail the four eras, (1) Oligocene and Miocene, (2) Pliocene, (3) Plio-Pleistocene, and (4) Pleistocene. In 4 (*a*) Middle Palæolithic implements have been found *in situ* in Upper Egypt in a series of four river terrace-gravels, as follows :

100 feet above the Nile .	Chellean.
50 " " " .	Acheulean and Micoque.
30 " " " .	Early Mousterian.
10 " " " .	Mousterian.

In 4 (*b*) the Later Palæolithic (post-Mousterian) history in Egypt is dealt with, and in 4 (*c*) the transition to Neolithic times.

Ancient Egypt, 1928, Part 3. Miss G. Caton-Thompson writes on the Neolithic Pottery of the Fayum. Miss Thompson points out that all the pottery is hand-made of coarse clay ; that unequal and insufficient firing has produced grey mottling on red pots, the core being black and soft, and that few pots retain their original surface. The forms of the pottery are grouped into five classes : (1) small bowls and cups ; (2) cooking bowls or pots ; (3) pedestal cups ; (4) cups with leg bases ; (5) rectangular dishes with peaked rims.

L'Anthropologie, Tome 38, Parts 5–6. MM. C. Gaillard, J. Pisset, and C. Côte contribute a well-illustrated article on the prehistoric rock-shelters of Sault and Trosset at Serrières-sur-Ain. At Trosset three levels (marked A, B, and C) were distinguished. A was rich in Neolithic pottery; B in Neolithic and Microlithic Implements; C in flint implements of older origin than B. Similarly at Sault three levels were distinguished. A contained a few Neolithic flint implements, B a sparse quantity of better formed implements, and C a large quantity of Microlithic implements.

Marthe and St. Just Péquard report on a Mesolithic site on the Isle of Téviec, Brittany. This is the first discovery of the Mesolithic culture in Brittany. The kitchen-midden deposits were rich in finds; amongst the most interesting were those of reindeer horn, bone weapons, and a shell necklace. Double interment was apparently practised. The article is well illustrated.

M. Coulonges describes the prehistoric site of Martinet, Sauveterre-la-Lémance (Lot-et-Garonne). In the course of excavation, finds from the following cultures were made. Layer 1, Upper Magdalenian, flint implements; Layer 2, Sauveterrien, Microlithic flint implements; Layer 3, Tardenoisian implements; Layer 4, Robenhausen, a carved stone.

Revue Anthropologique, January–March 1929. M. Poisson continues his article on the Neolithic and Eneolithic Cultures of France. In this, the concluding portion of his article, M. Poisson deals with Western influences: the advent of decorated pottery; the origin and diffusion of Brachycephalic man, and the traces of his route up the Danube to the Rhine and incursion into the North, and later the South of France and Spain. The author recognises a Copper Age in France, and traces, in the early Bronze Age Cultures, affinities with the Aunjetitz Culture of Bohemia, the original Bronze civilisation of Central Europe. This is an interesting and valuable survey, but it has suffered much from lack of illustration.

Africa, April 1929. Mr. A. J. H. Goodwin writes on the Stone Ages in South Africa. Roughly the Stone Ages of South Africa may be divided into divisions known as Early, Middle, and Late. The Early Stone Age comprises the local Cultures known as the *Stellenbosch*, *Fauresmith*, and *Victoria West* industries, comparable to our Lower Palæolithic Cultures; the Middle Stone Age comprises the local Culture known as the *Still Bay* industry, corresponding to the Mousterian of Europe; the Later Stone Age comprises the local Cultures known as the *Smithfield* and *Wilton* industries, which correspond to the *Upper Capsian* Culture. In view of the fact that the British Association for the Advancement of Science are to meet in

South Africa this summer, the Prehistory of South Africa assumes considerable interest for all. A valuable survey of the subject is to be found in Mr. M. C. Burkitt's recent book *South Africa's Past in Stone and Paint.*

The Times, February 26, March 16 and 19. Mr. Leonard Woolley discusses the recent work of excavation at Ur. The Royal Tomb was, as anticipated, discovered, but it had been rifled and the great treasures which it must have contained had disappeared. Some broken necklaces of lapis lazuli, gold beads, two silver lamps, and a broken sceptre were all that remained. Perhaps the most important discovery made in connection with the work at Ur this season was the evidence of a great Flood. In digging down through eight feet of water-laid clay a flat stratum was discovered rich in flint chips and cores, and painted pottery, similar to that of al 'Ubaid. Evidence of this civilisation buried beneath water-laid clay has been found at several points, some as much as 200 yards apart. Mr. Woolley points out that only a flood of great magnitude could have deposited an eight-foot bank of clay such as that overlying the original settlement of Ur. In a final letter to *The Times* Mr. Woolley discusses the fortifications of the city of Ur. The mud-brick wall is an amazing structure. It stood some 26 feet high, its outer face sloped backwards at an angle of 45°, and at its base it measured no less than 75 feet in thickness. In reality it served the purpose of an earth rampart along which ran the wall proper.

East African Archæological Expedition. In a letter to *The Times* of March 7, Mr. L. S. B. Leakey states that he has found traces of Mousterian Man in Kenya, a series of fine obsidian tools of undoubted Mousterian type, associated with the skeletal remains of a man, being found in Gamble's Cave.

Exhibition. An Archæological Exhibition was on view at University College, London, from February 19 to March 19. The objects shown consisted, for the greater part, of finds made in England, Scotland, Wales, and Ireland, during the last year. The age of the finds dated from Palæolithic times to the Mediæval era. Amongst other objects of great interest was the masked human figure engraved on reindeer rib (mentioned in Science Progress, April 1929) from Pin-hole Cave, Creswell; the bronze scabbard from Meare Heath, Somerset; and some of the objects from Skara Brae (Science Progress, January 1929).

ARTICLES

WHARTON'S AND DARWIN'S THEORIES OF CORAL REEFS

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THERE is a passage in Gardiner's review [1] of my book on coral reefs [2] which puzzles me. He there wrote: "It may be that Wharton's views . . . are 'against the consensus of geological opinion,' but I have failed to find any evidence cited against Wharton's idea of an island being cut down by the waves and currents to a flat bank 30–40 fathoms deep." Taken with the context, this can only mean that Gardiner regards Wharton's theory of atolls as still tenable; for if it be agreed that the foundations of atolls have actually been prepared by cutting down volcanic islands to flat submarine banks, as Wharton believed, the later upgrowth of the reefs around the bank margins to form atolls is acceptable enough. The puzzling part of the above quotation is therefore that Gardiner appears to "have failed to find any evidence" cogently cited against Wharton's theory; for, as I see the case, the counter-evidence is not only manifest and abundant but also unquestionable and convincing. In order to make this clear, let us look somewhat closely at the theory which Wharton proposed.

That experienced hydrographer was impressed, some thirty-five years ago, with the flatness of atoll-lagoon floors, in which he could not find the "deeply concave surface" of Darwin's text (1842, 93); but Wharton there misunderstood Darwin's meaning, for, although the originator of the subsidence theory of coral reefs also used other similar phrases, such as "saucer-shaped hollow" (28) and "bason-like form" (92) in connection with atoll lagoons, he elsewhere stated explicitly that "the greater part of the bottom in most lagoons is formed of sediment; large spaces have exactly the same depth" (20), thus showing that concavity is subordinate to flatness, and he very properly adduced the processes of aqueous deposition to account

[1] J. Stanley Gardiner, "The Coral Reef Problem," *Geog. Journ.*, lxxii, 1928, 268–71.

[2] *The Coral Reef Problem*, Amer. Geog. Society, New York, 1928.

for the flatness with which lagoon-floor sediments are spread out.

Wharton was also impressed with the moderate depth, 30 or 40 fathoms, of many lagoons, and asked whether that restricted depth "may not be the general limit of the power of oceanic waves to cut down the mass acted upon when it is fairly friable" (*Brit. Assoc. Rept.*, 1894, 709); but he nowhere showed that "fairly friable" rocks prevail in atoll foundations. Three years later he made a fuller statement of his views in an article entitled "The Foundations of Coral Atolls" (*Nature*, lv, 1897, 390–93); and here he wrote: "Toward the sides of a lagoon the depth gradually lessens"; hence the difference of opinion between Wharton and Darwin on this point would seem to have been rather verbal than factual. Darwin was considering chiefly the reef, and emphasised the slope by which descent is made from it to the flat lagoon floor; Wharton was concerned chiefly with the lagoon floor, and emphasised the general flatness of its surface, from which a gradual ascent is made to the encircling reef.

It is in Wharton's article of 1897 that some account is given of certain members of the group of drowned atolls north of Fiji, for which I have proposed the name, Darwin Hermatopelago, or Sea of Banks. Wharton there wrote: "The remarkable thing about these banks is the absolute uniformity of the depth of water over their areas inside the low [submerged] rim of growing coral which encircles their edges in various degrees." The Alexis bank is represented in a full-page figure with many soundings; its outer rim has diameters of 15 and 8 miles, with depths of from 13 to 20 fathoms; its floor measures 12 by 7 miles with depths of from 24 to 28 fathoms. Other banks are said to measure 22 by 10 and 18 by 9 miles with floors of similarly equable depths. But not enough emphasis is given to the fact that, by reason of the bank rims being submerged, the banks are swept over by the swell coming, not greatly impeded, from the open ocean; or to the inference that, by the action of the swell, the exceptional smoothness of the bank floors may be accounted for. It is then asked: "What causes this remarkable similarity of depth and this extraordinarily even surface over these large banks? Is it uniform subsidence of mounds of identical height over a great area? Is it building up of mounds to an identical distance below the surface? I cannot think that either can account for the conditions. I would venture to suggest the cutting down of volcanic islands by the action of the sea, and that this action has a far greater share in furnishing coral foundations than has generally been admitted."

The possibility of such a process is then discussed with especial reference to completely or almost completely truncated islands. Even the vast Seychelles bank in the western Indian Ocean, with an area of some 16,000 square miles and a general depth of 30 fathoms, is instanced as if in evidence of the competence of abrasion for the production of flat submarine platforms, in spite of the fact that the central granitic islands which surmount the Seychelles bank have non-cliffed and fairly well embayed shores. The conclusion is reached that the effect of abrasion " will be to cut down an island more or less rapidly, according to its construction, to a very considerable depth below the surface ; the final effect being a perfectly flat bank." The idea that barrier-reef lagoons can be levelled by sedimentation is rejected, thus implying that barrier reefs grow up from platforms of incomplete truncation.

The processes of Wharton's theory are easily conceivable. It is not to be denied for a moment that abrasion is highly competent to cut down a still-standing volcanic island, if it is not protected by a coral-reef breakwater ; but Wharton gave no reasons for the lack of such protection around islands in the coral seas while their abrasion was in progress. It is questionable, however, whether a volcanic island, when completely truncated, would be reduced to a " perfectly flat bank," for the surface of truncation should ordinarily be that of a very low cone, shallower at the centre than around the margin ; and a low cone of truncation could not gain a central depth of 40 fathoms unless the establishment of an atoll reef around its outer border was very long delayed.

But even if the processes of abrasion are accepted as competent to produce flat banks with central depths of 40 fathoms it remains to be learned whether such processes have actually operated in the production of atoll foundations. Wharton evidently concluded that they had so operated, but his articles give no indication that he secured independent confirmation for this conclusion by subjecting his theory to a critical analysis. Indeed, it does not appear that he had any acquaintance with the logical, thorough-going method of scientific investigation, by which independent verification for an invented theory can be reached. He seems to have been, like many of his contemporaries as will be shown below, satisfied with an incomplete, not to say archaic, " two-faculty " method of inquiry ; namely, a method that consists of hardly more than observation of facts to be explained and invention of an apparently competent theory to explain them. This over-simple method is no longer satisfying as a means of solving a geological problem. It is of course a recommendation for a geological theory that it can explain the facts which it was

invented to explain; it would not deserve a moment's consideration if it did not do that.

But it must be remembered here that what is, in geology, called an explanation, of coral reefs for example, is only an imagined series of events arranged in causal sequence, so that the later ones which we can see shall constitute a reasonable sequel to the earlier ones which we cannot see; and it must be at the same time remembered that what is called a geological theory is only a mental concept concerning the nature of the earlier, unseen events, from which the later, seen events shall follow. When such a theory is invented, its inventor of course hopes and perhaps believes that it is a true counterpart of the past events which have, with the passage of time, brought forth the present events; but, in order that the theory shall merit general acceptance as a veritable counterpart of the searched for, but unseen, past events, it must do more than merely explain the facts it was invented to explain; it must also explain various visible facts that it was not invented to explain, even facts that were not known when the theory was invented but which are later found to be closely associated with the facts that the theory was to explain. Wharton's theory does not meet this test.

Let it therefore be clearly recognised that in such a problem as the origin of coral reefs—as well as in various other geological problems, such as the origin of coal beds, of contorted strata in mountains, of foliated crystalline schists and of mountainous pelagic islands [1]—observation alone cannot safely reach the desired solution because a large number of essential facts, being lost in the past and therefore inaccessible to observation, can be recovered only by theorising; that is, by making inferences based on observation; and some independent evidence of the correctness of the inferences by which the unobservable past facts are recovered must be forthcoming before they deserve credence. In other words, geology is not a purely inductive science; it is very largely deductive. In order to secure independent evidence for the verification of geological inferences, two other mental faculties besides observation of visible

[1] Gardiner prefers "oceanic" to "pelagic" for the characterisation of open-ocean islands, because pelagic "is in general use for floating organisms and should be so employed according to the law of priority." Regarding this word the *New Oxford Dictionary* notes that, "used technically by naturalists, the term, Pelagic, *applied to living things* [the italics are mine], denotes those animals and plants which inhabit the surface waters of the seas and oceans"; but the primary definition of the word, given in earlier lines of the same dictionary, is: "of or pertaining to the open or high seas, as distinguished from the shallow water near the coast." Pelagic is, therefore, quite as properly applicable to inorganic objects, like open-ocean islands, as to organic objects, like open-ocean animals and plants.

facts and invention of explanatory theories must be employed; namely, critical deduction of all possible consequences from the invented theories, and impartial confrontation of these deduced consequences with appropriate facts of observation[1]; and in this "four-faculty" method Wharton seems to have been no more trained than were Rein, Semper, Murray and Guppy; for none of these often-quoted students of coral reefs carried his investigation so far as consciously and critically to deduce the consequences of the theory that he invented and impartially to confront the consequences with the facts.

Indeed, the "four-faculty" method does not seem to have been habitually employed by geologists in general through the last half of the nineteenth century and still less through the first half, familiar as it had then long been to logicians. In support of this perhaps hazardous statement the following instructive story may be told regarding the origin of the good-sized lakes which occupy the distal part of most of the larger valleys in the Alps, where they lead out from the mountains to the lower Piedmont lands. In 1862 Ramsay, geologist of wide experience, called attention to the close association of lakes in general with regions of former glaciation, and boldly concluded that the Alpine lakes resulted from the excavation of over-deepened trough-basins in the floors of Preglacial river-valleys by glacial erosion, even though some of the lakes are 1,000 or 1,500 feet deep.[2] Lyell, famous contemporary of Ramsay's, doubted the capacity of the ancient glaciers to excavate trough-basins of so great depth, and ascribed the Alpine lakes to a gentle crustal warping or flexing along the northern and southern flanks of the Alps, whereby belts of country within the mountain margins were slightly depressed, so that the main, out-going valleys of river-erosion, which had previously led by continuous descent, north and south, from mountain crest to mountain border, were given a reversed slope in their lower courses.[3] The valleys thus came to resemble a bent arm, in which water, running down from the shoulder, would gather as a lake in the elbow-sag and flow out over the wrist-sill. According to this pleasing theory, the chief function of the ancient glaciers was merely to occupy the down-warped basins for a time, thus preventing their

[1] Experimentation, by which the deduction of consequences has long been helpfully performed in physics and chemistry and is nowadays coming to be increasingly performed in biology, has only a limited application in geology, largely because of the impossibility of truthfully reproducing the spacial and time elements.

[2] A. C. Ramsay, "On the Glacial Origin of Certain Lakes in Switzerland," *Quart. Journ. Geol. Soc.*, **18**, 1862, 185–204.

[3] Sir Charles Lyell, *The Antiquity of Man*, 1863, 309–319. See also T. G. Bonney, "Do Glaciers Excavate?" *Geog. Journ.*, **1**, 1893, 481–99.

being filled with detrital deposits, except in so far as deltas have been formed in the upper ends of the lakes since the glaciers disappeared.

It is noteworthy that, in spite of their wide experience in the discussion of geological problems, neither Ramsay nor Lyell expanded the "two-faculty" method sufficiently into the "four-faculty" method of investigation, as a means of testing his theory of Alpine lakes. Ramsay appears to have discredited valley warping and to have preferred glacial erosion for the production of lake basins; while Lyell minimised glacial erosion and depended on valley warping for the creation of the lakes. No sufficient proof was given by either of these able geologists in support of his opinion; neither one showed deductively what kind of a lake should result from the lake-making process that he adopted; and the origin of the lakes therefore necessarily remained unsettled. The application of the "four-faculty" method to this problem was left to the famous naturalist, A. R. Wallace, who, although not particularly trained in geological science, appears to have been exceptionally keen-minded in this matter.[1] He showed deductively that, if the lake basins had been excavated beneath the floors of Preglacial river-valleys by glacial erosion, the lakes would have no lateral embayments; while, if the basins were formed by valley warping, every lake would have as many side bays as the down-warped part of the main valley had side valleys. Then, confronting these strikingly unlike consequences of the two theories with the facts and thus discovering that the simple shore lines of the lakes immediately confirmed Ramsay's theory and flatly contradicted Lyell's, he was well warranted in concluding that the lakes were the product of glacial erosion and not of valley warping, whatever preference he may have previously had for one of these processes, and whatever prejudice he may have had against the other. His adoption of one explanation and rejection of the other was logically compulsory.

The lesson taught by Wallace's impartial treatment of the problem does not, however, seem to have been generally learned; for following an account of the Alpine lakes given not long ago before the Royal Geographical Society by Collet, of Geneva, in which their glacial origin was presented as well proved, one of the hearers dissented from that conclusion and stated that he preferred to explain the lakes as occupying local depressions caused by the weight of the glaciers which formerly occupied them; yet he did not, in announcing this explanation, show that its deduced consequences could successfully confront

[1] "The Ice Age and its Work," *Fortnightly Review*, liv, 1893, 616–33, 750–74.

the facts; and, as a matter of logic, they could not do so, for, if the basins had been thus formed, the lakes occupying them to-day should be embayed quite as much as if the basins had been produced by Lyell's process of down-warping.

A paragraph may be here devoted to inquiring why the second two of the four above-named mental faculties or methods of thinking, all of which are so essential in geological investigation, have so often been neglected by geological investigators. Their neglect is probably due in large measure to the habitual association of logic, under which methods of thinking are usually studied, with philosophy; for, as a result of this association, the nature of mental processes is commonly presented in a rather dry and abstract manner. It would be better if logic were divorced from philosophy and associated with science, where the nature of mental processes might be taught vividly and directly by the case method, as called for. The neglect is also in part due to the growth of science to so enormous a volume of facts and theories that most of its teachers and students are occupied in imparting and learning its content rather than its methods. This is true also of textbooks; in geology they do not sufficiently emphasise the argumentation by which conclusions have been reached, because they hardly have space for the conclusions alone: if this statement is perhaps too sweeping for geology in general, it certainly holds good for the coral-reef problem. A third reason for the neglect of deduction and confrontation seems to be that undergraduate students are, as a rule, hardly mature enough to apprehend the "four-faculty" method, even if it is deliberately set before them in the course of their studies: hence, even if such students have heard of this method in their college years, they may fail to apply it in their later investigations. A fourth reason is that many persons who have undertaken geological research have never heard anything whatever of the "four-faculty" method.

Wharton appears to have made no more effort to extend his theory deductively to its various consequences and to confront the consequences with the facts than did Ramsay and Lyell to extend their theories; but, for that matter, neither did Darwin successfully apply deduction and confrontation to his subsidence theory. Hence both Wharton's theory of the upgrowth of reefs from platforms abraded on stationary islands and Darwin's theory of the upgrowth of reefs from non-abraded, subsiding islands are unsatisfactory and unconvincing in the form given them by their authors; all the more so because the consequences of both these theories are easily deduced. The consequences may be presented and contrasted essentially as follows:

First, during the abrasion of a non-subsiding volcanic island,

around which, before truncation is complete, a barrier reef is to grow up, the island must according to Wharton's theory gain a cliffed and non-embayed shore line, and most of the radial valleys eroded in the island slopes by its consequent streams must have hanging mouths, because the waves will cut the cliff base back faster than the streams can cut their valleys down in the cliff face. On the other hand, during the progressive subsidence of an island, around which a barrier reef is growing up according to Darwin's theory, the shore-line of the diminishing and more or less dissected island will not be cliffed, because the reef will act as a breakwater and hold off the attack of the waves ; but it must be embayed, because the ocean waters will invade the lower end of every valley as it dips below sea-level. The central volcanic islands of barrier reefs must therefore present strongly contrasted features according as the reefs are formed by the processes of Wharton's or of Darwin's theory.

Second, at a later stage of truncation the vanishing remnant of an originally good-sized island will, according to Wharton's theory, be reduced to one or more cliffed islets, and within the polygonal space defined by such islets, if several of them survive, no deep abrasion can take place ; but, according to Darwin's theory, the residual summits of a well-dissected and almost submerged island will be of sloping, mountain-top form, too small to show distinct embayments ; and the lagoon waters may have a considerable depth within the islet polygon. It is true that relatively rapid subsidence might almost drown an island in an early stage of its dissection, while its peaks are as sharp as those of lofty Tahiti ; but subsidence at that rate is improbable, because no small, sharp-peaked islands are found surrounded by almost-atoll reefs ; if it occurred it would probably be too rapid to be compensated by reef upgrowth. Slow subsidence, such as Darwin's theory demands, would seem to give time for the reduction of the sharp peaks of early mature dissection to the subdued forms of late maturity before they sink below sea-level. The islets of almost-atolls must therefore, like the larger islands within barrier reefs, present unlike features, according as they are formed following Wharton's or Darwin's theory.

Let it be remembered here that all the islands which come to serve as foundations for barrier or atoll reefs were initially, at the close of their eruption construction, essentially alike. The difference between the surviving islands within barrier-reef and the vanished islands of atoll foundations is that, according to Darwin's theory, the atoll foundations have subsided more than the barrier-reef foundations ; but, according to Wharton's theory, the difference is that barrier-reef islands

have been, for some entirely unexplained reason, sooner encircled and protected by upgrowing reefs on the margin of incompletely truncated foundations, while the upgrowth of atoll reefs has been delayed until completely abraded platforms have been provided for them. Wharton's failure to indicate any efficient cause for the early cessation of abrasion on barrier-reef islands and its long continuation on atoll foundations clearly leaves his theory in an unsatisfactory state.

Third, if a barrier or atoll reef be uplifted, its reef and lagoon limestones must, according to Wharton's theory, be found to rest upon a flat platform of truncated volcanic rocks ; while, according to Darwin's theory, the limestones must rest on the slopes of a more or less dissected cone. Hence here, for a third time, the consequences of the two theories are well contrasted.

Now, if we proceed to the Pacific and confront these several consequences of the two competing theories with the facts there found, no hesitation need be felt in making choice as to which theory succeeds and which fails. To be sure, so long as only sea-level atolls are examined, no one can discover how they have been formed, because the peculiar consequences of the two theories are there invisible. Not until numerous deep borings are made in the reefs and lagoon floors of such atolls will it be possible to determine whether a flat platform of volcanic rock underlies the lagoon at a moderate depth, as Wharton's theory demands, or whether the lagoon floor is underlaid by a sloping and dissected volcanic cone, as Darwin's theory demands. As no such borings have yet been made, this fourth consequence of the two theories need not be further considered for the present. All the more attention must therefore be given to the central islands of barrier reefs, to the central islets of almost-atolls, and to islands exposing the foundations of uplifted reefs ; and during the examination of these islands, the unlike consequences of the two theories must be borne clearly in mind.

The central islands of barrier reefs have, in nearly all cases, non-cliffed and well embayed shores. This clearly favours Darwin's theory and as clearly contradicts Wharton's. In a few cases, however, particularly in the exceptional case of Tahiti, the island shores are cliffed ; but in Tahiti the valleys by which the continuity of the cliffs is repeatedly interrupted have been embayed, apparently by subsidence (although most of the embayments are now filled by delta plains, as if the post-cliff-cutting subsidence had been followed by a brief still-stand pause) ; the cliffs must therefore be understood to plunge below sea-level and to show only the non-submerged, upper part of their entire height, as I have explained in a special article on

this highly instructive island.[1] It thus seems that, after an early period of active cliff-and-platform abrasion and of valley erosion, Tahiti subsided; and it must have been during this period of subsidence that its present barrier reef grew up. Réunion, in the western Indian Ocean, should be considered in this connection, for it has a strongly cliffed and non-embayed shore on which, in the absence of protecting reefs, the ocean waves are still breaking violently; hence Réunion not only represents the pre-subsidence stage of Tahiti, but also makes clear why reefs are generally absent in the early stage of a volcanic island's history; for during that stage, before subsidence has well begun, a beach of cobbles, gravels, and sand is formed around the shore, and on such a beach no coral reefs can grow. Not until subsidence drowns the cliff-base beach can reef-building organisms establish themselves.[2]

Before leaving barrier reefs, it should be pointed out again that Wharton's theory is incomplete in not suggesting any cause for the establishment and upgrowth of such reefs on platform margins before the abrasion of their islands is completed; and incomplete also in not giving a reason for the long reefless time that must elapse between the incomplete truncation of a barrier-reef island and the reduction of a completely truncated island to a "perfectly flat bank" 30 or 40 fathoms below sea-level, from which an atoll is to grow up. Some reasonable cause ought to have been proposed for the early establishment of barrier reefs and for the long-delayed establishment of atoll reefs; but not even the need of such a cause appears to have been recognised.

If we next turn to almost-atolls, their central islets are not cliffed but have a rounded and sloping, mountain-top form, as they should have according to Darwin's theory, but as they should not according to Wharton's theory. This is made clear not only by large-scale charts, but also by the many photographs of almost-atoll islets in Mangareva (Gambier) and

[1] "Les falaises et les récifs coralliens de Tahiti," *Ann. de Géog.*, xxvii, 1918, 241–84.

[2] Gardiner writes: "We wonder whether Davis, if he had seen Réunion, would have prophesied about it with such certainty. I would point out that movement of sharp sand along coasts cuts into and destroys seemingly well-established corals." I could wish that my reviewer had expanded those two sentences in order to state explicitly what facts seen on Réunion and not shown on its chart call for a revision of my explanation of its reeflessness, as his first sentence implies; and also to make clear why the opinion stated in his second sentence is introduced as if it contravened the opinion expressed in my book. I not only agree that the long-shore drift of sands will destroy well-established coral reefs (see my account of the Great Barrier Reef on the Queensland coast at its southern end in *The Coral Reef Problem*, pp. 353 *et seq.*), but go farther in believing that such shore drift will prevent the establishment of reefs.

Truk in two of Agassiz' monographs. It is singular that neither Darwin nor Wharton called attention to the testimony given by the visible form of these candid little witnesses. Moreover, in Mangareva a depth of 47 fathoms is found well within the polygon defined by several of its islets; again, within the large lagoon of Truk, two soundings of 58 fathoms have been made by German navigators; and such depths are, as to position in the first case and as to measure in the second, beyond reasonable explanation by abrasion without subsidence. To be sure Wharton did not exclude subsidence, but if it is admitted the level floors of relatively shallow lagoons have to be explained by smooth sedimentation, essentially as Darwin supposed; and then the need of abrasion, upon which Wharton depended, vanishes.

The evidence furnished by elevated barrier and atoll reefs comes next. When the consequences of the two theories for such reefs are confronted with the facts Wharton's theory is again contradicted as directly as Darwin's is supported. As to elevated barrier reefs, Marshall's admirable studies of Mangaia and Atiu in the Cook group, subventioned and published by the Bishop Museum of Honolulu, show conclusively that their reefs rise from the non-cliffed slopes of well-dissected volcanic islands, which must have subsided while the reefs were growing up around them, and which must have had strikingly embayed shores when the reef crests lay at sea-level before subsidence was reversed into upheaval. Similarly, a number of elevated and more or less dissected atolls in eastern Fiji disclose no evenly abraded volcanic platforms, such as Wharton's theory demands, although several of them are dissected to depths decidedly greater than 240 feet below their original reef rims; and in the few examples of these elevated atolls where underlying volcanic rocks are exposed they have subdued submountainous forms, such as would be produced not by abrasion but by prolonged subaerial erosion; and on these subdued forms the reef and lagoon limestones lie unconformably, precisely as they should according to Darwin's theory.

The account of Tuvuthá, one of these elevated atolls in Fiji, by Foye,[1] is especially pertinent; this island, when seen from the sea, appears to have the form of a truncated cone. Foye ascended its slope, where he saw many fossil corals in the position of growth, and, on reaching the nearly level rim-crest at an altitude of about 800 feet, he saw that it enclosed a roughly circular moat, from the centre of which rose a mass of volcanic rock, not with the form of a flat platform, but of subaerially eroded hills, 540 feet in summit altitude. He there-

[1] W. G. Foye, "Geological Observations in Fiji," *Proc. Amer. Acad. Arts and Sci.*, **54**, 1918, 1–145.

fore concluded that this foundation mass represents a volcanic island that had been maturely eroded after its eruption was ended, that had subsided during its erosion, that had been unconformably covered with reef and lagoon limestones during its subsidence until an atoll reef rose above its submerged summit not long ago, and that had then been elevated. The same observer reported certain members of the not-distant Exploring Isles of eastern Fiji as also consisting of a subaerially carved base of volcanic rocks unconformably covered by the limestones of uplifted and dissected atolls.

It is difficult to see how any theory but Darwin's can account for these significant features. Evidently, if the limestones of all these elevated atolls had been found to rest on flat platforms of volcanic rock, an excellent case for Wharton's abrasional theory—or for Daly's Glacial-control theory—could be made out; but, in the absence of such platforms, I find it impossible to give credence to any theory of atoll origins in which the abrasion of flat platforms enters as an essential factor.[1]

It thus appears that when certain essential consequences of the abrasional theory which Wharton did not consider are confronted with certain facts of which he took no adequate account, the consequences are strongly contradicted and the theory is thus shown to be incompetent. On the other hand, it also appears that when certain essential consequences of the subsidence theory which Darwin did not work out are confronted with certain facts to which he gave insufficient attention or of which he had no knowledge, the consequences are strikingly confirmed and the theory is thus found to be wonderfully competent.

Lest I may be suspected of having a prejudice against the production of reef platforms by abrasion, let me call attention to the islands and reefs in the marginal belts of the coral seas, where Darwin's theory, slightly modified in the light of Daly's Glacial-control theory, calls for the presence of such platforms and where their presence is clearly proved by the occurrence of cliffed volcanic islands, which are so rare in the coral seas proper. The case is briefly as follows: The limits of the coral

[1] Daly's theory of atolls assumes that they have grown up from flat platforms of abrasion, cut on old, worn-down, and deeply weathered volcanic islands when the ocean was lowered some 30 or 40 fathoms in the Glacial period; but, if so, there should be, among the many scores of Preglacial islands in the Pacific that now, truncated, serve as atoll foundations, at least a few younger islands which were not so greatly worn down or so deeply weathered as the older islands when the Glacial period arrived; and these younger islands would have been incompletely cut away by low-level abrasion, thus giving them the form of cliffed residuals; but there are, with the exception of Tahiti, no such cliffed islands in the true coral seas; and the Tahitian cliffs cannot be reasonably explained by low-level abrasion because the neighbouring islands of the Society group are not similarly cliffed.

seas are determined by the lowest shallow-water temperature at which reef-building organisms can exist. In Preglacial time, when somewhat higher temperatures than those of to-day are believed to have prevailed, the coral seas should have been expanded somewhat outside of their present limits; during the Glacial period, when lower temperatures prevailed, the limits must have been contracted within those of to-day. The belts, north and south of the Equator, across which these limits must have migrated in consequence of Quaternary climatic changes, may be called the marginal belts of the coral seas. The torrid area between these belts, within which reef growth has been persistent, may be called the true coral seas.

During the Glacial period reef-building organisms in the marginal belts must have perished and the dead reefs must have been cut away by the ocean waves. As the ocean was then somewhat lowered by the withdrawal of some of its water to form continental ice-sheets, the Preglacial reefs and their lagoon-floors must have been more or less completely cut away in low-level platforms. The relatively weak lagoon limestones of atolls may have been completely cut away, but the more resistant volcanic rocks in the central islands of barrier and almost-atoll reefs would have been as a rule cut away only in part, thus leaving the islands with cliffed shores. With the coming of a milder Postglacial climate, reefs would grow up more or less actively from the platforms, producing atolls over completely truncated islands and barrier reefs or almost-atolls over incompletely truncated islands; and from the central area of barrier or almost-atoll lagoons would rise a cliffed island or several cliffed islets. Hence, according to this deductive view of the problem, the marginal belts will be characterised by the occurrence of cliffed islands or islets surrounded by more or less aggraded, reef-bearing banks; and the breadth of the marginal belts will be indicated by the presence of such islands.

Now it is certainly remarkable that, while the volcanic islands of the true coral seas are, with very few exceptions as above noted, not cliffed, practically all of the volcanic islands of two belts, lying roughly between latitudes 23° and 28° north and south latitudes—and also the Marquesas Islands, which appear to lie in the southern marginal belt where it curves northward in the eastern Pacific to cross the Equator and join the northern belt—are strikingly cliffed, as I have pointed out in a special article[1]; and also that on the banks surrounding these cliffed islands, which are presumed to represent platforms of low-level abrasion more or less aggraded,

[1] "The Marginal Belts of the Coral Seas," *Amer. Journ. Sci.*, 6, 1923, 191–5.

coral reefs are at present growing up—except that the banks around the cliffed Marquesas Islands are free from reefs, as if the ocean temperature there were not fitting for reef growth. But these marginal-belt reefs are very unlike the stalwart veteran reefs of the true coral seas, which were not cut away during the Glacial period ; for the marginal-belt reefs are only timid Postglacial novices. They are narrow and discontinuous, they usually fail to reach present sea-level, and they commonly stand back from the outer edge of their banks, as if the temperature of ocean water on the banks had not long been favourable for reef growth. There can be no question that abrasion has acted here, for by no other process than abrasion can the cliffs of the islands and islets be accounted for ; but abrasion probably took place, as Daly's Glacial-control theory demanded, at a somewhat lower level than that of the present ocean surface, because the island cliffs seem to plunge below sea-level. In other words, where direct evidence of abrasion is found in the form of island cliffs the operation of that very effective process must be accepted as a matter of course. On the other hand, where no evidence of abrasion is found its operation should be, equally as a matter of course, rejected.

In the course of an extended review of the coral reef problem Wharton's theory became so completely unsatisfactory to me that I find it difficult to understand how a student of coral reefs so widely experienced as Gardiner can still give it credence. In the course of the same review the great superiority of Darwin's old theory over all its later competitors became more and more apparent.

Murray's theory of outgrowing barrier reefs around still-standing islands and of atolls derived from them has five peculiar consequences, none of which he deduced, all of which are, I believe, unescapable, and all of which are strongly contradicted by the appropriate facts. Murray's other theory of atolls, according to which their reefs are supposed to crown organically aggraded, still-standing banks, is contradicted by the structure of elevated atolls as far as they are known ; for most such atolls are not found to rest on pelagic deposits ; and beneath two that do so rest—namely, Roti in the East Indies and Barbadoes in the West Indies—the pelagic deposits are found to rest unconformably on subaerially eroded foundations ; and such unconformity indicates a subsidence which Murray's theory explicitly excludes. Guppy's theory of barrier and atoll reefs formed on rising shoals or islands has three characteristic consequences, none of which he took into account and all of which are contradicted by the facts. The case for Wharton's theory of abraded atoll foundations has been stated above ; its three inevitable

consequences were wholly overlooked in his essays and all of them are seen to be contradicted by the facts as soon as they are worked out. Darwin's theory of upgrowing reefs on subsiding foundations has three singular consequences, none of which he deduced, all of which are inevitable, and all of which are most beautifully confirmed by the facts.[1] It is not only difficult, but unnecessary to maintain an open mind regarding so successful a theory.

[1] See "The Formation of Coral Reefs," *Sci. Monthly*, xxvii, 1928, 289–300; also "Die Entstehung von Korallenriffen," *Zeitschr. Ges. f. Erdk.*, Berlin, 1928, 359–91.

THE DISCOVERY OF THE GAS LAWS

By W. S. JAMES, M.Sc., A.I.C.

II. GAY-LUSSAC'S LAW

In a previous article dealing with the history of Boyle's Law, it was shown that this generalisation was founded as early as 1661. Gay-Lussac's Law, on the other hand, was not firmly established until 1802 owing to the failure of experimenters to appreciate the rôle played by a water surface in contact with the expanding gas. In this article it will be shown how the gradual elimination of these errors led to the ultimate establishment of the law.

Work of Amontons, 1699 *to* 1702.—It was known long before Amontons that air expands when it is heated, but he, in 1699, was the first to investigate the relation between temperature and expansion. Galileo had used the principle that air expands on heating in the construction of his thermometer, but no relation between temperature and volume was assumed, the stem being arbitrarily graduated.

Amontons used a U-shaped tube with the shorter arm ending in a bulb and the longer measuring 45 inches (Fig. 1). The height of the mercury in the longer arm above the level in the bulb was a measure of the "spring" which the air had acquired. This instrument he treated as a Constant Volume Air Thermometer. Although the air was not accurately at constant volume, it was almost so kept, as the tube was narrow compared with the bulb. His first observation was that if the instrument was placed in boiling water the mercury always stood at the same level, no matter how long or how vigorously the water was boiling. Although not the first to arrive at this conclusion, he was thus independently led to the idea of the constancy of the boiling point. He was, however, unaware of the dependence of the boiling point upon air-pressure.

Fig. 1.

From experiments in 1702 with different volumes of air under the same initial pressure (at room temperature), he found that at the temperature of boiling water they were all able to sustain the same excess pressure. He expressed this as follows:

"Les masses inégales d'air chargées de mêmes poids, ou de poids égaux, augmentaient également la force de leur ressort par des degrez de chaleur égaux."

From a series of experiments with different initial pressures he found that at the temperature of boiling water the air could always sustain a column of mercury $\frac{1}{3}$ longer than it could at room temperature. His conclusion was :—

"Un même degré de chaleur, pour petit qu'il puisse être peut augmenter toujours de plus en plus la force de ressort de l'air, si cet air est toujours chargé d'un poids de plus grande en plus grande."

The law connecting pressure and temperature at constant volume has thus been referred to as "Amontons's Law." From Mariotte's Law, Amontons realised that what is true for an increase of pressure at constant volume is also true for an increase of volume at constant pressure. His researches really amount to an experimental proof of Gay-Lussac's (or Charles's) Law applied only to air, but his results and apparatus are not sufficiently accurate or reliable to justify any definite conclusion being drawn. Amontons was the first to arrive at some notion of absolute temperature.

"It appears that the extreme cold of this thermometer is that which would reduce the air by its spring, to sustain no load at all." From Amontons's data the absolute zero on the centigrade scale would be $-239{\cdot}5°$, and his coefficient of expansion per degree C. $\frac{1}{239 \cdot 5}$, a value far more accurate than most of those which were obtained during the next hundred years.

The Reaction after Amontons.—Soon after Amontons's work was published many physicists tried to repeat and verify his work, but with indifferent success.

In 1705 Nuguet found the ratio of the volumes of a given mass of air at the temperature of boiling water and ordinary temperature to be 2 : 1. In two other experiments he obtained 16 : 1. His apparatus consisted of a flask which he placed upside down in water, and which contained a small amount of air, whose volume he could read.

In 1708 La Hire, struck with the diversity of the results, investigated the subject. His apparatus was the same as that of Amontons except that the bulb carried a small tube for introducing the mercury. He was thus able to start with the air in the bulb at atmospheric pressure. In a first experiment he found that, on raising the air in the bulb from room to boiling water temperature, it could not sustain the extra column of mercury equal to one-third of the barometric height (as Amontons had stated). In a second experiment La Hire

found that, starting at a lower temperature and a higher barometric pressure, the air could not even sustain a mercury column as high as in the first case. These results were contradictory, but La Hire suspected no error and concluded "that one is obliged to confess that we do not understand the nature of air." La Hire attributed the great difference between his results and those of Nuguet as being due to the presence of water in the bulb of Nuguet's apparatus and to the fact that his enclosed air was in contact with the surface of the water in the bath. La Hire repeated Nuguet's experiments and obtained similar results. He became convinced therefore that the presence of water in the bulb causes abnormal results. About the same time Stancari showed that water greatly increased the rate of expansion of a given mass of air. Later experimenters, however, were slow in recognising the value of these observations, and it was a long time before the necessity for the absence of contact with the liquid phase became appreciated.

The following striking passage comes from H. Boerhaave's *Elementa Chemiæ*, published as early as 1732.

"Air of unequal masses, but of the same density, is always expanded in the same measure by the same degree of fire; so that these expansions in the same density of air, by a constant law of nature, are always proportional to the augmentations of heat."

Although he limited his "constant law of nature" to air of the same density, yet he seems to realise that the increase of volume is proportional to the increase in temperature. His opinion, however, was of little value, being based on insufficient experimental records, chiefly of Amontons.

In 1772 de Luc, from a consideration of the effect of temperature upon the relation between the altitude of a place and the logarithm of its barometric pressure, arrived at the result that the coefficient of thermal expansion was $\frac{1}{215}$ per °R. at ordinary air temperatures. This value was used by Lavoisier and Laplace in 1783.

In 1777 Roy published the results of a large series of experiments on the expansion of air. He found that 1,000 volumes of air at atmospheric pressure expanded to 1,484 volumes between 0° C. and 100° C., from which the calculated coefficient is $\frac{1}{207}$. There was less expansion when 1,000 volumes of air at pressures greater or less than atmospheric were taken. He pointed out that this contradicted the experiences of Boyle and Mariotte. Also he found a maximum expansion between

52° F. and 72° F. He investigated the effect of adding a little water to the gas whose expansion was to be determined, and showed "how greatly superior the elastic force of moist is to that of dry air."

Up to this time experiments had been limited to the use of air. Priestley was the first to investigate the expansion of other gases, but his apparatus and method were very unsatisfactory and consequently his results are correspondingly poor. He pointed out, however, that his work on this subject was only meant as a guide for further investigation and that he did not place great confidence in the results.

The gas to be experimented with was collected in the phial over mercury (Fig. 2). A drop of mercury, M, was left in the

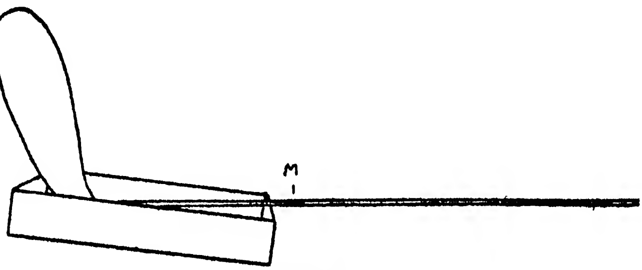


FIG. 2.

neck of the phial, and its position showed the expansion of the gas. The phial was carried into rooms of different temperatures. Since all the experiments were carried out with the same phial and same tube inclined to the same extent, Priestley was able to compare the expansions of different gases. The room temperatures were ascertained with a Fahrenheit thermometer.

The following are his calculated results representing the expansion along the tube in inches answering to a difference of 10° F.

Common air .	1·31 inches
Hydrogen .	2·05
Nitric oxide .	2·02
Carbon dioxide	2·20
Hydrochloric acid gas	1·33
Oxygen . .	2·21
Nitrogen . .	1·65
Sulphur dioxide	2·37
Hydrofluoric acid gas	2·83
Ammonia . .	4·75

Priestley suspected phlogiston as being the cause of some of the inequalities, but the large value of ammonia, he said, was due to the presence of moisture.

Views of Lambert, 1779.—In 1779 Lambert published his *Pyrometrie, oder vom Maasse des Feuers und der Wärme*, in which was given an account of his experiments on the relation between the volume, or pressure, of a gas and its temperature. With an accurately calibrated and weighed air thermometer he found that 1,000 volumes of air at 0° C. expand to 1,375 volumes at 100° C. After allowing for the expansion of the mercury and glass vessel he obtained 1,370 as the volume at the higher temperature, which was the most accurate value up to his time. Lambert also repeated Amontons's experiments, and as a result was able to suggest more clearly the idea of an absolute zero.

But Lambert is chiefly important for formulating the law that at constant pressure "the power of the heat increases in proportion to the expansion, or increased space." He also announced that at constant volume "the power of the heat is proportionate to the elasticity of the air."

These theoretical conclusions seem to have been based upon an insecure experimental foundation, but nevertheless they show a high degree of insight into Nature's problems. Lambert further showed this prophetic gift in his remarks concerning absolute zero. The volume of the air at absolute cold, he remarked, is zero, "or as good as zero," and, being so small, can be regarded as nothing. At this temperature the particles of air fall together "and become, so to speak, water-proof," and it is this volume of the compact molecules which prevents the volume of the air-space from entirely disappearing at absolute zero.

Cavendish's Unpublished Work on the Gas Laws.—Cavendish's papers show that in December of 1779 he commenced an investigation into the vapour pressure of water and its effect on the expansibility of air; and as a preliminary he studied the expansion of dry air between 32° and 150° F. He probably dried his air by dry pearl-ash and filter-paper, which were the drying agents used in his period. He used a modified form of Amontons' apparatus, and found the "expansion of air by 1° is $\frac{1}{500}$ of its bulk at $38\frac{11}{12}^{\circ}$ or $\frac{1}{461}$ of bulk at 0° or $\frac{1}{511}$ of bulk at 50°."

These figures demonstrate that air expands by an equal amount for each degree and their degree of accuracy is shown by the following values of absolute zero calculated from them: 272·8° C.—273·9° C.

In another unpublished paper, which is not dated, is found a comparison of the rates of expansions of different gases. No details were given of the method of preparation of the gas,

no methods of heating or drying it, and no correction for the expansion of the bulb was made. The investigation was apparently a rough one, and in view of the large errors of observation, a correction for the expansion of the bulb would be superfluous. He experimented with air, nitric oxide, carbon dioxide, oxygen, nitrogen, hydrogen and "heavy inflammable air"—probably impure carbon monoxide. For each gas the rate of expansion was found uniform for the degree in all parts of the scale between 45° F. and 85° F.; and moreover the coefficient, although too large, was sensibly the same for all the gases. It was characteristic of Cavendish's love of extreme accuracy that he refrained from stating any law as a result of his investigations, but had he published his papers he would have no doubt suggested the law as a probable hypothesis requiring further verification.

Cavendish's reluctance to publish his papers was unfortunate for the progress of many branches of physics; and, in particular, the scientific world had to wait another twenty years before the law of expansion of gases was clearly enunciated and experimentally demonstrated by Gay-Lussac. It is highly probable that Cavendish found that all gases expand equally and uniformly before 1787, the year in which Charles is thought to have made the discovery; the generalisation might thus have become known as "Cavendish's law"—a claim which Cavendish himself would, no doubt, have been the last to make.

In passing, it is interesting to note that Cavendish also demonstrated the truth of Dalton's law of partial pressures, but once more he allowed the discovery to pass unpublished and was content to see Dalton credited with the discovery twenty years later.

Saussure, in 1783, obtained values for the expansion of air corresponding to a coefficient of $\frac{1}{235}$ per degree R., *i.e.* $\frac{1}{294}$ per degree C. His experiments were conducted with a large bulb in which were enclosed a thermometer and barometer to indicate the temperature and pressure. He found that very damp air and dry air had almost the same coefficient of expansion.

In 1784, Luz published his *Vollständige Beschreibung aller Barometer*, in which he gave an account of his experiments on expansion of air. He dried his air and found that the ratio of the volume at 0° R. to the volume of 80° R. was 1 : 1·3775. Also he found the increase of volume for each 10° rise to be fairly constant. With damp air he found a far greater expansion. The experiments of Lambert and Luz do not seem to have been widely known, or to have had much effect on sub-

sequent progress in France or England. They were not mentioned by either Gay-Lussac or Dalton in their historical summaries. In Germany, however, they seem to have been regarded as of more importance, and when Gay-Lussac's work was published in 1802 it was regarded as a confirmation of, and advance upon, Luz's work.

In 1786 Monge, Berthollet, and Vandermonde published a memoir on iron. During their work they found it necessary to compare the amounts of hydrogen given off by a known weight of various kinds of iron and steel, when dissolved in dilute acid. To do this, they had to standardise the volumes of hydrogen—*i.e.*, calculate what the volume would be under normal barometric height and at 12° R. An investigation into the expansion at constant pressure of air and hydrogen was therefore carried out. An apparatus consisting of a bulb leading into a graduated tube dipping under cold water was used. The tube near the neck was bent so that the bulb could be transferred from a cold bath to a hot one, while the end of the graduated tube could remain below the surface of water in a separate vessel. The bulb was raised or lowered so that the level inside and outside the graduated tube was the same at both temperatures. The readings were thus taken at constant pressures. The air or hydrogen experimented with was thus in contact with a small surface of cold water. The temperatures of melting ice and boiling water were used and the result obtained was a coefficient of $\frac{1}{184 \cdot 83}$ for air and $\frac{1}{181 \cdot 02}$ for hydrogen for each degree Réaumur; that is, $\frac{1}{231}$ for air and $\frac{1}{226}$ for hydrogen, for each degree Centigrade.

In 1789 de Morveau and du Vernois published their work on the expansion of gases. A gas in a bulb was heated successively to 20°, 40°, 60°, and 80° R. by a water-bath, and the products of expansion were collected in separate vessels; these gases were placed in melting ice and their volumes found. From the results the coefficients of expansion were calculated. The results were poor and showed far greater expansion from 60° to 80° than from 0°–20°. Nitrogen and oxygen were found to expand from 4–5 times more than air between 0° and 80° R. Gay-Lussac, in 1802, severely criticised these values and the apparatus and method employed. He also suggested that water was present in their apparatus.

Work of Charles.—About 1787 Charles noticed that oxygen, hydrogen, nitrogen, carbon dioxide and air all expand uniformly and equally from 0° to 80° R. But he never published his results and it was by mere chance that this work came to the

notice of Gay-Lussac, who gave a short summary of Charles's work in his paper of 1802.

Charles's apparatus consisted of a barometer with a very long tube. The reservoir was surrounded by a chamber containing the gas to be experimented upon, which had been enclosed at 0° and under normal atmospheric pressure. On placing the barometer in boiling water the mercury rose in the tube and the difference in the heights gave the excess pressure which the gas had acquired.

Charles also experimented with gases which were soluble in water, but found that each such gas had its own expansion, different from all others. Gay-Lussac added: "In this respect my experiments differ greatly from his." Gay-Lussac, having seen Charles's apparatus, pointed out a defect in it. The barometer tube was fairly large in comparison with the capacity of the reservoir, and any alteration in the height of the barometer would mean a fairly large alteration in the volume of the gas. Gay-Lussac did therefore not place entire confidence in Charles's result and Gay-Lussac, and not Charles, should be credited with the discovery of the law of thermal expansion of gases.

Volta, 1793.—Several years before his more famous experiments in electricity, Volta carried out an investigation into the thermal expansion of air. He was acquainted with the results from previous research in England, France, and especially Germany, and respect was shown for the results of Lambert and Luz. But Volta's work seems to have exercised no effect upon later research in those countries, and Gay-Lussac and Dalton were apparently ignorant of his results. Working with air between 0° and 80° R., he found that it expanded uniformly by $\frac{1}{216}$ of its volume at 0° per degree R., and he clearly enunciated the law of uniform expansion for air. Unfortunately he did not extend his observations to other gases, although he had measured volumes of oxygen, hydrogen, and others, in other connections. Volta took special precautions to dry his air, and attributed the discordant results of many workers to the omission of this preliminary drying of the air and internal walls of the apparatus. He thought that, as the temperature increased in arithmetic progression, the vapour pressure of water increased in geometric progression; but he also thought it probable that water vapour, in the absence of the water phase, would also expand uniformly.

Dalton, 1802.—In 1802 Dalton published in the memoirs of the Manchester Literary and Philosophical Society a paper, which was read in 1801, dealing with various properties of gases and vapours. One section of the paper was concerned

with the expansion of gases by heat, and with the value of absolute zero calculated from certain assumptions, and also with the relation between volume of a gas and its absolute temperature.

Dalton, in reviewing the work of de Morveau and du Vernois, attributed their remarkable values to the presence of moisture in their sample of air. He himself dried his air with sulphuric acid before experimenting with it.

His apparatus consisted of a long tube, which was divided into equal lengths and whose internal diameter was $\frac{1}{15}$th inch. Before an experiment it was dried internally, and a small column of dry mercury introduced into it. This enclosed a definite amount of air whose volume at any temperature could be read off. The tube was placed in a deep tin vessel holding water, which could be heated up to the desired temperature.

Dalton found that 1,000 parts of air at 55° F. expand to 1,321 parts at 212° F., and, adding four parts for the corresponding expansion of the glass, he obtained 325 parts increase upon 1,000 from 55° to 212° F. The degree of accuracy of these results is shown by the fact that the calculated coefficient of expansion per degree Centigrade is ·00392, or $\frac{1}{255}$. Dalton did not find the expansion uniform. He agreed with Roy that the expansion is a slowly diminishing one above 57° F. "My experiments give for the 77½° above 55°, 167 parts expansion; for the next 77½° only 158 parts: and the expansion in every part of the scale seems to be a gradually diminishing one in ascending." Dalton experimented also with hydrogen, oxygen, carbon dioxide and nitric oxide, and found that the results of their expansion agreed not only with common air in total expansion, but also in the gradual diminution of it in ascending.

Dalton's conclusion was therefore: "All elastic fluids under the same pressure expand equally by heat, and that for any given expansion of mercury the corresponding expansion of air is proportionally something less, the higher the temperature."

Dalton gave but few figures upon which to establish his result. This is characteristic of his formulation of broad generalisations based upon insufficient and often most flimsy evidence. Later work has shown his conclusion regarding the similar expansion of all gases under the same pressure to be correct, but his idea of a smaller expansion per degree in higher temperatures to be without foundation. His work in this connection was entirely overshadowed by the publication in 1802 of Gay-Lussac's celebrated paper.

Gay-Lussac, 1802.—Gay-Lussac was struck by the lack of agreement between the results from previous investigations upon the thermal expansion of gases; not only between the actual numerical results obtained for the coefficient, but also between the discordant views upon the rates of expansion of different gases at different temperatures. He attributed the lack of uniformity in the results to the presence of water, and pointed out that when water evaporates it occupies a volume about 1,800 times greater and a large increase of pressure is consequently noted. Gay-Lussac felt the necessity of an accurate knowledge of the thermal expansion of gases in all branches of physical science, and more especially for calculating the volume a gas would occupy under different conditions of temperature and pressure. He showed, too, that no accurate experiments had been previously carried out on the dilation of vapours. In an experiment of Ziégler and Bettancourt, the vapour was always in contact with water, so that an increase of volume at a higher temperature was not due to the thermal expansion of pre-existing vapour alone, but also due to the production of new vapour. For these reasons Gay-Lussac carried out an elaborate series of investigations into the expansion of gases and vapours on heating, and enunciated the law which is now known as Charles's or Gay-Lussac's Law.

The gas to be experimented with was placed in a large bulb which communicated with the atmosphere by a long tube (Fig. 3). During the greater part of the experiment, however, the end of this tube was kept under mercury. The stop-cock at the neck of the bulb could be manipulated from outside the bath which contained the apparatus, by means of cords. The bath was heated and the water in it allowed to boil for 20 minutes. The end of the tube was taken out of the mercury and the stop-cock momentarily opened. The bulb was then placed in a mixture of water and ice and allowed to come to the temperature of the mixture. The stop-cock was opened and water entered the bulb, the levels inside and out being adjusted until they were the same. The stop-cock was now closed, the bulb removed from the water, dried externally and weighed. It was afterwards weighed full of water and also empty. From the results it is easy to calculate the volume of gas at 0° C., which expands to the volume of the bulb at 100° C. In some experiments Gay-Lussac used the apparatus shown in Fig. 4. The graduated neck dipped into a small mercury bath. The whole was placed in a boiling water bath as before, and the pressure at this temperature brought to atmospheric by the thin tube. The latter was withdrawn after readings had been taken, and the gas bulb placed for some time in melting ice.

The amount of water entering and the capacity of the bulb were found as before, corrections being made for the portion of the neck initially occupied by mercury.

Great care was taken to thoroughly dry the bulb and tube before each fresh experiment by means of a current of dry air. The mercury used was also dry and pure.

For gases soluble in water, and vapours, a different apparatus was used. Two similar tubes, similarly graduated, one containing air, and the other an equal volume of the

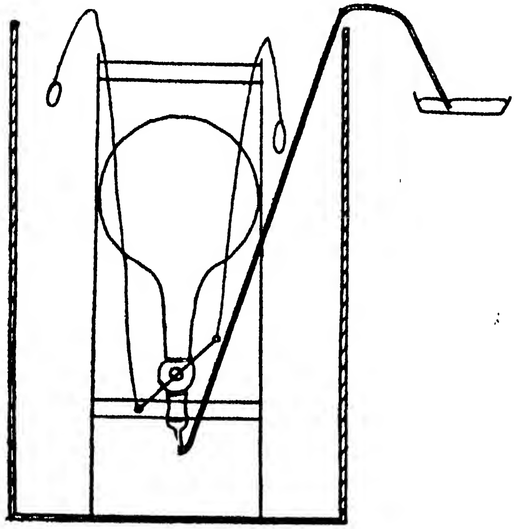


FIG. 3.

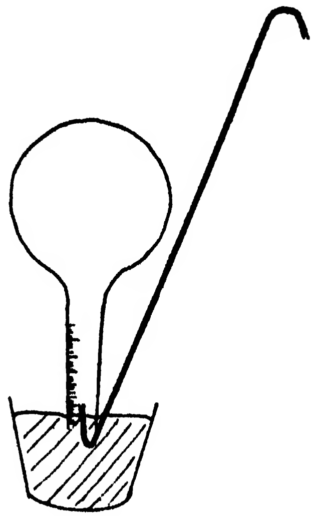


FIG. 4.

soluble gas or vapour, were allowed to stand over mercury. The whole was placed in a drying-oven and the temperature gradually raised. The expansion of air and the gas, or vapour, could thus be compared.

In six experiments with air, Gay-Lussac found that 100 volumes at the temperature of melting ice expanded to the following at the temperature of boiling water: 137·40, 137·61, 137·44, 137·55, 137·48, and 137·57 volumes.

The average is 137·50 volumes. This gives a coefficient of expansion $\frac{1}{266\cdot 66}$, or ·00375 per degree Centigrade.

Two experiments with hydrogen gave expansions from 100 to 137·49 and 137·59, giving an average of 137·52.

An average of three results with oxygen gave 100 volumes, expanding to 137·48 volumes, and with nitrogen the average from five experiments was 137·49 volumes.

The following table summarises these results :

	Volume at temperature of melting ice.	Volume at temperature of boiling water.
Air	100	137·50
Hydrogen . . .	100	137·52
Oxygen . . .	100	137·48
Nitrogen . . .	100	137·49

Gay-Lussac experimented with the following soluble gases and found that their expansions were exactly the same as that of air : ammonia, hydrochloric acid gas, sulphur dioxide, carbon dioxide, and nitric oxide. Their solubility did not in any way affect their rates of expansion ; but this only held true when the gases had been thoroughly dried. When this precaution had not been taken the gases always gave a greater expansion than air. Ammonia at first gave a greater expansion than the other gases, but after all moisture and ammonium salts, which dissociate on heating, had been removed from the gas and apparatus, it behaved normally.

To investigate the expansion of vapours, Gay-Lussac filled one of the tubes with ether vapour and compared its thermal dilatation with that of air. At all temperatures, except just above the boiling point of ether, the volumes of the ether vapour and air were exactly the same. Near the boiling point, ether vapour was found to condense a little more rapidly than air.

This result led Gay-Lussac to compare the thermal expansions of dry air and air containing water vapour. He found them to be exactly similar, but only when the water existed wholly in the vapour, and not in the liquid, state. Saussure's result that dry and damp air had the same coefficient of expansion was thus verified.

Gay-Lussac's formulation of the law was as follows :—
(1) " Tous les gaz, quelque soient leur densité et la quantité d'eau qu'ils tiennent en dissolution, et toutes les vapeurs, se dilatent également par les mêmes degrés de chaleur." (2) " Pour les gaz permanens, l'augmentation de volume que chacun d'eux reçoit depuis le degré de la glace fondante jusqu'à celui de l'eau bouillante est égale aux $\frac{100}{26666}$ du volume primitif."

Gay-Lussac did not definitely state in this paper of 1802 that a gas expands uniformly by $\frac{1}{266 \cdot 66}$ of its volume at 0° C. for a degree C. rise on any part of the scale. He realised that any such statement would require a careful investigation into the temperatures recorded by the mercury and air thermometers, and he was by no means certain that degrees on

the mercury scale represented equal increments of temperature. Gay-Lussac, therefore, carried out a series of experiments to test this and to verify his previous work on expansion. It seems remarkable that this later work on expansion, which has since been regarded as more important and accurate, was never officially published by Gay-Lussac, and was only made known by Biot, who gave a description of it in his *Traité de Physique* in 1816. Biot obtained his knowledge of the work directly from Gay-Lussac, and saw and sketched the apparatus himself (Fig. 5).

Great care was taken to dry the inside of the tube, G, which was to contain the gas to be experimented upon. A small

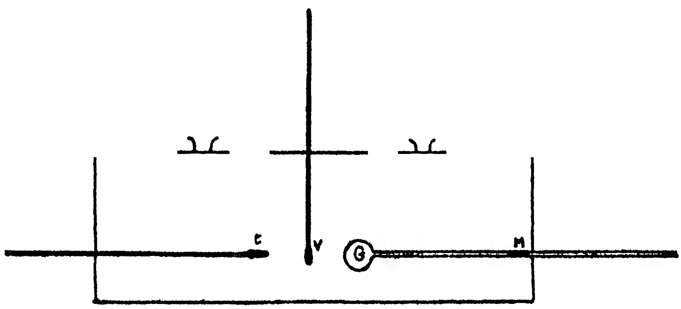


FIG. 5.

mercury index, M, enclosed the gas at constant pressure. The temperature of the bath could be raised from 0° C. to 100° C. and accurately taken by the thermometer, *t*, placed at the same level as the bulb, G. These two thermometers could be pushed in or drawn out so that the end of the mercury in each case was just visible, thus reducing errors due to the exposed stem to a minimum. V was a vertical thermometer used to give an approximate value of the temperature. In this way the expansion of the gas and mercury could be accurately compared, and if the atmospheric pressure altered during the experiment a corresponding correction was made.

Many experiments were made both with air and other gases, and Gay-Lussac verified his previous results, namely:

"(1) All gases subjected to equal changes of temperature, under the constant pressure, expand by exactly the same amount.

"(2) The amount of their common expansion from 0° C. to 100° is equal to 0·375 of their original volume at 0°."

He was also enabled to add a new conclusion :

"(3) Between 0° and 100° the expansion of a gas is exactly proportional to the expansion of mercury ; whence it follows that, for every degree centigrade, and under a constant pressure,

all gases expand by an amount equal to ·00375 of the volume which they occupy at the temperature of melting ice."

Summary of the Subsequent History of the Law

For many years the accuracy of Gay-Lussac's work and his value of the coefficient not only remained unchallenged, but even became more firmly established; for, in 1815, Dulong and Petit extended Gay-Lussac's researches to temperatures above 100° C. and still found the law to hold, and also verified his value of the coefficient; and Davy, in 1823, found the law held between pressures of six and one-fifteenth atmospheres, but his apparatus was too inaccurate to detect any small variation. The first experiments which pointed out the possibility of error in the value of the coefficient ·00375 were those of Flaugergues, who in 1825 obtained ·0037174; but no heed was given to his work. It came therefore as a surprise when, in 1833, Rudberg, a Swedish physicist, published an account of a series of accurate experiments from which he concluded that the true value must be between ·00364 and ·00365. He worked only with air, his early death preventing him from extending his observations to other gases. This proved a stimulus to other workers, and Magnus next attempted to find a more accurate value. Using Gay-Lussac's method for constant pressure, he found his results were not consistent, which divergence he attributed to the mercury index not completely preventing communication between the inside and outside air. He then used Rudberg's improved apparatus and obtained the following average values from many experiments: air ·00366508, hydrogen ·00365659, carbon dioxide ·00369087, sulphur dioxide ·00385618.

The value for hydrogen is seen to be somewhat smaller than for air.

Rudberg's value was thus verified and both Gay-Lussac's law and value of the coefficient shown to be only approximations. Magnus startled the scientific world with the surprising announcement—" la dilatation égale des gaz n'est pas rigoureusement juste."

In 1841 Regnault read to the French Academy the first of a series of papers on gases, in which he described experiments conducted with such care and manipulative skill that his results are still standards of reference. He first showed that the fault of Gay-Lussac's apparatus lay with the mercury index. He then showed that the coefficient varied with different gases, that of hydrogen being the smallest, and moreover, the values increased with increase of initial pressure and density, except in the case of hydrogen, when a decrease occurred.

Under normal conditions his values were :

Air ·0036706, hydrogen ·0036613, carbon dioxide ·0037099.

As a conclusion to this account of the history of the thermal expansion of gases it is worth while translating Regnault's conclusion to his work (1847), a conclusion which might be said to serve as an introduction to the next stage in the history of the gas laws—the stage of theoretically accounting for the deviations from the ideal :—

" To sum up, my experiments do not confirm the two fundamental laws of the theory of gases, hitherto accepted by all physicists, namely :

" (1) 'All gases expand equally between the same limits of temperature.'

" (2) 'The increase of volume of a gas between given limits of temperature is independent of its initial density.'

" Must these laws in future be banished from science? I do not think so. I rather think that these laws, the same as all such which have been recognised on gases, such as the law of volumes, etc., are true *in the limit*—that is, they approach more and more to complete correspondence with the results of observation as the gas is taken in a more and more dilated condition.

" These laws apply to a perfect gas, to which the gases which Nature gives us, approach more or less, according to their chemical nature ; according to the temperature at which they are considered, which may be more or less removed in each case from that point at which they would change their state ; but, above all, according to their state of small or great compression."

SYNTHETIC PERFUMES

By H. STANLEY REDGROVE, B.Sc., A.I.C.

THE science of Chemistry has invaded almost every department of daily life, without the man in the street being at all cognisant of the debt he owes to it. Nor is it realised how many common domestic operations, like cooking a dinner, for example, really consist in causing a number of more or less complicated chemical reactions to take place in the materials employed. By the man in the street and the housewife in the kitchen, " chemicals " are thought to be substances of a nature quite distinct from the things they daily handle and to be chiefly characterised by the possession of a " nasty smell."

There is, indeed, an important difference between the " substances " of the chemist and the raw products of nature, whether of animal, vegetable or mineral origin ; and it is very germane to the present study to ask, Wherein does this difference lie ? The answer is that chemical substances are " pure." It is true that when we seek to define what is meant by " purity " certain philosophical difficulties crop up, as Ostwald pointed out many years ago, and a long discussion could be entered into on the question, for example, whether solutions are chemical compounds or merely mixtures. For practical purposes, however, " purity " is well understood to denote obedience to the stoichiometric laws. A pure substance, moreover, possesses certain peculiar physical properties, such as a constant boiling point.

No doubt very few chemical substances ordinarily sold as pure are really pure, it being, indeed, extraordinarily difficult to obtain an absolutely pure substance. But chemistry—shall I say ?—strives after purity and attains to it very nearly.

On the other hand, Nature's products are never pure ; they are invariably mixtures, and usually very complex ones, taxing the skill of the analytical chemist to unravel the riddle of their composition.

The application of the term " impure " to the products of Nature, it will be understood, carries with it no implication of inferiority, which the man in the street, borrowing from the use of the word in the moral sphere, attaches to it when it is applied in the domain of material things. As a matter of fact,

the "impurities" present in natural products often enhance their value judged from human standpoints. Thus, from a purely chemical point of view, the trace of cholesterol in cod-liver oil is an impurity, the trace of ergosterol an impurity in the cholesterol, and the trace of vitamin D an impurity in the ergosterol. It is just this last impurity, however, which makes cod-liver oil so valuable a preventative of rickets.

I take another illustration, more cognate to the subject of Synthetic Perfumes. The natural perfume material of jasmine has been pretty completely analysed. It consists mainly of benzyl acetate, a substance easily synthesised from toluene. Now, benzyl acetate has a pleasant odour, reminding one very strongly of that of jasmine flowers. But it is certainly very inferior to this. The natural jasmine odour owes its perfection to the presence of other odorous bodies in association with the benzyl acetate, of which the most important are benzyl alcohol, linalol, linalyl acetate, methyl anthranilate, indole, and a ketonic body called "jasmone."

In the cases of many pleasantly odorous plants, the main constituent of the natural otto is known, as well as the chemical composition of the main "impurities," or, if the word seems a misnomer, let us say "subsidiary substances." Nowadays, it is usually a short step from the discovery of the chemical composition of a substance to its synthetic production, and, in many cases, these substances have been synthetically prepared. One can say, practically as a general rule, that the odour of the main substance gives a crude representation of the natural perfume of a flower. This odour is much improved by the addition of suitable proportions of the subsidiary substances. It still, however, almost invariably falls short of perfection; the reason being that there are further "impurities" present in most minute traces in the natural otto, which chemical analysis has failed to identify, but whose odours play their part in producing the fragrance of the flower.

Two important points emerge; first, the fact that extraordinarily small amounts of certain substances are capable of exciting the sense of smell, and may by their presence or otherwise modify the odour of a perfume; and, secondly, the fact that substances whose odours are unpleasant in a pure state may develop a pleasant fragrance in a state of extreme dilution and play an essential part in improving, from an æsthetic point of view, the fragrance of a perfume. Indole, present in the natural otto of the jasmin, is a case in point. Skatole, whose odour is one of the most unpleasant conceivable, affords an even more striking instance, since, used in tiny traces, this substance is distinctly valuable in compounding certain perfumes.

Synthetic perfumes have been criticised as having "coarse" odours. It will be appreciated that this coarseness may arise, not because of any positive property of the preparation, but because it lacks some of the essential "impurities." This explains why the growth of the synthetic perfume industry has not killed the natural perfume industry. Indeed, the effect has been quite the opposite, and the two industries are closely linked together. Chemical research has enabled many of the substances which are responsible for the sweet odours of flowers to be produced at a relatively low cost by synthetic means. Perfect perfumes, however, cannot be made with synthetic materials alone; to produce quite satisfactory results a proportion of the necessary "impurities" must be introduced by mixing with the artificial product a small amount of the natural one. The consequent cheapening in the cost of perfumes has resulted in a big increase in the demand for them to the benefit of both sides of the perfume industry.

Some analogies can be drawn between the aniline dye industry and that of synthetic perfumes. We must not, however, fall into the error of the man in the street, who seems to imagine that every chemical product comes from coal-tar. Certainly many synthetic perfume materials do, though some of the most important are made from raw products of a quite different nature.

In this connection the question arises, Where is the line to be drawn between a natural product and an artificial one? Essential oils obtained from plants by steam distillation are classed as natural products; but it would be rash to assume that no chemical changes whatever take place as a result of this operation. In the case, for example, of bitter almonds, it is well known that the essential oil is present in the kernels of the nuts, not as such, but in the form of a glucoside, amygdalin, which has first to be decomposed, the agent for effecting this decomposition, emulsin, being also provided by the kernels.

It seems reasonable, however, to class the products of such operations as steam distillation, and extraction with fats (*enfleurage*) or with neutral solvents like petroleum ether, as essentially "natural products," those obtained by the two latter processes having indeed special claims to be so considered, as their odours exactly represent those of the flowers from which they are derived.

By means of fractional distillation and, in some cases, by taking advantage of the property possessed by aldehydes of forming crystalline compounds with sodium bi-sulphite, certain of the constituents of essential oils can be isolated in a state of purity. Such products, usually called "natural isolates," are, of course, in no sense "synthetic"; but they may very well be

classed with the substances of synthetic origin inasmuch as they are " pure." The oils distilled from certain species of grasses belonging to the genus *Cymbopogon* are especially useful owing to their cheapness. Thus, from lemon-grass oil, distilled from *C. citratus* and especially *C. flexuosus*, the important alcohol, citral, is isolated. Palmarosa oil, distilled from *C Martini*, constitutes the main source for the isolation of the alcohol, geraniol, one of the constituents of otto of roses. Citronella oil, distilled from *C. Nardus*, provides a further source of this alcohol. The aldehyde, citronellal, is also isolated from this oil. Another very important natural isolate is the phenol-ether, eugenol, obtained from oil of cloves. In some instances these " natural isolates " provide starting points for the synthetic production of other important odorous substances, of which examples will be mentioned later.

So far I have written as though the one object of synthetic chemistry, as applied to perfumery, was the production by artificial means of the various constituents of floral ottos, in order that by mixing these together a chemically-exact replica of each and every otto might thereby be obtained. This is certainly one objective; but it by no means exhausts the field of research and practical achievement. In some instances, such as that of otto of roses, chemistry has admirably succeeded in its task, though, even so, the synthetic otto suffers from the imperfection common to all such preparations, and, for the production of a scent which is unmistakeably that of the rose, a small proportion of natural material must be added, preferably that obtained by *enfleurage*, or by the extraction of roses with petroleum ether.

Speaking generally, however, it may be said, in reference to this task, that chemistry, whilst so far not always successful in solving the problem, has done more than this. In some instances, natural perfume materials have up to date eluded analysis, and their exact chemical composition remains unknown. This is the case with ambergris, an important perfume of animal origin; or, to take an instance of a plant perfume, the exact constitution of the camphoraceous alcohol which is the main constituent of oil of patchouli still remains mysterious.

These are only two instances out of many, and much research remains to be done before it will be possible to say that the perfume of plants has yielded up its last secret. In some cases, however, in which it has not yet been possible to prepare synthetically a substance identical with the natural product, research has ultimated in the discovery of substances resembling this product in odour with a sufficient degree of exactitude to take its place, to a greater or lesser extent, in the art of perfumery.

An important example is provided by musk, the exact chemical composition of which is doubtful, though the main odorous principle would appear to be a methyl-cyclo-penta-decanone. Several synthetic "musks" have been prepared, exhaling the delightful fragrance of this exquisite perfume, which can be obtained far more cheaply than genuine musk and which have, to a considerable extent, replaced it save in the most costly perfumes. These artificial musks consist of nitro-aromatic compounds and bear no chemical relationship to natural musk whatever. The best is probably that known as "Musk Ambrette," which is a tri-nitro-butyl-meta-cresol-methyl-ether. Other imitation musks are provided by "Musk Ketone" (di-nitro-butyl-meta-xylyl-methyl-ketone) and "Musk Xylene" (tri-nitro-butyl-meta-xylene). The first artificial musk of commercial importance, it is interesting to note, was discovered accidentally by Baur so far back as 1888.

Moreover, in addition to producing imitations of some natural perfume materials and chemically-exact replicas of others, chemistry has enriched the art of perfumery with a whole multitude of odorous substances by means of which not only can the odour of flowers be imitated whose natural ottos it has not been found practicable to extract, but innumerable new nuances of fragrance can be produced.

It would be easy to fill pages with a bare catalogue of the chemical products whose odours render them of value in the making of scent. Many of these are very complex bodies, difficult to prepare and consequently of a costly nature, which are only employed in minute quantities for producing certain particular bouquets and "parfums de fantaisie." It will be more interesting to restrict our attention to some of the commoner synthetic products which are of everyday use in the confection of perfumes.

One of the first synthetic products to be used in perfumery was nitro-benzene, or "oil of mirbane." In the chapter devoted to "Materials used in Perfumery," in his *The Book of Perfumes*, published in 1867, Rimmel wrote: "The artificial series comprises all the various flavours produced by chemical combinations. Of these the most extensively used in perfumery is the nitro-benzene, usually called mirbane, or artificial essence of almonds. . . . Artificial essences of lemon and cinnamon have also been produced, but have not been brought to sufficient perfection to be available for practical use."

It was not a very auspicious beginning; for, not only is the odour of nitro-benzene very crude, but the substance is poisonous, and does not occur in the essential oil of bitter almonds. However, it was not long before real synthetic oil of bitter almonds, benzaldehyde, made its appearance, and nowadays the

use of this substance, which is extensively synthesised from toluene, either by direct oxidation or by chlorination followed by treatment with caustic soda, has very largely replaced the use of the natural oil both for perfumery purposes and for flavouring confectionery, etc., nitro-benzene being only employed to-day for scenting the cheapest and most inferior brands of soap.

Benzyl acetate has already been mentioned as the main constituent of the natural otto of jasmine. This is merely one of an enormous number of synthetically prepared esters which are valuable in perfumery, compounds belonging to this type often having agreeable odours. Certain of the esters of salicylic acid and cinnamic acid are particularly useful. Methyl salicylate is well known under the name of "oil of wintergreen." Amyl salicylate has a very pleasant odour, resembling that of certain species of orchids. It is extensively employed in making artificial orchid and clover perfumes. Some of the esters of cinnamic acid are well adapted for perfuming face-powders.

The flavour of vanilla is one universally liked. The odour of the natural product, the dried and cured fruits of *Vanilla planifolia* and allied species of orchids, is almost entirely due to the aldehyde, vanillin, the synthetic production of which is one of the great triumphs of synthetic perfume chemistry. Nowadays, the use of synthetic vanillin has largely replaced that of natural vanilla, both as a flavouring agent and, especially, in perfumery. The substance is made by several processes, in England from oil of cloves, on the Continent from guaiacol. Added to a perfume, vanillin gives a quality of sweetness and softness. Moreover, it possesses good fixative powers, serving to retard the evaporation of more volatile ingredients.

The synthesis from clove oil is of particular interest. The essential oil of cloves consists very largely of eugenol, which substance, as already mentioned, can be easily isolated from it. On treatment with caustic potash, eugenol undergoes an isomeric change, yielding iso-eugenol, itself a valuable perfume material, which forms the basis of most artificial carnation scents. On careful oxidation, iso-eugenol passes into vanillin.

Safrol, obtained as a by-product in the separation of camphor from camphor oil, undergoes similar reactions, being converted by treatment with caustic potash into the isomeric iso-safrol, which, when cautiously oxidised, yields the alcohol, piperonal. This substance, better known as "heliotropine," exhales a delicious odour of heliotrope, in the flowers of which plant it probably occurs accompanied by vanillin and other substances of an odorous character. It is much employed in perfumery, being especially useful on account of its low price.

Another very inexpensive and agreeable artificial perfume material is terpineol, which is manufactured from turpentine. This terpene alcohol has an odour resembling that of the lilac, and, being very resistant to the action of alkalies, is particularly adapted for scenting soaps and hair-washes, for which purpose it is extensively employed.

A newer synthetic product, which enables a more exact imitation of the rather sharp odour of lilac blossoms to be obtained, is phenyl-propionic aldehyde.

The odour of new-mown hay is very attractive and characteristic. Synthetic chemistry enables scents exhaling this fragrance to be easily prepared. The odour of new-mown hay is almost entirely due to coumarin, which occurs in *Anthoxanthum odoratum* (Sweet Vernal Grass) and other grasses. It is also the chief odorous principle of tonka beans, an extract of which was the chief material at one time used for making perfumes having odours of the new-mown hay type. Nowadays coumarin is prepared synthetically on a large scale, not only for the purpose of making these perfumes, but also for use with many other types of perfume materials as a fixative. It is often mixed with vanillin for the various purposes for which the latter substance is employed.

The synthesis of coumarin from phenol is particularly interesting. The phenol can first be converted into salicylic aldehyde, which yields coumarin by the action of acetic anhydride and sodium acetate. Salicylic aldehyde, itself, has some applications in perfumery. Its odour resembles that of Meadow Sweet, in which plant it actually occurs.

In Persia, and elsewhere in the East, the odour of the rose is held in the highest esteem, and many readers may be inclined to agree with those Easterns who consider otto of roses to afford the finest of all perfumes. Nevertheless, as was recognised years ago, the odour of the otto, obtained by steam distillation, falls short of that of the flower itself. For long the reason for this remained a mystery. But modern chemistry solved the riddle and supplied the means of remedying the defect. The cause is due to the fact that one of the constituents of the natural otto, phenyl ethyl alcohol, is rather soluble in water and is, therefore, washed out of the oil in the process of manufacture, being obtained almost entirely in the rose-water. Phenyl ethyl alcohol is now made synthetically by the reduction of esters of phenyl acetic acid; and by its aid very good synthetic rose ottos can be made. Other essential ingredients include the alcohols, geraniol and citronellol. The first, as already mentioned, is isolated from the cheap oils of citronella and palmarosa; the second is made by the reduction of citronellal, isolated from the first of these two oils.

Another product obtained from citronella also calls for mention on account of its importance. This is hydroxy-citronellal, a substance which provides a good example of those synthetic products which enable the fragrances of flowers to be very exactly imitated the extraction of whose natural ottos has not been found practicable. Hydroxy-citronellal is obtained by the hydration of citronellal, and is used for making scents exhaling odours resembling those of lily-of-the-valley (*muguet*), cyclamen, and lime-tree blossoms.

There are those who would give pride of place to the Sweet Violet amongst flowers of pleasant odour. Certainly scents exhaling the fragrance of this lovely little flower, which was so highly esteemed by the ancient Greeks, are exceedingly popular to-day, and can be quite cheaply obtained, thanks to synthetic chemistry. Except in the case of the most expensive, they contain no perfume material obtained from the violet itself, except, perhaps, a small proportion of the extract of violet leaves, added to give freshness to the odour.

Prior to the discovery of "synthetic violet," the preparation of satisfactory violet perfumes was a very difficult proposition, owing to the fact that the flowers contain only microscopic amounts of perfume material. The odour of the violet is a rare one in nature, orris-root and cassie (*Acacia Farnesiana*) being about the only available natural sources from which a tolerable substitute for the violet can be obtained.

An investigation by Tiemann and Krüger of the constitution of the oil of orris-root revealed the fact that its odour is almost entirely due to a ketone, which was christened "irone." These chemists prepared an isomer of this substance by condensing citral with acetone. By heating this substance with dilute sulphuric acid in the presence of a little glycerol, it was hoped that an isomeric change would take place resulting in the formation of irone. An isomeric change did take place; but the product was not irone, since synthesised by a different process. It was a substance, to be named "ionone," with an intense odour of violets, much nearer to that of the flower than the anticipated irone.

Nowadays, ionone is the basic material of all violet perfumes, and is one of the most important synthetic products in the art of perfumery. Actually it is not a chemical individual, but a mixture of two isomers. These can be separated. Their odours are not identical, and each has its several uses in the manufacture of various types of violet scents.

Another very important synthetic product employed in the confection of these and other perfumes is methyl heptine carbonate, which, used in minute quantities, gives that note of "freshness" so characteristic of the fragrance of Sweet Violets.

The list of synthetic materials used in perfumery could be extended indefinitely. But enough has been said to indicate how important a branch of chemistry the preparation of synthetic perfumes is. The average Englishman, perhaps, is apt to think of Perfumery as a rather frivolous subject. Actually, not only great technical skill and artistic sensibility are required for the confection of a fine perfume, but often years of scientific research have gone to the making of it. Every year brings forth new discoveries, more and more new substances, synthetically prepared, being added to the number of materials available for use by the perfume-artist. As the mass of material accumulates, it may be hoped that we approach nearer to the solution of the problem of the relation between odour and chemical constitution, and to that of the even more inscrutable puzzle of why certain classes of odours are pleasant, others unpleasant, to the olfactory nerves of human beings.

POPULAR SCIENCE

THE CUCKOO

By the Rev. E. A. ARMSTRONG, B.A.

The cuckoo, in his mantle grey,
  Cries on all day through lush tree-tops,
And verily—God shield me still!
  Well speeds my quill beneath the copse.
(*From the early Irish.*)

In the month of May it is a jolly thing to imitate the cuckoo's call and attract the bird to an adjacent tree where you may admire him and his call at your leisure. It is easily done. Clasp both hands together, making as large a space as possible between them, and blow between the thumb knuckles. There issues a mellow, rounded, deep-toned whistle which may be modified in pitch by lifting the palm of the right hand slightly and letting some of the air contained in the hollow of your hands escape. The result, if well done, is such a realistic imitation that even the wiliest cuckoo or field naturalist may be deceived. The notes may be modified to imitate the wood-pigeon and the brown owl. Wordsworth knew how it was done:

There was a Boy; ye knew him well, ye cliffs
And islands of Winander!—many a time,
At evening, when the earliest stars began
To move along the edges of the hills,
Rising or setting, would he stand alone,
Beneath the trees, or by the glimmering lake;
And there, with fingers interwoven, both hands
Pressed closely palm to palm and to his mouth
Uplifted, he, as through an instrument,
Blew mimic hootings to the silent owls,
That they might answer him.—And they would shout
Across the watery vale, and shout again,
Responsive to his call,—with quivering peals,
And long halloos, and screams and echoes loud
Redoubled and redoubled; concourse wild
Of jocund din!

I have often called a cuckoo in this way. Indeed, when out in the country of a May morning I can hardly resist having a

little fun with the bird which loves to make a dupe of others. The remarkable thing is the precision with which a cuckoo can locate the call. He will come from a great distance and alight quite close to your place of concealment. And sometimes he will proceed to reiterate his call so endlessly that you cry, "Hold! Enough!" and frighten the noisy creature away. If you are in a wood the bird will come dashing between the tree-trunks like a sparrow-hawk at an amazing speed.

I remember particularly one calm, bright morning in May when I went early to bathe in a little tributary of the Cam. Those were great days when we would pull on running kit and, seizing a towel, dash out into the song-laden air, sharp and fragrant, run along the stream side to the consternation of the water-voles, and plunge into the still pool.

> Bliss it was those morns to be alive,
> But to be young was very heaven.

After the dip one's senses were at their alertest and tuned to the highest appreciation of the fresh, fair world around. And while drying there was leisure to listen and enjoy. Sitting on the bank, I imitated a cuckoo, and, although I was quite in the open, he came flying in a wide circle overhead and perched, calling, on the nearest tree. Soon another joined up, and, continuing to mimic the birds, I made them accompany me back to college, and for as long as I had time to spare I had two frantic cuckoos flying swiftly back and forth before my study window. One of these birds which I heard calling at very close quarters made a clicking noise between the calls and erected its tail almost over its back in its excitement. The female is now known not to call "cuckoo"; she has a reeling call not altogether unlike the ripple notes of the dabchick.

The cuckoo is a strange bird and one never knows what strange things he—or more often she—may be found doing. I have seen the egg—a pale blue specimen of what would be called the "hedge-sparrow type"—in a linnet's nest on a turf bank on Texel Island, and I once knew a cuckoo lay twice in the same hedge-sparrow's nest. The second egg was laid in the ramshackle remains of the nest, which had been crushed out of all semblance by the sprawling young cuckoo which had fledged some days previously. I have no doubt, as the eggs resembled each other, that they were laid by the same cuckoo. Probably she was hard put to it and deposited the egg in the derelict nest *faute de mieux*. This observation raises interesting problems. Did the cuckoo remember the nest or had she kept it under observation and so knew it to be a nest? Otherwise she might have laid the egg in any other bunch of grass and twigs. I have never noticed a record of this before,

but somewhat similar happenings have been recorded. J. H. Owen has known a cuckoo's egg to be laid in a linnet's nest which had been robbed, and another in a whitethroat's nest only half-built,[1] and R. Ware found a cuckoo's egg in a greenfinch's nest from which the eggs had been removed a week before.[2] J. F. Green reports a cuckoo's egg laid in a hedge-sparrow's nest containing one egg which had been deserted three weeks,[3] and E. G. B. Meade-Waldo records finding a cuckoo's egg in a nest from which the young wood-warblers had flown four days previously.[4] In addition, there are many records of cuckoos laying in unsuitable nests such as those of the kestrel, green woodpecker, dabchick, and red grouse.[5] All this goes to show that the cuckoo is a bird whose vagaries may not be anticipated. It is dangerous, as some have done, to generalise about such a peculiar creature.

There are some who maintain that the cuckoo always deposits her egg directly into the nest. On the other hand, there are convinced upholders of the doctrine that the bird sometimes carries the egg in her bill or throat and so places it in the nest. There can be no doubt that the bird sometimes lays directly in the nest, as the process has been closely observed and even recorded by the kinematograph.[6] Perhaps the most remarkable observation on record in this connection is that of Father Horne, in a letter to Mr. B. W. Tucker, published in *British Birds*. He found a cuckoo laying in a nest on the sill of a staircase window. " I had a perfect view of the bird's body, which appeared to elongate and become pointed. This point was bent downwards, the bird standing above the nest and clear of it, and it almost seemed as though it felt about for the edge of the nest, for it moved from side to side. As soon as it touched the side of the nest, the egg began to be extruded, and when it finally left the bird it rolled to its place by the side of the other two eggs."[7] The cuckoo settled on the nest for about a minute, and then flew away without looking at the nest.

But does the cuckoo sometimes lay the egg and carry it in her throat and regurgitate it into the nest? This belief has tradition behind it, though it may be that the sight of the cuckoo, as she may often be seen, with an egg in her bill, pursued by a pipit or some other bird, has given rise to the belief that the egg

[1] *British Birds Magazine*, vol. 15, p. 210; also Kirkman's *British Birds*, vol. 2, p. 469; also *Zoologist* (1915), p. 355. E. Butterfield gives two instances of a cuckoo's egg being buried through completion of the nest.

[2] *British Birds Magazine*, vol. 7, p. 264.

[3] *Ibid.*, vol. 1, p. 326.

[4] *Ibid.*, vol. 14, p. 263.

[5] Kirkman, *op. cit.*, vol. 2, p. 492.

[6] Cf. *The Cuckoo's Secret*, by E. Chance.

[7] Vol. 17, p. 215.

was the cuckoo's and not, as it is now known to be, an egg which the cuckoo had stolen.[1] Cuckoo's eggs have been found in covered nests into which they could only have been introduced, it is maintained, by means of the bird's beak.[2] To quote only one recent record: Miss E. L. Turner found a young cuckoo in a wagtail's nest in a flower-pot containing a plant of *Hoya carnosa*, the tendrils of which were very strong and close together. "It was obvious," she writes, "that the adult cuckoo must have clung to the pot, and inserted the egg into the nest with her bill."[3] This testimony is particularly valuable as Miss Turner has photographed the cuckoo laying directly—sitting on the nest.

My friend Mr. J. Beetham, of Doncaster, made an interesting observation which has not hitherto been published. In a letter to me he writes: "As far as I can remember it was the spring of 1906. I was out before breakfast and had found a wheatear's nest with eggs in an old rabbit burrow on the side of the sandy bank facing the railway at Rossington. I had passed the nest and had been examining what I thought might be a jay's nest in some small, bushy trees, and, when coming back, suddenly spotted a cuckoo flying low over the ground with something whiteish in its beak. It evidently did not see me, for it flew straight up to the wheatear's nest, settled a moment, and flew away in the opposite direction. I went straight to the nest and there was the cuckoo's egg—which I have now in my collection. I always remember this because it was the first cuckoo's egg I got and I know for certain the egg was not there when I examined the nest just previously."

Perhaps the best evidence in this matter is that of Messrs. P. F. Bunyard and G. J. Scholey, who placed their records before the British Ornithologists' Club not long ago.[4] Mr. Bunyard had waited nearly an hour on June 28th, 1927, in a hiding tent eight feet away from a reed-warbler's nest, when, at 5·50 p.m., he saw the cuckoo arrive at the nest with no egg in her bill. She disappeared, but Mr. Bunyard heard her "wh–ar" and "caught sight of her on my right, gently and slowly threading her way through the reeds towards the warbler's nest. Having safely reached it, she clung to the side, *with her shoulders level with the top of the nest*. Again there was no egg in her beak. She then thrust her head and neck into the nest, slightly withdrew, repeated the operation, and on

[1] Aristotle, *Hist. Anim.*, vol. 7. "The cuckoo lays in a nest which she has not herself built, but of some smaller bird, eating the eggs she finds there and leaving her own."

[2] Dr. Jenner found a cuckoo's egg in a pied wagtail's nest in a hole under the eaves of a cottage.

[3] *Broadland Birds*, p. 146.

[4] Bunyard, P. F., *Bull. Brit. Orn. Club*, vol. 48, pp. 31–2, 1928.

her withdrawing a second time I saw she had something in her beak. I gave her time to get away . . . then left the 'hide' and made an immediate inspection of the nest, which I found, to my great satisfaction, now contained the cuckoo's egg and one warbler's (there had previously been two). The actual time she spent at the nest was not more than eight seconds. . . . She neither went on to or over the nest. Obviously the only possible way her egg could have reached its destination was by regurgitation." [1]

The problem of how the cuckoo lays her egg is no new one; it is discussed in an interesting little book called *The Architecture of Birds*, published in 1831. The author quotes a passage from Vaillant's *Oiseaux d'Afrique* which is sufficiently to the point to reproduce: "I always entertained the hope that I should one day surprise a female didric (Gilded Cuckoo, *Cuculus auratus*, Gmelin.) in the act of depositing its egg in the nest of another bird, but, having been disappointed in this respect, I began to imagine that my ignorance on this point would never be removed, when one day, having killed a female of this species and wishing to introduce into its throat a hempen stopper, according to my custom after bringing down a bird, in order to prevent the blood from staining the plumage, I was not a little surprised, on opening its bill for this purpose, to find in its throat an entire egg, which I knew immediately, from its form, size, and beautiful whiteness, to belong to the didric. Delighted at length, after so many useless efforts, at having obtained a confirmation of my suspicions, I loudly called my faithful Klaas, who was only a few paces distant from me, to whom I imparted my discovery with much pleasure, as he had used his best exertions to second my views. Klaas, on seeing the egg in the bird's gullet, told me that, after killing female cuckoos, he had frequently observed a newly-broken egg lying upon the ground near where they had fallen, which, he supposed, they had dropped in their fall, from being at that moment ready to lay. I recollect very well that when this good Hottentot brought me the fruits of his sports, he frequently remarked, as he pointed to the cuckoo, 'This one laid her egg as she fell from the tree.' Although I was convinced, from this circumstance, that the female cuckoo deposits her egg in the nests of other birds by conveying it in her beak, I was very desirous to collect what facts I could upon the subject. Klaas and I therefore began to shoot as many cuckoos as we could meet with, which accounts for the great numbers of this species we procured. (130 females). However, among all the specimens

[1] Cf. also *British Birds Magazine*, vol. 15, pp. 130–86. G. J. Scholey, in this article, writes of the cuckoo "that she carried it (the egg) to the nest in her bill I have no doubt."

there occurred only one instance similar to that which I have just mentioned—that, namely, of a second female who was transporting her egg in her mouth, like the former." [1]

Perhaps those are wisest who withhold judgment until irresistible evidence is forthcoming. As old Sir Thomas Ware wrote, after discussing whether or not the Barnacle goose is generated from shell-fish: "In a matter that may deserve further search, I determin nothing." In spite of the apparently purposive actions of birds we are learning that their behaviour is less due to intelligence and emotion than we would at first imagine.[2] Instinct and reflex action account for a great deal in animal (and, indeed, also human) behaviour. Are we to believe that a female cuckoo can come so close to reasonable behaviour as would be required of her in depositing her egg in a covered nest with her bill? Think what it involves: inspection of the nest and realisation that the egg cannot be directly deposited; extrusion of the egg on the ground and recognition of it as what it is; inhibition of the impulse to crush it; retention until a suitable opportunity occurs, and the feat of regurgitating it while clinging in an awkward position. A formidable process, and one which is so seldom called for that it cannot be called instinctive. Here, indeed, is the crux of the matter. There are birds and insects which go through as wonderful or even more complicated series of actions; but intelligence is not involved, for the doing of one action brings into play the impulse to perform the next, and so on to the end of the series. But, break the series ever so slightly, and the creature is helpless.[3] It is easy—the easiest thing in the world—for an organism to follow the path that has been beaten smooth by countless generations; but to blaze a new trail, be it over ever so short a section of the route, is to triumph over immense difficulties. It has been done, or we would not have the variety of

[1] The cowbird *Molothrus ater* lays in hole-nests, and is therefore believed, at least occasionally, to lay the egg outside the nest and carry it in its bill. Cf. *British Bird Book*, vol. 2, p. 472, and C. Bendine, *Life Histories of N. American Birds*, p. 436.

[2] The French psychologist Giart has proved that the apparent maternal affection of the hen is dependent upon, if, indeed, it is not identical with, the occurrence of local inflammation. The hen sits on the eggs to obtain relief and the most unbroody hen may be converted into a perfect sitter by the simple expedient of irritating the appropriate places with pepper!

[3] A bird victimised by a cuckoo will allow its own eggs and even wriggling young to perish outside the nest without taking the least interest. Photographs have been taken of the egg and young bird being ejected from underneath the sitting bird. Once outside the nest the egg had no meaning for the bird. Whitman, however, found that if the eggs of the dovecot pigeon were moved two inches away from the edge of the nest she would usually try to retrieve one or both; in similar circumstances the ring-dove would be satisfied with one, while the passenger pigeon (now extinct) soon knew there was something wrong—and flew away.

behaviour which the world exhibits in its many organisms to-day. But for one slight divergence in behaviour to become instinctive through the behaviour of many generations is one thing; for a whole string of purposive responses to a new situation to come into being and be retained in the inheritance of the species for the rare occasions on which it may be required is quite another. Prof. Köhler has recently shown us the limitations in the intelligence of apes; the greatest height of intelligence they reached—and it *is* a great height—was when a chimpanzee whittled a stick with its teeth to make it fit into another stick and so enable it to reach a banana. Is it likely that where a longer chain of inference is involved a cuckoo would be able to deal with the situation? On the other hand, if it is maintained that the laying of the egg in the nest by means of the bill when occasion requires is due to instinct, what sort of mysterious instinct is it which may be called into play so seldom? To sum up the discussion: if a cuckoo can modify her behaviour to deposit an egg with her bill when occasion demands, no present-day theories of the nature of instinct or intelligence are adequate to meet the case. The problem, it may be seen, is not without considerable importance for modern psychology.

Although the cuckoo still offers many interesting problems not a few errors in regard to its life-history have recently been dispelled. So lately as the middle of the last century there were some who held that the cuckoo sometimes laid in a nest of her own building on the ground. This error apparently arose through confusion of the young cuckoo with the young of the nightjar, although the nightjar usually lays its eggs on the bare ground.[1] It was also believed until recently that sometimes the young cuckoo devoured its own foster parents—a mistake repeated from the time of Pliny. Instances have indeed been put on record of the young monster killing a foster parent by swallowing the poor dupe's head.[2] The relevant passage as translated in Holland's *Plinie* is so quaint that the reader may like to have it by him: "The titling therefore, that sitteth, being thus deceived, hatcheth the egge and bringeth up the chick of another bird. And this young cuckoo being greedy by kind, beguiling the other young birds and intercepting the nest from them, groweth hereby fat and fair-liking, whereby it comes into special grace and favour with the dam of the rest and nurse to it. She joyeth to see so goodly a bird toward, and wonders at herselfe that she hath hatched and reared so trim a chick. The rest, which are her owne, indeed,

[1] Mr. O. G. Pike, in *Adventures Among Birds*, has published a photograph of a nightjar's nest.

[2] Klein, *Ordo Avium*, p. 29.

she sets no store by, as if they were changelings, but in regard of that one counteth them all misbegotten, yea, and suffereth them to be eaten and devoured of the other even before her face; and this she doth so long, until the young cuckoo, being once fledged and readie to flie abroad, is so bold as to seize on the old titling and to eat her up that hatched her."

In order to resolve some of the mysteries of the young cuckoo's existence I made a series of notes on the development of a bird in a nest not too far away to prevent my keeping it under daily observation. I looked up what had been already written on the subject and made my observations specially to obtain information on certain points which seemed to me to be in need of further investigation. The excerpts from my notes which follow are recorded as the answers I received to questions which I put to myself, such as:

At what age can a young cuckoo throw an egg or young bird out of the nest? Is it possible for it, when only a few hours old, to perform the prodigious feat of carrying an egg weighing almost as much as itself up the side of a nest and throwing it overboard?

What stimulates the young cuckoo to such a strange and difficult endeavour?

How long does the "tendency to eject" continue? and when is it at its maximum intensity?

When does the hollow in the young bird's back, so well adapted for the purpose of evicting nest-mates, appear and disappear?

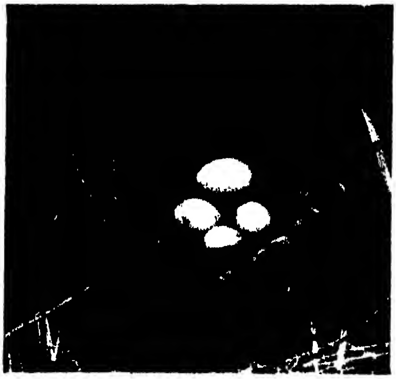
How exactly does the eviction take place?

Has the young cuckoo sufficient energy to eject several eggs or young birds in quick succession?

At what stage does the gape assume its bright red colour and when does the skin become black?

My observations are as follows:

Monday, 4th July, 1927.—At 9 a.m. I visited the hedge-sparrow's nest. The previous evening, about 8–30 p.m., a child saw the egg, described as being "mottled brown," lying in the nest. Now the young cuckoo has hatched. It is moving restlessly and continually opening its mouth, the inside of which is yellow, but not nearly so deep in colour as it will be. Its wings are prominent, and are more like arms than wings. The garden boy says he saw a large bird on the nest several days ago and found two hedge-sparrow's eggs thrown out, one resting among the twigs near the nest. A quarter of an hour later I again visit the nest. One of the two hedge-sparrow's eggs has chipped in a circle round the middle and the young bird is wriggling inside. The young cuckoo is naked and blind.

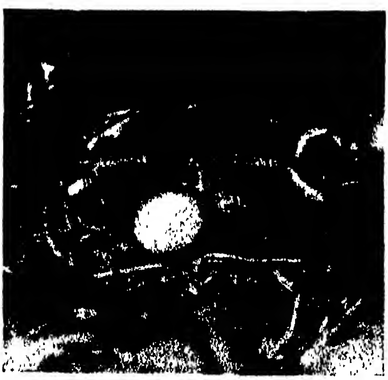


Cuckoo's egg in linnet's nest.

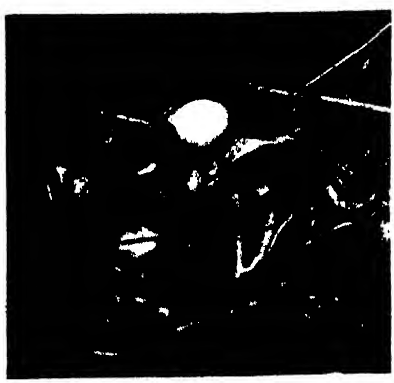
The egg irritates the young cuckoo

—it gets the egg on its back

—pushes it up the side of the nest



–and heaves it overboard.



Another view of the egg being ejected.

Copyright photos, E. A. Armstrong.

The skin over the eyes is dark. Visited the nest again in the evening at 6 o'clock. In the meantime, *i.e.* between 11 a.m. and 6 p.m., the egg has been thrown out. There is scarcely any hollow in the cuckoo's back. Skin pale flesh colour.

Tuesday, 5th July.—About 9 a.m. visited the nest. The other egg has been thrown out. The young hedge-sparrow is getting badly mauled by the arms of the wriggling cuckoo. Put the egg on the cuckoo's back, but even then it made no definite effort to throw it out. I kept the nest under observation until midday. The cuckoo is uttering a very faint squeak. Replaced the egg.

Wednesday, 6th July.—Visited the nest at 8–30 p.m. Young hedge-sparrow has completely disappeared. If pushed overboard it may have fallen into the hedge, where it would be impossible to see it. The egg has been pushed out and is resting on the side of the nest. Placed egg on the cuckoo's back, which is much hollower than it was. Immediately it set to work to push it out. It heaved the egg against the side of the nest, pressing with its beak on the nest bottom and gripping the sides with its feet, which are well developed. It worked haphazard round the nest with its wings flapping until it came where the side was not so steep, where it pushed the egg right out—bill still held against the floor of the nest, neck rigid, legs wide apart and stumps of wings waving. I thought it remarkable that even when the egg had been lodged on the side of the nest, and was no longer touching the cuckoo, it would make efforts to push it still farther—holding itself right out of the nest on what might be called the sill and giving, or trying to give, according as the egg had or had not rolled away, a further push or pushes. (Neither on this nor any other occasion did I get the impression that the cuckoo was feeling about with its wings; rather that it used them automatically to help it to keep its balance.) The bird remains clinging for as long as about 20–30 seconds on the nest-sill in this final effort. Gradually its efforts subside and it drops back into the nest and lies as if exhausted. The whole process is over in about one minute. (I think no egg was left in the nest.)

Thursday, 7th July.—10–30 a.m. The cuckoo is not very lively. It made no effort to throw out the egg, even when I put it on its back. (At first I was afraid that a heavy thunderstorm in the night had caused the hedge-sparrow to desert.) The skin is now black and quills are beginning to sprout on the wings, which have a scaly appearance reminiscent of woodlice. The inside of the bill is much deeper in colour and might be called orange. The young bird already shows the cringing wriggle or shuddering movement so noticeable later when it is crying for food.

I returned about 3 p.m. Put the egg on cuckoo's back and immediately it pushed it against the side of the nest where before it had not been able to get it out, and in a few seconds it was ejected. The cuckoo balanced itself on the sill of the nest and gave a final push which knocked the egg from where it had lodged on the rim of the nest off it into the twigs. I had not expected the bird to be so lively and expeditious, and had not set up the camera.

Set up camera and tried again. The cuckoo promptly wedged the egg, which I had returned to the nest, against the side and heaved it out as before. I took two photographs—the first as the bird rested a moment with the egg up the side of the nest, the second when the egg was on the nest-sill and the chick was struggling with head down and flapping wings to give the final shove. I had to work quickly in order to change to the other side of the slide in time for the second photograph. I left the nest in order to change plates and returned about 4–20 p.m., when I took another picture of the egg being ejected, but this time from another angle. I was just too slow in changing plates to get a second photo. After this the cuckoo would not make a serious effort to throw out the egg again. It was perhaps exhausted. Later it moved about in a curious, uneasy way, sometimes throwing itself backwards, but not quite so as to fall on its back. The bird's querulous squeak is much more pronounced now.

The strenuous jerking movements when the egg is right out on the nest's rim are remarkable, as one would expect a collapse after the egg has ceased to touch the young cuckoo. Such is not the case. After these final efforts the ugly black chick droops and falls back into the nest.

Friday, 8th July.—3–30 p.m. "Cringing" is more noticeable. The quills have grown considerably. The garden boy has been experimenting with the bird and says that it has thrown out the egg three times already. As soon as I put it in it was ejected. The egg fell into the undergrowth and got lost. I looked about and found an apple nearly an inch in diameter. The young cuckoo reacted in exactly the same way. I have no doubt it would have thrown out this huge burden, but I thought it might become too exhausted and took it off its back. But it seems to recover very quickly. In the ejection process the creature usually pushes sideways against the object and then, with wings stretched back and flapping vigorously and beak bent down against the bottom of the nest, pushes backwards.

Saturday, 9th July.—2–30 p.m. Quills grown and wings looking more like wings. The hedge-sparrow was brooding and sheltering cuckoo from the hot sun. I put in the small

apple and the chick hoisted it up as before. It did not get it out, but perhaps I did not let it try long enough. The garden boy told me that he had placed the apple in the nest earlier that day and that the cuckoo had thrown it out, getting up on the rim of the nest and finally pushing it right off. The creature is a black, ugly thing, always opening its bill, which is orange inside, and now cringing when it does so—quivering its attempt at wings. Its back is now rather a platform than a hollow.

Sunday, 10th July.—10–30 a.m. Put in small apple and the cuckoo set to work at once to push it out.

Monday, 11th July.—12–15 p.m. Put pebble on the cuckoo's back, but it made no effort to eject it then nor when it was put at its side. Its eyes are open to-day for the first time. Cheeping and cringing. Hardly any hollow in back. Wings well quilled and rounded. The inside of mouth is now definitely orange in colour.

Tuesday, 12th July.—3–15 p.m. Put small apple on the cuckoo's back. It made no effort to eject, but rather tried to bite it. It allowed it to lie by its side. The creature occupies most of the nest now. Quills in wings and tail well sprouted. Back bare and black. Eyes not yet fully open.

Wednesday, 13th July.—Gape reddish. Quills along back sprouting.

Thursday, 14th July.—Did not visit the nest.

Friday, 15th July.—The cuckoo is dead, badly bitten about the head by some small animal.

How far have our queries been answered? As regards the first question the evidence shows that an egg had been removed from the nest within twenty-four hours of the hatching of the cuckoo's egg. It could not be proved that the young cuckoo and not the old bird was responsible, but the circumstances leave no doubt in my mind that the deed was done by the newly hatched cuckoo. This observation is only confirmation of a discovery made as long ago as 1787 by no less a person than Dr. Jenner, the discoverer of vaccination, and recorded by him in the *Philosophical Transactions of the Royal Society of London*, vol. lxxviii, 219–37. "On the 18th of June, 1787," he says, "I examined the nest of a hedge-sparrow, which then contained a cuckoo and three hedge-sparrow's eggs. On inspecting it the day following, the bird had hatched; but the nest then contained only a young cuckoo and one hedge-sparrow. The nest was placed so near the extremity of a hedge that I could distinctly see what was going forward in it; and, to my great astonishment, I saw the young cuckoo, though so lately hatched, in the act of turning out the young hedge-sparrow." To any-

one who has not actually witnessed the paroxysm of energy which seizes the newly-hatched cuckoo it must seem incredible that a thing so small and flabby could hoist overboard the relatively gigantic burden of a hedge-sparrow's egg from the deep cup of the nest. "That, at the age of two days, it (the young cuckoo) should be able to throw out what to it must be a very heavy weight, is astonishing enough," writes Mr. F. B. Kirkman; "that it should do so a few hours after birth would border on the miraculous."[1] The miracle, if we like to call it so, does happen, and a most uncanny thing it is to see the soft, limp, sprawling squab suddenly suffused with this mysterious access of power. But as one watches the hideous creature, apparently so full of fiendish purpose and endowed with supernatural strength, it is to feel rather that the very powers of darkness are at work, incarnate in that wretched, murderous morsel of life.

The behaviour of the young cuckoo reminded me of a story by a well-known war correspondent in which the hero manages to escape from the dungeon into which his Tibetan captors have flung him by chewing some hashish-like drug which gives him momentarily abnormal strength and enables him to raise a heavy flag-stone in the floor. As if it had its purpose clearly in view the creature braces its muscles, straddles its legs, balances the egg or young bird horizontally on its hollow back and grimly and methodically hoists it to the rim of the nest and actually climbs right out to give the final push which sends its victim to destruction—I had almost written perdition!

But, of course, to attribute purpose or malicious intent to the young cuckoo is sheer nonsense. We must not project our modes of thought into the behaviour of a chick a few hours old as if it could "Look before and after, and sigh for what is not." The cuckoo has no notion as to the consequences of its behaviour; it has no idea what it is doing. It goes through a series of reactions to its environment which are as natural to it, as automatic and conditioned, as to sit on its own eggs is natural to the hedge-sparrow. Virtue and vice are terms applicable only to the behaviour of mankind, and it is as unreasonable to praise a broody hen as to blame a wriggling cuckoo squab.

The best explanation of the cuckoo's behaviour is that given by W. H. Hudson in his *Hampshire Days*, on page 16. He attributes the creature's efforts to the irritation caused by the contact of any foreign object with the sensitive sides and back. Let us have his own words.

"The process in each case was as follows: The pressure of the egg against the cuckoo's side, as I have said, was a constant irritation; but the irritability varied in degree in

[1] *The British Bird Book*, vol. ii, p. 483.

different parts of the body. On the under parts it scarcely existed ; its seat was chiefly on the upper surface, beginning at the sides and increasing towards the centre, and was greatest in the hollow of the back. When, in moving, the egg got pushed up to the upper edge of his side he would begin to fidget more and more and this would cause it to move round, and so to increase the irritation by touching and pressing against other parts. When all the bird's efforts to get away from the object had only made matters worse, he would cease wriggling, and squat down lower and lower in the bottom of the nest, and the egg, forced up, would finally roll right into the cavity in his back, the most irritable part of all. Whenever this occurred a sudden change that was like a fit would seize the bird ; he would stiffen, rise in the nest, his flabby muscles made rigid, and stand erect, his back in a horizontal position, the head hanging down, the little naked wings held up over the back," and so the work of eviction would proceed. In connection with the ejection of a young robin it was only when its beak came in contact with the cuckoo's back that the latter shrank down into the nest, " as if hot needles had pricked him, as far as possible from the side where the cuckoo was lying against him, and this movement, of course, brought the robin more and more over him, until he was thrown right upon the cuckoo's back. Instantly the rigid fit came on, and up rose the cuckoo, as if the robin weighed no more than a feather on him ; and away backward he went, right up the nest, without a pause, and standing actually on the rim, jerked his body, causing the robin to fall off clean away from the nest."

There are several matters in this account which require further elucidation. The degree of sensitiveness of the cuckoo's back seems to vary considerably at different times. How is it that the bird I kept under observation did not show any signs of irritation on Tuesday and on Thursday morning? Also, I found that pricking its back lightly did not annoy the cuckoo as I expected it to do. Direct pressure by some relatively heavy object was much more effective. Another point which would be worth inquiring into is whether the irritability disappears earlier when there is no egg or bird in the nest to keep up the irritation. In the case of the cuckoo of which the early life-history is given in the preceding pages, and which was supplied with objects to eject each day, the irritability persisted until the seventh day after hatching, whereas a bird observed by Mr. J. H. Gurney lost the desire to evict when it was three days and ten hours old.[1] My notes show that although evictions may take place before the cavity in the young bird's back is pronounced and after it has almost disappeared,

[1] *Zoologist*, 1905, p. 164.

there is rough correspondence between the impulse to eject and the persistence of this adaptation for that purpose. Mr. Gurney reports that the depression in the back of the cuckoo mentioned was no longer perceptible when the bird was four days and four hours old. Dr. Jenner records that the depression disappears about the twelfth day and the impulse to eject "begins to decline from the time the bird is two or three till it is about twelve days old, when, as far as I have hitherto seen, it ceases. Indeed the disposition for throwing out the eggs appears to cease a few days sooner; for I have frequently seen the young cuckoo, after it had been hatched nine or ten days, remove a nestling that had been placed in the nest with it, when it suffered an egg put there at the same time to remain unmolested." That the desire to evict should persist until the tenth or eleventh day seems to me, in view of my own observations, very remarkable. In the case of my bird it is interesting to note that the impulse disappeared the day on which its eyes opened.

Although Jenner's account still remains as one of the best descriptions of these strange proceedings, he speaks of the purposeful employment of the wings in a way which I do not think is justified. He describes how the bird he was watching climbed to the rim of the nest and threw the young hedge-sparrow over, and proceeds: "It remained in this situation for a short time, feeling about with the extremities of its wings, as if to be convinced whether the business was properly executed, and then dropped into the nest again. With these, the extremities of its wings, I have often seen it examine, as it were, an egg and nestling before it began its operations; and the nice sensibilities which these parts seem to possess, seemed sufficiently to compensate the want of sight, which as yet it was destitute of." Dr. Jenner is here falling into the error of which we have already spoken, the "anthropomorphic fallacy" of attributing human modes of thought and feeling to other creatures. Goethe said with justice, "Man never knows how anthropomorphic he is." The motto is an excellent one for all students of bird behaviour. As I have recorded in my notes, the movements of the cuckoo's wings seem to be as significant as the arm movements of a man walking the tight-rope and no more. They are automatic, and help the bird to keep its balance, and they may, by chance, help it to haul itself to the rim of the nest, or even, as they flap wildly during these struggles, knock the egg overboard, but they are not used as "feelers," as Dr. Jenner would suggest.

My observations bear out those of Dr. Jenner, W. H. Hudson, and others that the cuckoo has a marvellous store of energy, such that with very short respites it may go on ejecting,

or trying to eject, eggs or young birds, or indeed other objects, one after another.[1]

For several days after the young bird has left the nest the unfortunate foster-parents may be seen feeding it assiduously, sometimes perching on the giant changeling's head in order to insert the food into its blazing red gape. Their pains are often rewarded by a vicious peck, which they are usually agile enough to avoid. "Chiz, chiz, chiz," goes the hungry, peevish cuckoo, and its foster-parents work like slaves while their own eggs or young rot beside the nest.

The majority of the adult cuckoo population depart for their African winter quarters early in July. They usually go singly, but occasionally small parties are seen on migration. The young birds for the most part leave later and find their way alone, so that they leave our shores as much slaves to mysterious forces as when, newly hatched, they ejected their nest-mates. The life-story of a cuckoo is like the theme of a Hardy novel: a record of inscrutable forces playing with their blind, impotent victims. The great writer, so exquisitely sensitive to Nature, carried his outlook on her non-moral realm into the sphere of human activity. And is not this the explanation of his sad view of man and of life? He set the forces of Nature on Olympus and personified them as the President of the Immortals; but had he not forgotten that in knowing his own impotence man rises superior to blind forces, becomes Captain of his Soul in spite of the bludgeonings of fate, and inherits a wider destiny where blind force is not supreme and Righteousness, after all, is immortal?

In *A Philosopher with Nature*, Benjamin Kidd gives a vivid description of the power of the instincts which work to cause a bird to migrate and so preserve its life. Slave as it is, its bondage is to a taskmaster whose highest interest is the welfare of the creature and the race. He tells how a young cuckoo, kept captive after the usual time of migration, used to sit on a stationery-case in his study under a green-shaded reading-lamp, and quiver its wings rapidly. In the early stages the bird responded when he spoke to it, "but in time it ceased to do this, and became lost in a kind of trance with eyes open and wings ceaselessly moving. The bird became, as it were, locked in the passion of that sense by which the movement of flying was thus simulated."[2]

"All things go out in mystery," said an ancient sage, and is it not better so? What would life be but for the enchanted lands which lie beyond the distant mountain ranges, the

[1] *British Birds Magazine*, vol. 8, p. 51. J. H. Owen records a case of a cuckoo ejecting two sedge-warbler's eggs at the same time.
[2] P. 158.

mysteries which encompass us, perplex us, and lead us onward? But I am glad to know, as I sit here on a grey winter's day reflecting how the consideration of a wriggling young cuckoo may raise the greatest problems of philosophy, that when next I hear that wandering voice and the leaves are green again my heart will leap with joy, the world will grow young, and in spirit I shall sit once more in the dewy warmth of the morning sun and hear the cuckoos calling by the smooth waters of the Cam.

> And I can listen to thee yet;
> Can lie upon the plain
> And listen, till I do beget
> That golden time again.
>
> O blessed Bird! the earth we pace
> Again appears to be
> An unsubstantial fairy place;
> That is fit home for Thee!

ESSAY

HOW TO SECURE GREATER EFFICIENCY IN MILL INDUSTRY[1] **(Sir Malcolm Watson, M.D., LL.D., Principal, Malaria Department, Ross Institute).**

SINCE coming to Bombay I have heard on all sides of the disastrous effects of the strike, of the poverty of the worker, of the heavy loss of capital. In your issue of December 4 in a report of a sitting of the Bombay Mill Strike Inquiry Committee, a spokesman of the Millowners' Association compared the costs in Bombay and Japan and explained how the mill-owners hoped to increase the wage of the operative by asking him to attend more machines and improving methods of working. Another witness said that the operatives were not doing more than three or four hours' actual work a day, and that in many departments the worker could do double the work without being unduly taxed. Another witness also emphasised the need of decreased cost of production in view of Japanese efficiency. I am told the more extreme workers insist not only that they should receive higher wages, but that the Communists are out to do everything in their power to bankrupt the millowner and gain control of the mills for themselves. They claim for the worker the whole profit. On the other hand the extreme millowner pleads that the workman gets enough now; that he cannot afford to give him more; that this long strike has reduced him to the verge of bankruptcy; that if the worker gets more he will work less; and that, if the cost of production is not reduced, bankruptcy is inevitable.

With the extremists I am not concerned at the moment. I wish, however, to write of one factor which influences efficiency and the cost of production, a factor which would still remain if every Communist were eliminated from the country, and if every millowner saw eye to eye with his worker. I refer to the malaria problem of Bombay. Major Covell brings out in his report very strikingly the incidence of malaria throughout Bombay. He says: "A dense population, composed largely of the lowest classes, live in the mill areas, their dwellings

[1] Reprinted from *The Times of India*, December 7, 1928, by kind permission of Editor.

frequently abutting on the mill compound. More favourable conditions for the propagation of malaria can scarcely be imagined." "In large parts of Worli and Parel sections malarial conditions approach those encountered in hyper-endemic areas in other parts of India." By this he means that the most intense malaria prevails. Under these conditions efficient labour is quite impossible, and I am not surprised to hear that living under these conditions the Bombay operative cannot compete with the Japanese, who works not only in a cooler climate, but in one which is free from malaria.

Conditions such as Major Covell describes in Bombay were prevalent throughout Malaya some thirty years ago. I have had an opportunity of studying malaria among workers, not only on the rubber and coffee estates and tin-mines of Malaya, but on the tobacco and rubber estates of Java and Sumatra, and among labour forces in Panama, British Guiana and Assam. For many years I was in direct charge of thousands of labourers who were suffering from the most intense malaria. I was also directly associated with the great anti-malaria work carried out in the city of Singapore, and with the Malaria Advisory Board of the Federated Malay States. In a recent address to the Federal Council of the Federated Malay States H. E. Sir Hugh Clifford stated that "in his long experience the Malay Peninsula as it was forty years ago, was by far the most malarious tropical country of which he had any personal experience." He had been greatly struck by the efficient manner in which the malarial problem had been dealt with. He said: "It must be quite obvious to all who are acquainted with the conditions that prevailed in the Malay Peninsula during the concluding decades of the nineteenth, and the opening years of the present century, that developments, such as the rubber industry, which in so short a space of time has spread over so enormous an area, would have been totally impossible unless the danger of malarial infection had first been successfully combated. The record of the American engineers and physicians, who, by exercising a despotic and unresisted authority over the population of a very limited area on either bank of the Panama Canal, rendered that notorious death-trap a perfectly healthy locality for a very large labour force, sinks, in my judgment, into complete insignificance when compared with the practical achievements which have been effected during the last decade and a half in the Malay Peninsula. . . . In all my long experience of tropical countries, I have never yet seen such close co-operation between the medical experts and the employers of labour as that which has, happily, been established in British Malaya." Does such close co-operation exist in Bombay

between the experts, the Corporation and other authorities? The answer is unfortunately, no, and the result is malaria.

One of the most striking things we found when we first obtained control of malaria was that while we reduced the number of deaths from fever, we simultaneously reduced the number of deaths from other diseases by an equal amount. Diarrhœa and dysentery, pneumonia and high infantile mortality are always associated with malaria epidemics, but are rarely recognised as being due to the disease. As soon as malaria on an estate has been abolished, the dysentery ward can be shut; and when in Singapore a thousand lives had been saved from fever, another thousand had been saved from diarrhœa and dysentery and other diseases. When we bear this in mind we cannot help thinking that much of the disease that exists in Bombay is not attributed to its true cause, namely, malaria. But that there must be a large number can only be too true from what Major Covell describes in his report. It is invariably found that when malaria is reduced, sickness and death from other diseases are correspondingly reduced.

Examples of what malaria was in Malaya may be given. In 1901 Sir Frank Swettenham ordered the closing of Port Swettenham two months after it was opened, as the Railway and Customs staffs were mainly in hospital; the loading coolies had absconded; crews refused to sign on ships which called at the port. For twenty years before 1911 the city of Singapore was swept every year by a great wave of malaria. For years the rubber estates had their labour forces so completely disorganised, that heavy loss was incurred by the jungle encroaching on the land, hundreds of acres were abandoned, and even entire estates were closed down. The Government and the planters faced the problem with firmness, and gradually overcame the disease. Singapore has almost entirely eliminated the great annual wave; the estates have been equally successful.

At one time I was anxious to find out what commercial value improved health had. In discussing the matter a banker suggested that if malaria control had any commercial value, the cost of production would be lower on healthy estates than on unhealthy ones. This seemed to me to be a proper test. From large agency firms and estates I received details of the production and the cost of production of rubber on estates of 100,000 acres. When these figures were analysed it was found that healthy estates produced more and cheaper rubber than the unhealthy ones. The f.o.b. cost per pound in cents for the group which had been healthy was 28·25; while for the group that had remained unhealthy it was 36·03. Further the average yield per acre in pounds for a healthy group was 431·01,

and for an unhealthy group 400·30. More striking figures can hardly be imagined ; and they proved what of course is obvious to ordinary common sense, that a healthy man produces more, and produces more cheaply, than a sick man. Another difference which was apparent between healthy estates and unhealthy estates was that labour did not work less, but worked more, on a healthy estate than on an unhealthy estate, and that labour came much more freely to a healthy estate than to an unhealthy one. The arguments of the employer that he could not afford to make his place healthy and that he would be made bankrupt, if made to spend money, proved to be entirely unfounded.

In Bombay malaria exists, as Major Covell says, in man-created conditions. The breeding places of the mosquito have been made by man and can be eliminated by man. Many places would pay Bombay a lakh of rupees if they could merely exchange their difficult malaria problem for the comparatively simple one of Bombay, where, if a breeding place is once permanently closed down, it ceases for all time to be a danger. Few places are in this happy position. Elsewhere anti-malaria work requires constant supervision from one end of the year to the other. In view of the wide prevalence of malaria throughout Bombay and the fact that all classes are more or less affected by it, that breeding places are in both private and public property, and that every one is very much concerned in the speedy eradication of the disease, it would be foolish in the extreme, not to say entirely uneconomical, to make the work of controlling the disease more difficult than it need be. Co-operation in control is required in Bombay. Want of co-operation between various public bodies was encountered both in the Federal capital of Malaya, and in the city of Singapore. Both were solved in the same way. Instead of health officers wasting their time in serving notices on owners, taking them to court and finally having to do the work themselves, Government decided that the most satisfactory way was to have the work carried out at its own expense, which, of course, means at the expense of the whole community. Unity of control was obtained by putting the work under the Health Officer of the city in the case of Singapore ; and of an anti-malarial Engineer of the Malaria Advisory Board in Kuala Lumpur. Unity of control is advocated by Major Covell. If Bombay seriously intends to get rid of the disease it will have to get it. Only in this way the work will be done expeditiously and without any severe, or indeed, appreciable, cost to any individual.

A few people have genuine religious objections to the abolition of their wells. It seems to me to be entirely reasonable, that if they insist on keeping a well which may be a danger

to the public, they should be required, as is suggested in Dr. Bentley's report, to take out a licence. I suggest, further, that they should be charged with the whole cost of weekly supervision and of any measures like stocking of wells and ponds with fish which may be required. If there was no genuine religious feeling, it is not at all likely that the person would raise objections to the elimination of dangerous breeding places within his premises, when it would cost him nothing.

Some people doubt if malaria can be controlled in Bombay. I would point out that Dr. Bentley's report is one of the ablest malaria reports that has ever been written, and that had his recommendations been followed Bombay would have been free from the disease to-day. Bombay is more than twenty years behind the Malay Peninsula in its attitude towards malaria control. Malaria is a disease chiefly of children, and, when it does not kill either as fever, diarrhœa, or dysentery, or convulsions, it often leaves its marks on a child in an impaired constitution for the rest of its life. The Parsis have been foremost in carrying out good works in Bombay for all communities. We have splendid examples of that in the J. J. Hospital and the Wadia Hospital. The Bombay public looks for a leader in the control of malaria. They will not look in vain, when they understand how much it is costing the city every year in money and lives.

NOTES

Crystal Structure and Chemical Constitution (F. I. C. R.)

The Faraday Society is well known for its Discussions dealing with specific problems within the general range of physical chemistry.

The Conference held in London on March 14–15 last with the title " Crystal Structure and Chemical Constitution " was a notable addition to the series, attended as it was by a number of prominent workers from abroad.

A novel and most welcome feature consisted of an opening lecture by Professor V. M. Goldschmidt, of Oslo, in which he described the researches undertaken in his laboratory, and which amount to no less than a systematic crystal-chemistry of the inorganic compounds. Hand in hand with X-ray analysis have gone investigations by which the properties of inorganic crystals can be anticipated from a knowledge of their component elements, mainly in terms of ionic radii and polarisation. Chemical substitution forms a basic feature of the scheme, leading to the preparation of numerous bodies displaying most interesting characteristics, mostly capable of being foreseen in a way not unlike that associated with the methods of Mendeléeff.

After this Introduction the Discussion proper resolved itself into four parts—(I) Inorganic Compounds, (II) Organic Compounds, (III) Metals, (IV) General.

(I) was concerned essentially with the silicates—introduced by Prof. W. L. Bragg—and the felspars, together with a consideration of the etch figures of sylvine and of the nature of ultramarine. Throughout this section the focus of attention was largely atomic environment (*i.e.* co-ordination number), and the properties of linkages in the substances concerned. Dr. A. M. Taylor contributed an account of his work on the infra-red absorption spectra of the AX_4 group, and Dr. W. A. Wooster exhibited his two-circle spectrometer.

(II) opened with a general survey by Sir William Bragg of the X-ray crystallography of organic compounds. The following papers dealt with the hydrocarbons and long chain compounds. Mrs. Londsdale described the beautiful work on the structure of the benzene nucleus.

(III) was in some respects the most fruitful. Mr. J. D. Bernal spoke on the problem of the metallic state (including a paper by Westgren and Phragmén on alloys). It is impossible in a brief note to do justice to the ideas contained in Bernal's contribution, for the author explained that they had barely "solidified" as yet and the audience recognised that they were witnessing an advance of great promise in the very making. Before the conclusion of this section, Mr. Astbury gave an account of his integrating photometer for X-ray crystal reflections.

(IV) produced a number of experimental points of great interest; and led to a prolonged general discussion. The subjects dealt with included intensity and sharpness of Debye-Scherrer lines, methods of determining the size and shape of small crystals, lattice determination in polycrystalline aggregates, allotropy, and densities.

A real *bonne bouche* was reserved for the end, when Professor Ewald gave a résumé of the wave mechanics in their bearing upon crystal structure, and the new theories of chemical combination (including the resonance phenomenon) due mainly to Heitler and London.

Gas Cylinder Research (L. F. B.)

THE Fourth Report of the Gas Cylinder Research Committee, published under the authority of H.M. Stationery Office, 1929 (price 4*s*. net), dealing with cylinders for liquefiable gases, marks the completion of the work for which the Gas Cylinder Research Committee was appointed. The earlier reports dealt with ordinary commercial cylinders for the permanent gases, with the periodical heat-treatment of carbon steel cylinders and with alloy steel light cylinders for special purposes. Since the Committee was appointed to determine the technical conditions which should be observed in the manufacture, testing, and charging of cylinders for the conveyance of compressed gases, they did not consider whether the acceptance by one firm of the test marks of another should be permitted, or whether private ownership of gas cylinders should be permitted. These matters will be considered by the Home Office. The storage of chlorine and other gases otherwise than in solid-drawn cylinders will be reported upon later by a sub-committee. Liquefiable gases are those which have relatively high critical temperatures, and which are generally reduced to the liquid state by the pressures which are employed in forcing them into the cylinders for storage. The properties of the following ten gases—which do not cover all gases at present transported in the liquid state—sulphur dioxide, ammonia, chlorine, methyl chloride, ethyl chloride, hydrocyanic acid, phosgene, carbon dioxide, nitrous oxide, and

ethylene were investigated. The gases, ethyl chloride, hydrocyanic acid, and phosgene all have normal boiling points above 0° C., so that they are in the liquid state at atmospheric pressures and low atmospheric temperatures, but their special properties require that they should be transported in closed containers, and they therefore come within the scope of the investigation. When a permanent gas is placed in a cylinder, it is certain that the maximum possible internal pressure is practically limited to the pumping pressure used in filling the cylinder, but in the case of the liquefiable gases, so long as the cylinder is not completely filled with liquid, the internal pressure is the saturation vapour pressure, if the gas is pure. This is usually quite low compared with 1,800 lb. sq. in., but danger arises from the relatively high thermal coefficient of the liquefied gas, which, if the cylinder does not possess sufficient free space after filling, may cause dangerous pressures to develop. In only a few cases is it found that the liquefied gas is so readily compressible that the cylinder may be completely filled with liquid without danger. The filling pressure is clearly not an accurate measure of the charge supplied to the cylinder in the case of a liquefiable gas, and the report points out that the most direct and reliable method of verifying the correctness of the charge is by weighing the cylinder before and after filling. The maximum changes of temperature to which a charged cylinder is likely to be exposed are a matter of great importance, and the Committee decided to class cylinders as available for use in "temperate" or "tropical" regions, according as the highest temperature the cylinder was likely to attain was below or above 45° C. respectively.

The amount of liquefiable gas which may be safely introduced into a cylinder is measured by the "filling ratio," which is the weight (in lb.) of gas permissible divided by the weight of water which would fill the cylinder. Of the gases in the list given above, the first eight are always either partly or wholly liquid in a charged cylinder, and the other two may be in a wholly gaseous state at certain atmospheric temperatures. With the first four gases the point at which the cylinder becomes full of liquid is very sharply marked and the filling ratio varies from 1·19 for sulphur dioxide to 0·54 for ammonia, in "temperate" climates. When hydrocyanic acid is stored, traces of alkali and iron cause it to undergo polymerisation with a considerable evolution of heat, so that it is necessary to add 0·1 per cent. of concentrated sulphuric acid to the liquid. In the case of carbon dioxide the saturation pressure is relatively high, even at relatively low atmospheric temperatures, and the point at which the cylinder becomes completely filled with liquid is not definite.

The Committee give a detailed specification of the manufacture and testing of the storage cylinders, which should be made of seamless steel. The hydraulic tests to which they must be subjected are very interesting. In one test, the cylinder is immersed in a closed vessel, or jacket, filled with water, with a gauge-glass projecting from the top. Hydraulic pressure is applied to the inside of the cylinder and the displacement of water in the gauge measures the extension of the cylinder. Another " water jacket " test and a " non jacket " test are also described. A valuable appendix to the report, by A. Bailey, M.Sc., is an account of some physical properties, saturation vapour pressure, specific volume, etc., of commercial samples of sulphur dioxide, ammonia, chlorine, methyl chloride, carbon dioxide, nitrous oxide, and ethylene. The investigator mentions one peculiar phenomenon which was not followed up. When the liquid was compressed the pressure rose to a certain value, and then began to decrease, at first rapidly, and then more slowly until it reached a constant value after some 15 to 40 minutes. Again, on decreasing the pressure on the liquid the pressure fell considerably below the normal and then increased to a steady state, the changes being surprisingly large.

Chemical Science in Czechoslovakia (J. G. F. Druce, M.Sc., F.I.C.)

In the Middle Ages Prague, the capital of the old kingdom of Bohemia, was an important seat of learning. Its university was founded in 1348 by Charles IV, after whom it is named. His reign was a golden age for Bohemia, whose mines supplied Central Europe with gold, silver, and other metals which were minted into currency in several centres. It was thus natural that attempts at transmutations and other alchemical processes should come to be practised in Prague, often under noble and royal patronage. During the sixteenth century Dr. Dee and Edward Kelley were invited to Prague by Rudolf II, and they also " projected " for Count Rosenberg in South Bohemia. Loss of Bohemia's independence and subsequent persecution hampered scientific progress. Komensky, the great Slovak pedagogue, for instance was exiled, and although there was a revival in the pursuit of the natural sciences in the nineteenth century the Czechoslovaks had largely to make a fresh start ten years ago. During this past decade there has been marked progress in the research work conducted in the universities and technical colleges at Prague, Brno, and elsewhere. The wider scientific public, however, has not been able to form an adequate opinion of the extent and value of the results attained by Czechoslovak scientists because, unless the original work was abroad in English, French, or German journals, it escaped attention. Consequently many important memoirs have

remained untranslated in the archives and publications of Czechoslovak learned Societies.

To remedy this, the chemists of that country have founded a *Collection of Czechoslovak Chemical Communications*, the editors of which are Prof. E. Votoček and Prof. J. Heyrovsky (Prague II, Albertov 2030). The *Collection* will publish each month in full the more important researches in pure chemistry carried out in Czechoslovakia, provided that they have not previously been published elsewhere. In addition there is to be a bibliography of all the scientific and technical publications by Czechoslovak chemists and reviews of their books.

So far four numbers have appeared and all contain matter of considerable scientific interest. In the first issue Prof. Štěrba-Böhm, who has made a life study of the chemistry of scandium and the rare earths, gives an account of his researches (with S. Škramovsky) on the complex oxalates of that rare element. Prof. Heyrovsky and S. Berezicky describe the electro-deposition of radium and other alkaline earth metals at the dropping mercury cathode. This is a continuation of the researches being carried out in the Physico-chemical laboratory of the Charles' university with the aid of Heyrovsky's polarograph. Particular interest attaches to the work on the separation of radium from barium. The preparations used were obtained from the mines of Jachymov (St. Joachimsthal) in N.W. Bohemia, from which Prof. and Mme Curie obtained their pitchblende in which radium was first discovered. The mines are still productive, but do not yield so much as those of the Belgian Congo.

In inorganic chemistry there are contributions dealing with the reaction between cupric salts and sodium thiosulphate (Prof. Hanuš and V. Horovka); the estimation of thallous salts by titration with permanganate in the presence of hydrochloric acid (A. Jilek and J. Lukas); the formation of acid lithium aluminate (D. Prociv); the dissolution of silver in water (Prof. H. Křepelka and F. Toul); and the rapid electrolytic estimation of tin (J. Švéda and R. Uzel).

The contributions in organic chemistry include several papers by Prof. Votoček, who has spent many years investigating the nature of the polysaccharides, especially the less common sugars and their derivatives. With various collaborators he now describes rhamno-convolvulic acid and a 3:12 dioxypalmitic acid derived from it. The sugar, quinovose, is the subject of two papers in which it is shown to be identical with iso-rhodeose. Other investigators in organic chemistry describe certain nitro-naphthylamines, pyrollones, complex salts of dimethyl glyoxime, and also the interaction of hydrazine with some unsaturated acids.

A bibliography of books and original articles (250 in number) in pure and applied chemistry and which appeared during 1928 has been compiled by Prof. A. Simek and appears in the March issue. They include contributions to all the main branches of chemical science. In addition to the researches already noted the bibliography indicates that in physical chemistry attention is being given to the study of the hydration of ions, and to X-ray and general spectroscopy. Among the works of the inorganic chemists published last year those upon the revision of the atomic weight of arsenic, a study of perselenic acid, the thionic acids, and the perborates are worthy of mention. A number of biochemical and pharmaceutical papers appeared, whilst other articles deal with dyeing and photographic chemistry.

In applied chemistry several papers emanated from the sugar research laboratories. The research establishments of the leather trades and fermentation industries also studied problems of technical importance. Prof. Krauz and his pupils continue to publish work on explosives, having been concerned recently with the separation of the tri-nitro-benzenes and toluenes and with the stabilisation of smokeless powders.

Reviews of two new books appeared in the April number. The first is by Prof. Simek, who writes a notice of Prof. Baborovsky's *Introduction to Theoretical and Physical Chemistry* (in Czech), and the second is Dr. Herasymenko's review of *Die polarographische Methode*, which Dr. S. Prát has contributed to Abderhalden's *Handbuch der biologischen Arbeitsmethoden.*

The editors can be congratulated upon the high standard attained by these initial numbers of the *Collection of Czechoslovak Chemical Communications*, the subscription to which is 170 crowns (£1 in Great Britain). The new journal should soon find its way into all libraries where a representative list of scientific publications is maintained.

Fishery Investigations (M. V. Lebour, D.Sc.)

Report on Mussel Purification, being an Account of the Establishment of a System of Purification of Polluted Mussels; of the Experimental Work upon which it is based; and of certain General Considerations and Suggestions regarding the Sewage Pollution of Shellfish in its Public Health Aspect. By R. W. Dodgson, M.D. (Lond.), M.R.C.P. (Lond.), M.R.C.S. (Eng.), Director of the Ministry's Shellfish Research Station, Conway. Ministry of Agriculture and Fisheries. Fishery Investigations. Series II, Vol. X, No. 1, 1928. London: His Majesty's Stationery Office, price £1 1*s.* net. This most important report discusses at length all the chief points regarding the pollution of mussels and the methods of purification, with occasional notes on oysters. The oysters, however, are to

have a later report to themselves. Not only does Dr. Dodgson give us a very clear and interesting account of the purification processes now in use at Conway, where he is Director of the Research Station, but he knows the why and wherefore of every point, illustrated by a perfectly simple description of the anatomy of the mussel. The whole is so clearly written that it is read with much enjoyment and one fully realises that its author is in perfect sympathy with the vagaries of this most erratic mollusk which is always behaving in an unexpected manner. The simplicity of the method of purification is absolute. The mussels purify themselves, given a suitable environment with pure water.

It has been found that cleansing of mussels which are used for human food is necessary because the natural habitat of these shell-fish is almost always in the neighbourhood of sewers, or in some way a certain amount of sewage can reach them. River estuaries are the favourite natural sites for mussel beds where the water is salt to a certain extent, fresh water killing them, but sea-water tolerated. Thus mussels can live between tide-marks or on the coast and can grow on buoys or at the bottom of boats in the sea, but never far from land. The estuaries where the mussels live are nearly always the places chosen for the disposal of sewage. Even when much care is taken to carry the main sewers out to sea it is usually possible for certain winds and currents to waft it back to a certain extent into the neighbourhood of the mussels, and when special sterilising plants are used it is still possible for the mussels to be contaminated. Although statistics show an enormous decrease in those diseases which may be carried by mussels (of which the most important is typhoid, or enteric), probably owing to cleaner feeding and habits, and more careful disposal of sewage, cases are still known in which mussels are the cause of these outbreaks. It is therefore eminently desirable to purify the mussels before allowing them to be sold for human food. A large portion of the report is given up to showing how necessary this purification is and in indicating the diseases which may be prevented by such a simple remedy. Then comes an account of the anatomy of the mussel and its method of feeding which is the basis of the system of purification.

The feeding process is essentially the taking in of an enormous volume of water by means of currents set up by the movement of the cilia or hair-like processes, on the gills and elsewhere on the body. From this water all suspended matter is filtered, fixed in mucus secreted by the gill glands, and, entering the mouth, goes through the alimentary canal to be discharged as fæces after a certain amount of food has been retained, or the larger particles of mud or extraneous matter may take another

course, and, still entangled in mucus, may be discharged as so-called pseudo-fæces without entering the alimentary canal at all. Thus a large portion of the solids taken in with the stream of water, including the disease-giving bacteria, are sent out of the body. A mussel functioning properly sends through its body 10 gallons of water or more in 24 hours. The fæces and pseudo fæces with the bacteria entangled in mucus fall to the ground. The mussel is thus capable of clearing itself from harmful bacteria.

An account follows of the methods employed at Conway for allowing the mussels to purify themselves and of putting on the market the clean and superior shell-fish, the cleansing process requiring 48 hours. Large tanks are provided with grids on which the mussels (cleared of obvious mud and stones) are placed by the fishermen. These are first hosed to remove remaining dirt, and sea-water, first sterilised by a minute amount of bleaching powder and as it flows into the tank mixed with a very small amount of thiosulphate of soda ("hypo"), is sent over them. In this water the mussels are left overnight during which time they clear themselves of solid matter including contained bacteria; the water is then run off, carrying with it the excreta, and a fresh lot of sea-water is run in. This is again left for a night and any remaining solid matter got rid of, run off the next morning, and a final bath given with sea-water containing a certain amount (3 parts of chlorine to 1,000,000 parts of water) of chlorine, in order to kill any bacteria left on the shells, tank walls, or grids. This last bath is by way of precaution only, and not absolutely necessary—and the mussels are ready for the market. The process, simple as it is, involves careful supervision, for it is absolutely necessary to be able to tell whether the mussels are functioning properly all the time—perfectly easy if one knows what they should look like, but requiring someone with knowledge to be on the spot—and to be quite sure that the whole process has been managed properly. The purification plant has been evolved only after much research and the outcome of all this labour is a system which is economically sound in every way. The fishermen bring their mussels and place them in the tanks, have them purified and sealed, for which they pay, and then send them to the market. After purification the water is crystal clear, so clear that it has been suggested that in the event of the tanks being used in the off season as swimming baths, a layer of mussels should be placed at the bottom to keep the water clean.

The latter half of the report has chiefly to do with the methods of bacterial analysis to which samples of the mussels are subjected, depending on the number of colonies of harmful bacteria to be found before and after purification. The value

of these methods is discussed at great length. Also the value of investigating carefully the topography of the beds. Plans and maps and good figures of the mussel when functioning naturally, and photos of bacterial colonies are included, the whole making up a large volume of nearly 500 pages. The value of this beautiful work is enormous, rewarding the author for all the trouble and time taken in making the observations contained therein, not to mention the bringing together of all this huge mass of material.

Experts and Quasi-Experts (R. R.)

There is a current saying that when doctors differ no one can decide. Reading "Experts" for "Doctors" we can almost say with certainty that "Experts" seldom differ from each other but often differ from "Quasi-experts." The labour of becoming an expert is serious and severe, but anyone can become a quasi-expert simply by stating that he is an expert. Not only that, but while the expert acquires little credit for his work, the quasi-expert often succeeds marvellously in the world's opinion, besides acquiring many comfortable little jobs. The reason for this is that while the expert toils at his subject, whatever it is, the quasi-expert easily becomes more proficient in getting the good things of the world and the interest so necessary for acquiring the comfortable jobs. In fact our heading might have been "Experts and Jobbers."

We have recently seen, for instance, a notice to the effect that an anti-malaria campaign is going to be carried out, not only by the means advocated by experts, but also by the simple process of improving the diet of natives in a certain British Colony. Mosquito-control, which has generally been advocated by experts, is, according to one statement, to give place to beefsteak breakfasts, let us presume for the whole population. We are informed that badly nourished people are very liable to contract malaria, so that all we have to do is to feed the populace! Good! but unfortunately we must remember the case of British soldiers, who are generally well-fed enough, but contract malaria nevertheless. And then, while these quasi-experts deprecate the cost of anti-malaria campaigns, they say nothing of the cost of beefsteak breakfasts to natives and to their families. Bogus advice of this kind seems to be swallowed as easily by the public as the beefsteak breakfasts themselves.

Public Indifference to Medical Discoveries (R. R.)

Another fact which assures us that the human race is not so logical as it supposes itself to be is the indifference which it shows to the medical affairs which are concerned with the lives

of millions of human beings. While they fuss immensely over little matters of taxation and over different kinds of game-playing, they leave such vital affairs as the causes and progress of epidemics to any medical man or biologist who troubles to take up such things, and they show small thanks to them even if they succeed. For instance, during the last days of last century the Americans demonstrated how yellow fever is carried, without finding the causative organism. Some years later, after I had tracked the malaria parasites in mosquitoes, I wrote an official letter to the American Government, offering my services to track the yellow-fever parasites in the same insects. By that time I was presumably well acquainted with the tissues of mosquitoes, and also with the minute objects which are parasitic in them, and should have been able to find the yellow-fever germ in them if anybody could. The American Authorities, however, wrote back to me saying that they would welcome my assistance, but that I would have to pay my own expenses to America and back, and for living there, for their good! Naturally I refused to go under these conditions, especially as I was cognisant of their niggardliness to Walter Reed and his colleagues, who had demonstrated the fact that the yellow-fever germ, whatever it is, is carried from the infected to healthy persons by mosquitoes. I will not claim that I would certainly have succeeded in the quest I proposed, but I was probably more likely to succeed than others who had not worked so much at a similar problem. Anyway, neither the Americans nor any other research workers have as yet found the yellow-fever germ, and probably will not find it for some time to come. This sort of thing is called "penny wise and pound foolish," and I wonder how many people have died of yellow fever since that date.

The authorities who manage British affairs are scarcely less "bright." For example, the *B.M.J.* for February 2, 1929, reports that there is now a very serious outbreak of influenza in Scotland, where the Registrar General reports that during the week ending January 19, 1929, deaths from influenza numbered 179, of which 151 occurred in Glasgow alone. "The Public Services suffered severely, for about 250 tramway men were off duty largely from this cause, and the Police Force was reduced by a sick list of 230," while there were many absentees from the schools. Even here, two friends of ours living at Wimbledon have recently suffered from such severe attacks that one has been very ill, while the other, whom we saw in perfect health a fortnight ago, has just died. Our worthy politicians, who are so fond of talking and proving their immense capacities (especially just before election time) apparently take little interest in a mortality which, if caused

by a human invader, would produce an outbreak of war. Of course many researches on influenza and "colds" have been attempted, but the result has apparently been but small, either because not numerous enough investigators have been employed or because these persons have not possessed sufficient capacity to solve the numerous problems concerned. If I were the head of the British Government instead of being a mere unit near the tail-end of John Bull, I should put every available penny into medical research, which affects the whole nation more nearly than do questions which are much less germane to it. When *Homo sapiens* develops into a reasonable creature we may improve, but not till then.

Notes and News

The third triennial award of the Manson Medal of the Royal Society of Tropical Medicine and Hygiene has been made to Sir Ronald Ross.

Dr. F. O. Bower, F.R.S., lately Regius Professor of Botany in the University of Glasgow, has been chosen for nomination as President of the British Association for the meeting in Bristol in 1930. The centenary meeting of the Association in 1931 will be held in London.

Sir Alfred Ewing, who relinquishes his post as Principal of the University of Edinburgh in September, has been made a freeman of the City of Edinburgh. He has also been appointed to be a member of the Advisory Council to the Committee of the Privy Council for Scientific and Industrial Research in place of the late Mr. R. W. Reid.

Prof. E. V. Appleton, of King's College, London, has been awarded the Morris Liebmann Memorial Prize for 1929 by the American Institute of Radio Engineers.

The Council of the Royal Society of Edinburgh has awarded the Gunning Victoria Jubilee Prize for 1924–8 to Prof. E. T. Whittaker for his mathematical work and the Macdougall-Brisbane Prize to Dr. W. O. Kermack for his contributions to chemistry.

Dr. W. H. Eccles has been elected President of the Physical Society, Prof. J. W. Gregory President of the Geological Society, Prof. W. C. M'Intosh President of the Ray Society, and Mr. J. Ramsbottom President of the Quekett Microscopical Club.

Mr. Henry Young, assistant secretary of the Royal Institution, is retiring, after fifty years in the service of the Institution. Mr. Thomas Martin, secretary to the Institute of Physics, has been appointed to succeed him.

We have noted with regret the announcement of the death of the following men, well known in scientific circles, during the past quarter: Dr. Alex. Hill, secretary of the Universities

Bureau of the British Empire; Mr. C. E. Benham, of the *Essex County Standard* and a well-known amateur scientist; Mr. W. W. Beaumont, motor engineer; Mr. T. H. Blakesley, physicist; Prof. F. Kehrmann, of Lausanne, organic chemist; Sir George Knibbs, Director of the Institute of Science and Industry, Australia, 1921–6; Prof. J. von Kries, of Freiburg, physiologist; Dr. S. J. Mauchly, of the Department of Terrestrial Magnetism of the Carnegie Institution; Dr. T. B. Osborne, of New Haven, organic chemist; Sir Henry Rew, agriculturist and statistician; Sir B. Windle, F.R.S., of Toronto, anthropologist; Prof. R. H. Yapp, of Birmingham, botanist.

The first Congress of the International Society for Microbiology will be held in Paris in June 1930, and not in October 1929, as previously announced.

The facilities hitherto existent for the examination of dead salmon, trout, and freshwater fish at the Fisheries Research Station, Alresford, Hants, will not be available for a period of about twelve months. Meanwhile, temporary arrangements have been made for the examination of fish suspected of having died from disease at 101, George Street, Edinburgh. Fish suspected of having been killed by pollution cannot be examined. In sending specimens the following instructions should be observed: (i) The fish should be addressed to Dr. Williamson, Fishery Board for Scotland, at the above address and despatched without delay by the quickest route, carriage paid; (ii) fish should be packed in ice, having first been wrapped round in grease-proof paper. No disinfectants of any sort should be used on fish or packing. If any outside packing is necessary wood shavings should be used, not hay or straw; (iii) notification of despatch should be sent by telegram and a letter should follow giving all particulars.

On January 1 the Rockefeller Foundation took over the work in Europe which was previously under the administration of the International Education Board. Dr. Lauder W. Jones, of Princeton University, has been appointed Associate Director for the Natural Sciences of the Rockefeller Foundation. Dr. Jones assumed his duties the beginning of April and will have his headquarters in Paris, carrying on the work as successor to Dr. Augustus Trowbridge.

An International, Colonial, and Maritime Exhibition is to be held in Antwerp next year in celebration of the Treaty of Belgian Independence. The British section, which is being organised by the Department of Overseas Trade, has been allotted an excellent position and it should be a predominant feature of the Exhibition. The Treasury has sanctioned an expenditure of £100,000, and the building has been designed by Sir Edward Lutyens. The most important shipping companies

are to have displays and scientific and navigational instruments will be among the principal exhibits.

One of the first duties of the Explosives in Mines Research Committee, appointed in September 1922, was to undertake a revision of the official test of explosives for inclusion in the permitted list, the test formerly carried out at the Official Testing Station at Rotherham. An account of the Committee's labours in this connection, and of the results, is contained in a report entitled "The Testing of Explosives for Use in Fiery Coal-mines." (Safety in Mines Research Board Paper No. 51, H.M. Stationery Office, price 2*s.* net.)

The Committee, after a long course of trials, have found that firedamp is the most suitable inflammable gas to use in the test, and, after further experiments, have formulated a new test which will give greater regularity in its results than the present test and should thus afford better means of controlling the uniformity of explosives while maintaining an ample margin of safety. The experiments have further shown the great value of effective stemming in preventing the ignition of gaseous mixtures and the Committee lay emphasis on the importance of a rigid observance of the regulations with regard to stemming.

A detailed description of the new test is appended to the Report.

We have received from the Terramare Office (Berlin, S.W.48, Wilhelmstr. 23), an attractive little booklet containing a classified list of all the principal festivals, exhibitions, conventions and sports meetings to be held in Germany during the year. It might well be consulted by anyone proposing to spend the long vacation in that country. Included in the section devoted to Science are notices of the following meetings: International Soil Congress, July 20–25, Königsberg; German Botanists' Congress, August, Danzig; German Physicists' and Mathematicians' Congress, September, Prague.

Building Science Abstracts (H.M. Stationery Office, Kingsway, price 9*d.* per copy, or 10*s.* per year, post free), is now in the second year of its publication. The first volume contained 1,612 abstracts of articles, 168 references to new books and 513 to patents, all of interest to builders, to manufacturers of building materials, to teachers and to research workers. The abstracts and references were drawn from 123 periodicals. In the 1929 volume a still larger number of periodicals is being used, an improved classification of abstracts has been adopted, and the subject range has been extended.

The Report of the National Institute of Industrial Psychology for 1928 shows that the rapid growth of the work of the Institute continues unabated. The fees increased by 29 per

cent. as compared with 1927, the number of members and associates has increased by 200 and the number of cases in which vocational advice has been sought from 180 to 301. The equipment of the premises in Aldwych House acquired during 1928, which has been designed and arranged in accordance with the principles advocated by the Institute, is nearly complete. Plans for 1929 include the formation of a Household Section, to attempt to do for the home what has already been done for the workshop and office.

In a paper dealing with the separation of the chemical constituents of petroleum in the *Bureau of Standards Journal of Research* (Vol. 2, p. 467), improved methods of separation by both distillation and crystallisation are described. A special apparatus for combustion analysis has been evolved which makes it possible to determine n and x in the formula C_nH_{2n+x} and in any hydrocarbon up to C_{100}. A footnote suggests that at ordinary temperatures the vapour pressure of mercury becomes infinitesimal when the surface of the metal is covered with a film of grease such as would form under ordinary conditions in the laboratory. This is comforting news for those who fear poisoning from deposits of mercury in the cracks of tables and floors.

In the same journal two papers on building research have recently appeared. The first (Vol. 2, p. 541) deals with the transmission of sound through wall and floor structures and is supplementary to previous research carried out by the Bureau of Standards. It is shown that for partitions of similar structure the sound absorption depends upon the mass per unit area of the partition. The second communication (Vol. 2, p. 1) gives a detailed account of a great number of tests for fire resistance of hollow load-bearing wall tile. Specimens of wall of various structures and composed of different units were exposed to fire on one side for different lengths of time and subjected to typical loads. These two papers represent researches which are intimately connected, since in general it would appear that fire-proof buildings are the noisier on account of the steel girders and thin masonry partitions employed in their construction.

For a considerable number of years it was firmly believed that the photo-electric effect was completely independent of temperature over a range from liquid air up to 600° C., when a clean surface in high vacua was used. However, in 1922, Ives found that when photocells containing alkali metals were cooled to liquid air temperatures the photo-electric emission decreased for all wave-lengths in the incident light, the longer wave-lengths being the more affected. In an interesting paper in the *Transactions of the Royal Society of Canada* for May 1928,

McLennan, Matheson, and Niven describe an extension of these experiments down to the temperature of liquid hydrogen, 21° K. They find that the photo-electric emission shows a further decrease. It would appear that these results are due to a change in the work function of the surface with temperature, brought about by volume or surface strains.

Dr. H. H. Potter contributes a paper on the X-ray structure and magnetic properties of single crystals of Heusler alloy to the *Proceedings of the Physical Society* for February. These ferromagnetic alloys consist approximately of two parts copper, one part manganese, and one part aluminium or tin. The tin alloys have so far yielded no single crystals. With the aluminium alloys, crystals up to roughly one-eighth of a cubic cm. in volume have been obtained. They crystallise as a body-centered cube with lattice constant equal to 2·95° Å., the aluminium atoms being distributed in such a manner that they lie on a face-centered cube with lattice constant equal to 5·9 Å. It is very interesting that these crystals exhibit the same directional properties as nickel crystals which have a face-centered cubic structure. Now, it appears very probable that the ferromagnetic properties of these alloys are due to the presence of manganese, and the magnetic behaviour of the crystals indicates that the manganese atoms, whose positions cannot be determined by X-ray analysis, also lie in a face-centered cubic lattice.

In an interesting paper, "Some Aspects of Forest Ecology," which appears in the *Empire Forestry Journal* for 1928, Prof. E. J. Salisbury draws attention to the importance of the study of ecology to the forester. It is pointed out that large differences in conditions may exist between natural woodlands, which are artificially tended, and purely artificial plantations even of long standing; such differences may be exemplified by the presence of certain slugs, such as *Limax cinereoniger* and *Limax tenellus* in ancient semi-natural woodlands, which appear to be entirely absent from old plantations in the near vicinity.

The most important factor affecting the tree layer in both natural and artificial woodlands will be differences in soil conditions the chief of which is soil structure. In natural woodlands the soil exhibits a definite organisation or stratification. At the top is the litter consisting entirely of organic material, below is a surface layer of soil rich in organic material, and below that the subsoil, largely inorganic in character and derived from the underlying rock. The surface layers are the most important to the forester, as the feeding roots are situated there, and there also the seeds will germinate. The litter in particular is most important, as by means of it mineral salts and other nutrient materials are returned to the soil;

the great importance of it has been demonstrated by comparison of areas where the litter has been removed, and those in which it has been left undisturbed. In Bavarian State Forests, for instance, Ragl records that growth with litter was two and a half times as great as where the litter had been removed.

Other important factors connected with the organic soil layer are its reaction and production of toxic substances. The level of the water table and the effect on this of the woodland itself is also worthy of investigation. The surface layers also, owing to their high organic content, will tend to be much more aerated than the lower layers, the density of the soil increasing with the depth ; this will help to determine the shallow character of the feeding roots of the woodland community, and may be extremely important in determining the successful establishment of seedlings.

The soil reaction as mentioned above is also extremely important, the variation in hydrogen-ion concentration often corresponding to the presence of definite societies in the ground flora ; these are important to the forester as indicators of the soil conditions. The fungus flora and bacterial content also depend on the soil reaction, as does also the presence or absence of certain mycorrhizal fungi.

It is thus evident that all factors affecting the soil conditions are dependent upon the entire community of organisms, both plant and animal, which comprise what we term woodland or forest. Above all, however, a much closer liaison must exist in future between the forester and the user of the timber, in order that the conditions investigated may be correlated with the character of the timber produced.

In the *Journal of the Council for Scientific and Industrial Research for Australia* there are interesting accounts of two diseases of bananas in Queensland by Prof. E. J. Goddard. The first is known as the Bunchy Top Disease, which is well named, as the outstanding characteristic of a thoroughly infected plant is the rosetting of the leaves at the top of the plant. The disease was first reported from Fiji in 1885 and has since become so serious in Australia that a Bunchy Top Investigation was initiated in 1924 through the co-operation of the Governments of the Commonwealth, New South Wales, and Queensland.

It was discovered that the disease was not due to any fungus, bacterium, or insect pest, but to a virus which was transmitted by aphides. In the course of the investigation it was found that aphides transferred from diseased to healthy plants grown under nets or in an insect-proof house never failed to transmit the disease ; whereas, aphides taken from

healthy plants growing in areas where the disease did not exist failed to transmit the disease to healthy plants ; and that the symptoms of the disease always appeared twenty-six days after transfer of the aphides, thus indicating an incubation period. A very interesting experiment consisted in growing diseased and healthy plants together in the same tank, all being entirely free from aphides before planting. Under these conditions the diseased plants remained diseased and the healthy ones healthy, and the disease was in no case transmitted. This evidence showed that the Bunchy Top was a virus disease transmitted by the banana aphis *Pentalonia nigronervosa.*

It was found that the times of rapid spread of Bunchy Top can be co-ordinated with the maximum winged migration of aphides during September-October, November-December, January-February and March-April ; there are practically no winged forms during the summer. In this connection, though spraying with aphicides can have little protective effect, yet it may be useful when plants are being transplanted during the time when winged aphides are abundant. The control methods, however, originally recommended by the Bunchy Top Investigation are those most likely to have most effect. These are the immediate eradication and destruction of infected plants, the destruction of deserted plantations, and an embargo on the shifting of suckers from infected areas. The last has proved a considerable check to the spread of the disease and in many cases the disease has been eradicated from whole plantations.

The second disease described by Prof. Goddard is that known as Squirter Disease, and has caused serious damage to banana fruit in some parts of Australia in recent years. In 1925 an Investigation was appointed, supported by the Institute of Science and Industry and the Department of Agriculture and Stock, Queensland, of which the preliminary report is now given.

The symptoms of the disease are very characteristic, and the name suitably describes the disease, which consists in a softening and darkening of the centre of the fruit, the whole fruit substance eventually becoming a mushy liquid which squirts out at the base or angles of the fruit upon slight pressure.

It has been definitely established that the disease is in no way associated with the presence of a bacterial or fungal organism ; also that the rate of ripening of the fruit and method of ripening has no bearing on the occurrence of the disease. The disease is absent in N. Queensland, and it has been found impossible to produce it there, whereas it is very prevalent in S. Queensland. Climatic conditions such as temperature and rainfall seem definitely to be a factor in its production ;

transport conditions also seem to constitute another factor in the problem.

It would seem that fruit grown in certain parts, namely S. Queensland, suffers during the winter under a physiological disability, which responds to unsuitable temperature or other conditions during transport to produce the condition known as "squirter."

Further investigation is still necessary, but it is hoped that some solution of the problem may be found in the scientific ripening of the bananas under standard conditions of temperature and humidity as is already practised in America.

The appearance of the first portions of a new popular scientific work, *The Science of Life*, by H. G. Wells, Julian Huxley, and G. P. Wells, must be a source of considerable satisfaction to all who seriously desire the spread of accurate scientific knowledge. By means of his *Outline of History*, Mr. H. G. Wells made it possible for the busy man-in-the-street to obtain a helpful glimpse of the broad world of human experience and human achievement. With the assistance of two very able collaborators he has now turned his attention to the problem of placing before the public a concise, accurate, and easily assimilated account of the facts of life which scientific research has revealed. We cannot but admire the way in which this work has been planned, and we cannot help feeling that its publication will produce a far-reaching effect if it is read by the general public to the extent which we confidently expect.

It is surprising to find from the *Abstracts of Dissertations Approved for the Ph.D., M.Sc., and M.Litt. Degrees in the University of Cambridge* during the academical year 1927–8 that no fewer than 12 out of a total of 56 of these higher degrees were awarded for research in Chemistry. Physics came next with 10 degrees, while Mathematics with 2 and Engineering with only 1 seem to be represented very inadequately. Among the dissertations on physical subjects were a study of the scintillation method for the detection of α particles by Dr. J. Chariton and an investigation of the chemical activity of helium by Dr. D. M. Morrison. Dr. Chariton's work shows that the percentage of the scintillations seen through a microscope giving a magnification of 50 is quite negligible when fewer than 300 quanta of light ($\lambda = 5050$ A.U.) are emitted. With the unaided eye, on the other hand, a few scintillations are perceived when the energy reaching the eye from a single scintillation is as small as 35 quanta. Dr. Morrison was unable to confirm Boomer's work, which went to show the existence of a helide of tungsten (WHe_2) nor was it possible to cause helium to combine with nitrogen. He found strong evidence, however, for the formation of a helide of radium B or radium C.

ESSAY-REVIEWS

SOME PROBLEMS IN CAPILLARITY. By Allan Ferguson, M.A., D.Sc., being a review of **Kapillarität und Oberflächenspannung**, by Prof. Dr. G. Bakker. [Pp. 458, with 114 figures.] **Wien-Harms Handbuch der Experimentalphysik**, Band VI. (Leipzig: Akademische Verlagsgesellschaft, m.b.H. 1928. Price M. 42; bound, M. 44.)

It is hardly necessary to say that a work on capillarity associated with the name of Dr. Bakker will be both scholarly and profound; the author's long-continued and weighty researches into the subject are sufficient guarantee that the volume on *Kapillarität und Oberflächenspannung* in the Wien-Harms *Handbuch der Experimentalphysik* will prove to be a contribution of permanent value to the investigator in this branch of physical science. The ferment of ideas which is altering the whole of our outlook on physical science in general has not been without effect on the theories of surface phenomena, and Dr. Bakker, while giving a full and judicious account of modern work, has kept the balance very fairly so that the inquirer for information on experimental methods and results, on thermodynamic theories of capillarity, on the classical theories of Laplace, of Gauss and of Rayleigh, on solutions of the differential equation to the capillary surface, on thin films, on the effect of temperature on surface tension, and the relation of surface tension to other physical quantities, on impure surfaces and oil layers as well as on the work of Langmuir, of Harkins and of Adams, will find descriptions which are on the whole full, precise and completely documented.

The section which deals with experimental methods is specially full and interesting, although here and there one who has devoted special attention to these methods will find gaps which indicate that Dr. Bakker has overlooked some parts of the English and American literature. Thus the problem of determining the surface tension of liquids obtainable in small quantities only has received considerable attention during the last dozen years. The obvious solution of the problem—if one is granted the use of at least a few cubic centimetres of liquid—is to employ what is in effect a vertical U-tube, one limb of which is capillary. The diameter of the wider limb may be such as to permit the experimenter to consider the

surface therein as plane, or, if its width be inside this limit, the correction for the meniscus depression therein may be calculated to any required degree of accuracy from the tables of Bashforth and Adams [1]. The difference in level of the surface of the liquid in the U-tube is measured and the surface tension calculated from the usual formula. Richards and Coombs [2] have carried out experiments by the first method, and their work deserves more than a passing mention. Indeed, it is curious that Dr. Bakker, after describing the capillary-rise method as " die älteste und noch immer einer der besten Methoden zur Bestimmung der Kapillarkonstanten," should devote so little space to a description of the experimental details associated with the method. Richards and Coombs, for example, employ a narrow and a wide tube, the latter 38 mm. in diameter, joined at the top and bottom and provided at the top with a tap for convenience in filling the apparatus. The wide tube contains a glass sinker which serves to reduce the quantity of liquid required. It is interesting to note that Richards and Coombs measure the actual height (y) of the meniscus in the narrow tube, and, for the " corrected " height (h_0) take

$$h_0 = h + \tfrac{1}{3}y \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(i)},$$

where h is the difference in level of the surfaces of the liquid in the narrow and the wide tube measured to the bottom of the narrow-tube meniscus. Richards and Coombs adopt this correction, having some suspicion of the adequacy of the formulæ usually put forward—a hesitation arising from the fact that the correcting formula gives impossible values for the correction when used outside the limits for which the approximation is valid. It need hardly be said that the important matter is to adjust the experimental conditions so that those conditions are fulfilled under which the formula has been deduced. Thus, defining a^2 by the equation $a^2 = \mathrm{T}/g\rho$, where T is the surface tension, and ρ the density of the liquid under test, a^2 is usually expressed in terms of a series in ascending powers of r/h, where r is the radius of the tube and h the " ascent-height," as defined above, of the liquid employed. Now Poisson [3] has shown that, including terms in r^2/h^2, we may write

$$2a^2 = rh\left(1 + \tfrac{1}{3}\frac{r}{h} - 0{\cdot}1288\frac{r^2}{h^2}\right), \quad . \quad . \quad . \quad \text{(ii)}$$

and Rayleigh [4] has obtained a third-order correction involving a term in r^3/h^3. Restricting ourselves to equation (ii), it is evident that the condition under which this may be legitimately

employed is that r^2/h^2 shall be negligible in comparison with unity, *and unsatisfactory values deduced from an application of the formula outside this range are no test of its correctness or incorrectness within the range.*

It is not without interest to inquire how, within this range, the empirical correction of equation (i) agrees with Poisson's formula as given in equation (ii). It is easy to show [5] that, for a liquid of zero contact-angle, the approximate equation to the meridional curve of the surface of the liquid in a capillary of radius r is

$$y = r - \sqrt{r^2 - x^2} + \frac{r^2}{3a^2}\log_e \frac{r + \sqrt{r^2 - x^2}}{2r} \quad . \quad . \quad \text{(iii)}.$$

When $x = r$, we have the value of y contemplated by Richards and Coombs, which is therefore

$$y = r - \frac{r^2}{3a^2}\log_e 2 \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(iv)}.$$

The "corrected" value of h is $h + y/3$, and consequently we have as our working formula

$$2a^2 = r(h + \tfrac{1}{3}y) = rh\left(1 + \tfrac{1}{3}\frac{y}{h}\right) \quad . \quad . \quad . \quad . \quad \text{(v)}.$$

The value of y given by (iv) is of the first order in r^2/a^2, that is, is of the first order in r/h. As we are substituting this value in a *small* term in (v) our approximation is sufficiently exact for the calculation of second-order terms in (v). We thus obtain

$$2a^2 = rh\left[1 + \frac{1}{3h}\left(r - \frac{r^2}{3a^2}\log_e 2\right)\right] \quad . \quad . \quad . \quad \text{(vi)},$$

and substituting in the small term the approximate value $a^2 = \frac{1}{2}rh$, we easily find that

$$2a^2 = rh\left(1 + \tfrac{1}{3}\frac{r}{h} - \frac{2}{9}(\log_e 2)\frac{r^2}{h^2}\right) \quad . \quad . \quad . \quad \text{(vii)}.$$

In Poisson's formula (ii), the coefficient of r^2/h^2 is $-0{\cdot}1288$; in equation (vii) the coefficient of r^2/h^2 is $-0{\cdot}1540$, and this gives the measure of the discrepancy.

Sugden [6] keeps down the quantity of liquid used by employing a narrower "wide" tube, and calculates the meniscus correction in the wide tube by an ingenious use of the tables of Bashforth and Adams [1].

Kiplinger [7] places a small quantity of liquid in an open capillary tube, and *tilts* the tube until its axis makes an angle

α with the vertical, the tilt being such that the meniscus at the lower end is *plane*. If l is the length of the column of liquid employed, then, small corrections apart,

$$T = \tfrac{1}{2} r l \rho g \cos \alpha.$$

The values obtained are uniformly low and the percentage error is considerable (from 1·5 to 4·8 per cent.).

The writer of this article has described a method [8] in which a small quantity of liquid is placed in a vertical tube, and the pressure ($g\rho h$) is measured which is required to force the liquid to that position in the vertical tube at which the meniscus at the lower end of the tube is plane. If the density of the liquid under test is ρ_1 and the length of the column of liquid is h_1, then

$$2T/gr = \rho h + \rho_1(h_1 + r/3) \quad . \quad . \quad . \quad . \quad . \quad \text{(viii)}.$$

It thus becomes possible to make precise determinations of surface tension, employing as little as a cubic millimetre of liquid. It has been objected that, given only a few cubic millimetres of liquid, the determination of the density of the liquid becomes a matter of no small difficulty. To this objection it may be pointed out that in most capillary tube determinations of a surface tension we are mainly concerned with the measurement of a *pressure* and a *curvature*; and in the ordinary capillary-rise experiment the density of the liquid under test comes in, *simply because the liquid acts as its own manometer*. In equation (viii), the density of the liquid under test enters the equation in quite a secondary manner; in fact, by decreasing h_1 sufficiently we can make the term containing the density quite unimportant without appreciable sacrifice of precision. Alternatively, by varying h_1 and consequently h, we see that a plot of ρh against h_1 gives a straight line from the slope and intercept of which we are able to infer both the surface tension and the density of the liquid under experiment. The method may be applied to the determination of the surface tension at the interface of two small quantities of liquid.

Damerell [9] has evolved a micro-method for the simultaneous determination of surface tension and density which consists in measuring the capillary rise in a fine capillary (of radius 0·165 mm.) when dipped into a sample of the liquid contained in a tube of about 0·7 cm. diameter and about 0·5 cubic centimetres in capacity. The density of the liquid under test was determined by weighing a thread of known length contained in the capillary tube.

The method of Lecomte du Nouy [10] is described in detail by Dr. Bakker, and specifically mentioned as demanding the

"Benutzung einer kleinen Quantität flussigkeit." In the practice of this method a platinum ring 4 cm. in *circumference* is suspended with its plane horizontal from a torsion balance, brought into contact with the liquid and raised until a film is formed. "The torsion of the wire is used to counteract the tension of the liquid film and to break it."

It is evident, therefore, that a growing literature is accumulating around the problem of the determination of the surface tension of a small quantity of liquid, and it would be well in a comprehensive treatise to devote some little space to the problem.

Another problem which, unless we have overlooked the section devoted thereto, is passed over by Dr. Bakker is that of the ascent of liquid between two vertical plates which meet each other at a small angle. The method deserves some attention, if only on account of its antiquity. The hyperbolic nature of the curve of contact of the liquid with the plates was very early recognised by Brook Taylor [11] and by Hauksbee [12]. A practical apparatus for measurement is figured in 's Gravesande's book [13], and the description of the apparatus and proof given by Musschenbroek [14] deserve citation for their clearness and compactness. "Si ambo specula plana, ad solum perpendicularia, uno latere jungantur, ut se contingant; opposito latere distiterint, angulumque forment, hæc imposita Aquæ, eam altius attollent, in locis, quibus specula sint propriora; minus ubi specula distant; elevatæ aquæ inter specula superficies formabit curvam, Hyberbolam, cujus Asymptotæ erint speculi latera CD, DE; est enim Da ad Db, ut distantia speculorum aa^1 ad bb^1; sed est altitudo Bb ad Aa in ratione inversa distantiorum in his locis; sive est uti Da ad Db: adeoque erit Da × Aa = Db × Bb. Quæ est proprietas Hyperbolæ." [1]

Grunmach [15] has given much attention to the improvement of the technique and accuracy of this method. In his earlier apparatus the x, y co-ordinates of various points on the hyperbolic curve are measured by means of a kathetometer, with reference to axes taken, one in the general level of the liquid, and one along the vertical line of junction of the plates. The values of the product xy, which should be constant within the limits of experimental error, show traces of a systematic variation. Grunmach, however, uses the mean value in order to determine the surface tension. In his later experiments

[1] CD represents the (vertical) line of junction of the plates, DE the horizontal edge of one of the plates immersed in the liquid considered. Da and Db are of length x and $x + \delta x$ respectively, so that a and b are neighbouring points on the line DE. Aa and Bb represent the heights to which the liquid ascends between the plates at the points a and b respectively.

Grunmach photographs a series of confocal hyperbolæ on one of the plates, and adjusts the angle between the plates until the curve of the liquid surface coincides as nearly as may be with one of the hyperbolæ. The writer of this article and a colleague have shown [16] that it is possible to avoid the difficulty of estimating correctly the positions of the axes—which is the most serious difficulty in the practice of the method—by placing the plates vertically and with their line of junction vertical in a vessel containing the liquid under test, and measuring the xy co-ordinates of points on the curve of separation of the liquid from air relative to any arbitrarily chosen horizontal and vertical axes. The simple formula then becomes

$$(x + h)(y + k) = \mathrm{K},$$

and graphical methods are known [17] by means of which the constants h, k, and K can be calculated with an accuracy comparable with that of the measurement of the experimental quantities. In this way a value of 73·32 dyne-cm.$^{-1}$ was found for the surface tension of water at 15° C., and of 28·74 dyne-cm.$^{-1}$ for the surface tension of benzene at 20° C. A disadvantage of the method is that a comparatively large quantity of liquid is required.

The velocity of flow of the free surface of a liquid in a capillary tube offers some interesting problems, including a possibility of the simultaneous determination of surface tension and viscosity. The literature associated with this particular problem is far from small, but it is not discussed by Dr. Bakker. Washburn [18] showed some time ago that the relation between the distance, x, traversed in a time, t, by the meniscus of a liquid surface advancing in a horizontal capillary tube of radius r under a constant head was given by

$$2\eta x^2 = \mathrm{T}\cos\theta\, rt$$

where η is the viscosity of the liquid, T its surface tension, and θ its contact angle. Rideal [19] has studied such flows experimentally under a small pressure head and has obtained, for a number of different liquids, good agreement between the observed and calculated values of the "penetration coefficient" $(\mathrm{T}r/2\eta)^{\frac{1}{2}}$. A discussion of the theory of the advance of the meniscus through horizontal capillary of radius r under a constant pressure, P, is given by Bosanquet [20], who, taking the accelerating force as $\pi r^2\mathrm{P} + 2\pi r\mathrm{T}$, the viscous retarding force as $8\pi\eta x\dot{x}$, and the momentum of the column (of length x) as $\pi r^2\rho x\dot{x}$, thus obtains

$$\pi r^2\mathrm{P} + 2\pi r\mathrm{T} - 8\pi\eta x\dot{x} = \frac{d}{dt}(\pi r^2\rho x\dot{x}).$$

This gives

$$\frac{d}{dt}(x\dot{x}) + ax\dot{x} = b,$$

where

$$a = \frac{8\eta}{r^2\rho},\ b = \frac{Pr + 2T}{r\rho}.$$

Finally,

$$x^2 - x_0^2 = \frac{2b}{a}\left[t - \frac{1}{a}(1 - e^{-at})\right].$$

The author extends the equation to the problem of two fluids in contact, and to a liquid flowing into an inclined tube.

Perhaps the most interesting of the investigations carried out on this subject is that of Dr. Emma Müller [21], who, working in the laboratory of Prof. Gustav Jaeger, has made simultaneous determinations of viscosity and surface tension from observation of the rate of fall of the meniscus in a vertical tube dipping into the liquid under test. If h_0 is the equilibrium height of the meniscus, and h the height of the meniscus ($h > h_0$) at a time, t, then, assuming Poiseuille's law to hold,

$$(h_1 - h_0) + (h_0 + a)\log(h - h_0) = -bt + K,$$

where K is a constant of integration given by the initial conditions and b is written for $g\rho r^2/8\eta$. This equation [1] holds good if the cross-sectional area of the capillary is negligible in comparison with that of the containing vessel. Otherwise a simple correction has to be made. The method seems to be simple to carry out in practice, and to give reliable results. Thus, for water at 20° C., using a tube of radius 0·0103 cm., the author finds for T the value 71·68 dyne-cm.$^{-1}$, and for η the value 0·01029 gm-cm.$^{-1}$-sec.$^{-1}$. The method has been used to study the variation of surface tension and of viscosity with concentration for glycerin-water mixtures.

Dr. Bakker's interests are curiously indicated by the fact that the term "*Randwinkel*" is not entered in the *Sachregister*. Not that the term is not used. In the discussion of the capillary surface inside a narrow tube, and of the shape of large bubbles and drops, the usual expressions are formed and their dependence on the contact-angle carefully examined. But for forty years past, determinations, direct and indirect, of the magnitude of contact angles have been carried out, and it is surprising to find in a volume of such weight as the present one not even a passing mention of such determinations. Thus Magie [22] has measured the total depth of a large bubble of air imprisoned under a concave lens and has thus determined

[1] a is the depth of immersion of the tube in the liquid in the containing vessel.

the contact angle with glass of a number of different liquids. Fürth [23] has, from observations of a convex surface partially covered with mercury, deduced the contact angle of mercury with glass. Anderson and Bowen [24] have measured optically the radius of curvature (ρ) at the vertex of the capillary surface inside a tube of radius r. The observations are repeated for tubes of different radii and a curve is plotted between r and ρ. Since in the limit the ratio r/ρ measures the cosine of the contact angle it is clear that at least an approximate value of this angle may be obtained by drawing the tangent at the origin of the r-ρ curve. They find in this way that the contact angle of mercury with glass is 139°. Richards and Carver [25] have studied contact angles by observing the reflection of light-rays in the immediate neighbourhood of the line of junction of a liquid with glass. They find that the angle of contact between glass and water is very accurately zero. Ablett [26] has described an extremely interesting series of observations on the contact angle between paraffin wax and water, and the variation of the contact angle according as it is measured on a rising, falling, or stationary meniscus. A cylinder, having its axis horizontal, is coated with paraffin wax and is immersed in water to such a depth, h, that the water surface as tested optically is plane right up to its line of contact with the curved surface of the cylinder. If the diameter of the cylinder is d, clearly the contact angle is given by

$$\cos \theta = (2h - d)/d.$$

By rotating the cylinder about its horizontal axis it becomes possible to measure, θ_1, the contact angle corresponding to a rising, and θ_2 corresponding to a falling meniscus. Ablett finds that as the speed increases from 0·13 to 0·44 millimetres per second peripheral velocity, θ_1 increases with speed, and θ_2 diminishes, but always so that $\theta_1 + \theta_2 = 2\theta$, where θ is the contact angle for a stationary meniscus. From 0·44 mm. per second to 3·9 mm. per sec.—the highest speed observed—θ_1 and θ_2 remain constant. His final results are:

$$\theta = 104°·34'\,;\ \theta_1 = 113°·9'\,;\ \theta_2 = 96°·20'.$$

Of the various methods proposed for the determination of a surface tension no one method has accumulated so vast a mass of literature around it as the so-called drop-weight method. Dr. Bakker gives a very full account of the important work of Lohnstein [27], describes briefly the researches of Rayleigh [28] and of Harkins [29], and restricts himself, as far as detailed descriptions of practical methods are concerned, to the stalagmometer of Traube. There are a number of problems of great

interest—historical and practical—associated with the drop-weight method, some of which are touched on very lightly by Dr. Bakker, and it may not be without interest to consider one or two points in detail. Thomas Tate [30], of Hastings, pointed out that the weight of a detached drop was proportional to the radius of the tube from which the drop was detached, and the method was very early used by Quincke [31] to determine the surface tensions of ordinary liquids and of molten drops of metal falling from the ends of thin vertical rods. It was until relatively recently the common practice to assume that the upward pull, $2\pi r T$, due to surface tension balanced the weight, mg, of the detached portion of the drop so that

$$2\pi r T = mg \quad . \quad . \quad . \quad . \quad \text{(ix)}.$$

In establishing the equation in this way, it is hardly ever overtly stated—indeed the point is usually completely ignored—that, quite apart from the difficulties introduced by the fact that the detachment of the drop is a dynamical phenomenon, this equation assumes that the curvatures at points on the surface in the plane of rupture are so adjusted as to leave no pressure excess at this level in the interior of the drop. With tubes of the dimensions ordinarily employed, equation (ix), used as an absolute equation, would give results close on 100 per cent. in error. Rayleigh [28], after emphasising the erroneous nature of equation (ix), points out that if the drop be assumed cylindrical at the line of contact with the tube

$$mg = \pi r T \quad . \quad . \quad . \quad . \quad \text{(x)}$$

represents a closer approximation to the facts. Nevertheless, it is important to notice that Quincke, who was not likely to overlook so obvious a point as the importance of inserting proper expressions for the pressure-excess due to curvature in the equations of equilibrium of the drop, used equation (ix) and obtained for water a value of 6·3 milligrammes per millimetre—low, certainly, but not by any means 100 per cent. in error. It may be profitable, then, to examine Quincke's actual procedure, especially since the $2\pi r T$ tradition begins with him and has been uncritically continued for some generations of textbooks by the simple process of equating $2\pi r T$ to mg without pausing to consider the possibility of entry of other terms into the equation. After writing the equation to the capillary surface in the well-known form

$$g\rho z = T\left(\frac{1}{R} + \frac{1}{R_1}\right),$$

he substitutes for R and R_1 the ordinary expressions which

these quantities assume when the capillary surface is one of rotation. He then proceeds to calculate the weight of liquid which is raised above the "allgemeine Niveau" when a hollow vertical cylinder of radius r is dipped therein. He finds that, if G is this weight, then, as is otherwise obvious,

$$G = 2\pi r \mathrm{T} \cos \omega$$

where ω is the contact angle. If ω is zero, then

$$G = 2\pi r \mathrm{T} \quad . \quad . \quad . \quad . \quad \text{(xi)}.$$

He then proceeds to point out that "Der durch Gl. (xi) gegebene Satz gilt nun auch noch für Tropfen, die sich an der Ausflussöffnung einer vertikalen Rohre bilden unter der Annahme, dass im Innern der flussigkeit an der Ausflussöffnung wegen des allmähligen Zuflusses von neuer Flussigkeit-*derselbe Druck stattfindet, wie in einer ebenen Flussigkeits-oberfläche*. Der Tropfen wird so lange wachsen, bis . . . das hochste Element der Flussigkeitsoberfläche vertikal ist, und dann wird er abfallen. Ist der Radius des Cylinders, an welchem sich der Tropfen bildet, sehr klein, so kann man das gewicht des hangen bleibenden Theiles der Flussigkeit vernachlassigen, und das Gewicht des abgefallenen Theiles des Tropfens als gewicht G in der Gl (xi) in Rechnung bringen." Although Quincke's results are low, they involve a multiplier of rT which is certainly considerably greater than π, and it remains to be considered whether, with still smaller cylinders than those employed by Quincke for any given liquid, the multiplier of rT will more closely approach 2π.

We shall return to this point later.

Rayleigh [28], after discussing the substitution of π for 2π in equation (xi), proceeds to a dimensional argument. We assume that the mass of a falling drop depends on the density (ρ), the surface tension (T), the acceleration due to gravity (g), and the linear dimensions of the tube (r). For liquids such as water r may be taken as the external radius of the tube, and it is premised that the ratio of the internal radius to r shall remain constant. We have then

$$\mathrm{M} \alpha \mathrm{T}^x g^y \rho^z r^w,$$

leading at once, by equating dimensions of mass, length, and time, to

$$\mathrm{M} \alpha \frac{r\mathrm{T}}{g}\left(\frac{\mathrm{T}}{g\, r^2}\right)^{x-1}$$

or

$$\mathrm{M}g = r\mathrm{T}\phi\left(\frac{\mathrm{T}}{g\rho r^2}\right) = r\mathrm{T}\psi\left(\frac{r}{a}\right), \quad . \quad . \quad . \quad \text{(xii)},$$

if, as usual, we write a^2 for $T/g\rho$. If now, using a liquid of *known* surface tension, we determine the weight of a drop falling from a tube of known radius and repeat the experiment for tubes of different radii it becomes possible to determine the value of the function $\psi (= Mg/rT)$ for different values of the independent variable r/a. In this way Rayleigh obtained the table given below.

$$\frac{a^2}{r^2} : 2{\cdot}58,\ 1{\cdot}16,\ {\cdot}708,\ {\cdot}441,\ {\cdot}277,\ {\cdot}220,\ {\cdot}169.$$

$$\psi\left(\frac{a^2}{r^2}\right) : 4{\cdot}13,\ 3{\cdot}97,\ 3{\cdot}80,\ 3{\cdot}73,\ 3{\cdot}78,\ 3{\cdot}90,\ 4{\cdot}06.$$

It will be seen that there is no very serious fluctuation in ψ over a very wide range of the independent variable, and that it is sufficient for tubes of ordinary dimensions and liquids of the common range of surface tension to put

$$Mg = 3{\cdot}8rT \quad . \quad . \quad . \quad . \qquad \text{(xiii)}.$$

Lohnstein's work [27] comes next in chronological order; as the result of a laborious analysis an equation

$$Mg = 2\pi rTf\left(\frac{r}{a}\right) \quad . \quad . \quad . \quad . \qquad \text{(xiv)},$$

which is closely analogous to Rayleigh's equation (xii).

It is clear that the use of this equation is much simplified if the quantity, a, which involves the surface tension, T, can be replaced in the function, f, by a quantity of the same dimensions which may be determined in the course of the drop-weight experiment. Such a quantity is the cube root of the volume of the detached drop, and Harkins and Brown [29] consequently put

$$Mg = 2\pi rT\, F\left(\frac{r}{V^{\frac{1}{3}}}\right) \quad . \quad . \quad . \qquad \text{(xv)}.$$

The convenience of the change is obvious. If equation (xv) holds good, then all liquids which have the same value of $r/V^{\frac{1}{3}}$ will have the same value of $Mg/2\pi rT$. Suppose, then, that, using the drop-weights of certain liquids of known surface tension, we plot a curve between $r/V^{\frac{1}{3}}$ and $F(r/V^{\frac{1}{3}})$. This curve is a perfectly general one, and if, therefore, we now make a drop-weight experiment with a liquid of unknown surface tension so that we know Mg and also V, we have only to calculate the value of $r/V^{\frac{1}{3}}$, and look up the corresponding value of $F(r/V^{\frac{1}{3}})$. This value of F is identical with $Mg/2\pi rT$, and, as Mg and r are known, we obtain the value of T.

All this, however, depends on the legitimacy of the sub-

stitution of $V^{\frac{1}{3}}$ for a. That the two quantities should have the same dimensions is a *necessary* condition, but it by no means follows that it is a sufficient condition. This requires independent investigation, and a simple dimensional argument will serve to show whether the substitution is justifiable and the conditions under which the substitution holds good. If we assume that the weight of a detached drop depends on the density, (ρ), the volume, (V), the surface tension, (T), and the radius (r) of the tube from which the drop falls we have

$$Mg \propto r^x T^y \rho^z V^w,$$

which at once gives

$$Mg = krT\left(\frac{V}{r^3}\right)^w = krTF\left(\frac{r}{V^{\frac{1}{3}}}\right).$$

Harkins and Brown, putting the non-dimensional constant k equal to 2π, have, by means of experiments carried out on liquids of known surface tension, tabulated a series of values of r/a and $f(r/a)$ (equation xiv) and have done the same for $r/V^{\frac{1}{3}}$ and $F(r/V^{\frac{1}{3}})$ (equation xv).

This is, in effect, repeating Rayleigh's pioneer work with greater accuracy and over a wider range of the independent variable.

Returning now to the problem of the conditions under which Quincke's simple equation (xi) may hold we see that in the equation

$$Mg = 2\pi rTf\left(\frac{r}{a}\right),$$

the value of r/a must be such that $f(r/a)$ is unity. In Rayleigh's experiments the lowest value of r/a used was about 0·6 ; the experiments of Harkins and Brown cover a range of r/a from 2·20 to 0·025. We show below the values of $f(r/a)$ corresponding to the lower numbers of the series, and it will be seen that at the lower levels the value of $f(r/a)$ increases very rapidly.

$\frac{r}{a}$: 1·0, ·90, ·80, ·70, ·60, ·50, ·40, ·30, ·20, ·10, ·025.

$f\left(\frac{r}{a}\right)$: ·599, ·604, ·610, ·620, ·633, ·652, ·676, ·705, ·741, ·805, ·924.

It will be noticed, first, that the earliest member of this series quoted almost overlaps with Rayleigh's series, and that ·6 × 2π is just under 3·8, in satisfactory agreement with Rayleigh's result. Second, that $f(r/a)$ ascends in value very rapidly when r/a is small. Whether it is legitimate to assume that the limiting value of $f(r/a)$ as r/a approaches zero is unity,

is a doubtful question. But it is interesting to note that with water (for which a may be taken as about 0·27 cm.) as the liquid under test, a tube for which r/a is unity, that is, a tube about five millimetres in external diameter, will provide a drop whose weight is given by

$$Mg = (0{\cdot}6) \times 2\pi r T,$$

that is, by the ordinary Rayleigh formula (xiii). At the other end of the scale a tube for which r/a is ·025, that is, a tube about 0·14 mm. in diameter, gives a drop whose weight is determined by

$$Mg = (0{\cdot}924) \times 2\pi r T,$$

which is considerably closer to the simple formula (xi) used by Quincke.

It should be emphasised that the drop-weight method used in conjunction with equations (xii), (xiv), or (xv) gives results of very high precision, but the success of the method is conditioned by the perfection of the tip used and above all by the slowness of formation of the detached drop. Abonnenc [32] has shown that if N is the number of drops falling per second from a given tube the mass of a detached drop is given by

$$m = a + bN - cN^2,$$

and Harkins and Brown show that it is not until the natural period of formation and fall of a drop is of the order of 4 or 5 minutes that the weight of the detached drop is independent of the time of formation. It is very evident, then, that those instruments which depend on drop-number rather than on drop-weight will not be reliable. If a large number of drops is to be counted it is impossible to give to the formation of each drop a period of time greater than a few seconds, and comparable figures thus become difficult to obtain.

If, in this essay, we have dwelt at length on some topics that have been overlooked, or but briefly discussed, by Dr. Bakker we have done so mainly because of their intrinsic interest. There are other subjects which we would like to discuss did space permit. The variation of surface tension with temperature; the relation of *total* molecular surface energy and temperature; the work of Kleeman; relations between surface tension and density; the properties of the parachor; the relation of free energy, total energy, and temperature, as discussed by A. W. Porter,—these are but a few of the topics, some of which Dr. Bakker has discussed at length, some of which he has passed over, and concerning all there are debatable points of considerable interest. We trust that the reader will understand that we are not ungrateful for the

volume which Dr. Bakker has written. It represents a great mass of positive achievement, and is indispensable to all workers in this field. It is morally certain that a new edition of the work will very soon be called for, and the hope that the author will put us more deeply in his debt by providing us with fuller information on certain topics is one factor which has prompted these remarks.

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THE STRUGGLE FOR SUPREMACY. By H. F. Barnes, B.A., Ph.D., being a review of: (1) **The Principles of Applied Zoology,** by Robert A. Wardle. [Pp. xii + 427.] (London: Longmans, Green & Co., 1929. Price 21*s*. net.) (2) **The Problems of Applied Entomology,** by Robert A. Wardle. [Pp. xii + 587.] (Manchester: at the University Press, 1929. Price 30*s*. net.) (3) **The Scientific Principles of Plant Protection,** by Hubert Martin. [Pp. xii + 316.] (London: Edward Arnold & Co., 1928. Price 21*s*. net.)

From time to time in the history of the world the attention of the man in the street has been attracted to some great struggle. Sometimes the onlookers are so keenly interested

that they take an active part, at others they look on with something akin to derision. Sometimes interest is universal, at other times only individual.

Struggle is the very essence of life. First struggle for existence, then struggle for supremacy. All life on an island is destroyed by volcanic action; very soon seeds are drifted to its barren shores. Finally, perhaps after several failures, the plants get a footing. One plant is more successful than some other and gains more ground; another plant follows and encroaches on the ground that the other plant has previously monopolised; such is the beginning of ecological succession. The arrival of animal life complicates matters. Then comes Man. With his advent the struggle is intensified, plants and animals being used by him for food, for clothing, for ornament, and for pleasure. Man, unlike animals and plants, looks after weaklings of his own race. In this way is often evolved a man who cannot always see even the end of his own nose.

Therefore unnoticed, unrealised, and so often ignored, except by a few, is the contest which is perpetually going on, the struggle for supremacy between man and the rest of the animal kingdom, with weeds as an occasional ally of the animals. At intervals men rise up with clearer vision to point out problems and principles; they attract but little attention. Prof. Wardle in *Principles of Applied Zoology* scans the ever increasing field. He has compiled a book which deals with the animal kingdom in relation to man. The book is divided into three parts; the first deals with medical and veterinary zoology. In this, one can read of Protozoa that live in the alimentary canal of animals and man, some acting as scavengers, others that cause such diseases as amœbic dysentery, or of Protozoa that live in the circulatory system causing diseases such as sleeping sickness, malaria, and syphilis. The Flukes, Tapeworms, and Roundworms next come under review. These animals are responsible for diseases such as "liver rot" of sheep, "grouse disease" of grouse, and ancylostomiasis of man. Almost any part of the animal body—the alimentary canal, the lungs, the muscles, and the circulatory system—is subject to attack by one or more of these Helminth parasites. The pathological aspect of Helminth infection is considered and it is pointed out that the presence of the parasite may cause no apparent inconvenience, or general malady or localised lesions. Arthropods, which here include insects, spiders, scorpions, ticks, mites, centipedes, and millipedes, as well as crustacea, are dealt with next. Arthropods may actually put some toxic substance in the animal host, this is toxic inoculation, or else they may be actually present in the host themselves, myiasis, or they may carry some organism with which to infect the host by

mechanical transmission, or finally they may act as an indispensable host to some organism before putting it into the animal host, in which case they are cyclic transmitters.

In the second part the subject is agricultural and horticultural zoology. Here one reads of soil organisms, insect pests, vermin repression, bird encouragement, animal domestication, types and breeds of farm animals, and livestock feeding. The insects naturally take up more space than the other subjects and are dealt with in a remarkably clear manner. They are put into their categories and their life-cycle is explained. Separate chapters are devoted to their distribution, behaviour, and mortality factors. Man must consider himself lucky that the survival potential of insects falls short by a long way of the reproductive potential, when he is reminded that a pair of house flies could produce 5,598,860,000,000 flies in seven generations, *i.e.* by the end of a single season, especially as he should know that house flies are disease carriers.

The third part is devoted to animal industries. Bee-keeping, sericulture, and lac culture are dealt with in two chapters. Then fishing (fresh water, estuarine, inshore, and offshore), whaling and sealing come under notice. A few diseases of fish are explained. Finally the writer deals with fur-bearing animals, the trade and fur-farming, and ends with a chapter on animal conservation.

Thus one is treated to an accurate well-written account of the whole field of applied zoology. The interest is never allowed to sag, and after reading the book one has to admit that even if nothing is new in the world many things are unknown.

Man perhaps can ornament himself at the expense of some animals such as fur-bearing animals and live on some animals without apparently injuring himself, but when it comes to insects it is a different story. One has seen how insects carry disease to man. They also have another means of attack, that of spoiling man's food. Insects are therefore perhaps the most dangerous of all man's enemies. Prof. Wardle's second book, *The Problems of Applied Entomology*, has two main parts. The first deals with general problems such as the resistance of the host to insect attack, the resistance of insects to climatic factors, disease, and parasites and predators. The behaviour of insects to factors of environment, such as temperature, light, chemical substances, and moisture, is dealt with under four headings: trophic behaviour concerned with food finding, sexual behaviour concerned with mating, oviposition behaviour, the search for suitable oviposition media, and motor behaviour concerned with purposeless movement as distinct from the movements concerned in the three other

categories. Having so dealt with the nature of the enemy, Prof. Wardle deals with insecticides. The theory is explained and stomach poisons, contact insecticides, fumigants, and combination insecticides are reviewed. It is here that Mr. Martin's book, *The Scientific Principles of Plant Protection*, comes in as a valuable companion volume. The author takes the view that in order to save himself man must protect his plants. Just as in Prof. Wardle's book the insect is the central figure, so is the plant in Mr. Martin's book. Plant resistance to insects and fungi can sometimes be increased by the production of resistant varieties; nutritional and climatic factors also play an important part. As one would expect from a chemist, fungicides and insecticides receive more detailed attention in this latter book. The references to literature on the subject immediately dealt with appear at the end of each section instead of at the end of the book as in Prof. Wardle's books. There is much to be said in favour of both arrangements.

After dealing very fully with fungicides, insecticides, and fumigants, Mr. Martin deals with seed treatment and soil treatment. Two chapters are included which deal with biological control and another one deals with traps, chemotropic, phototropic, and stereotropic, and poisons for use in traps. Finally the book ends with a chapter on the treatment of the centres and vectors of infection. The book should be remarkably useful to all who wish to protect plants against disease, whether insect, fungous, or any other. Particularly useful will it be to chemists concerned. Mr. Martin, we consider, has accomplished two of his objects very successfully—to present to the mycologist and entomologist a view of the chemical and physical aspects of the control of crop pests and to provide for teacher and student of Agricultural Chemistry a book of reference upon Insecticides and Fungicides. He has not, however, done quite so well in his attempt to present to the view of the chemist and physicist a means of approach to the biological side, especially with regard to entomology. One must realise, however, that Mr. Martin only claims to give the chemist and physicist a means of approach, whereas he claims to give the entomologist and mycologist a view.

In the second part of his book on applied entomology Prof. Wardle deals with Area Problems. For example, he takes an area such as the Mediterranean and deals with its particular problems, the Olive Fly, the Vine Moth, Phylloxera, the Pink Bollworm, the Dusky Cotton Bug, Citrus insects, and other problems of the area, both agricultural and medico-veterinary. In this way the problems of the world are regionally discussed. Owing to this method of treatment, problems

such as that of malaria which occurs in more than one area are dealt with separately, and in order to obtain a real grasp of the whole problem one has to refer backwards and forwards. This, however, is a minor point. The two concluding chapters are on locality disinfection and locality protection, the word locality being used to denote any restricted environment in which an insect may occur, *e.g.* a sack of grain, a flower bed, a particular field, or a district, or even a country. The bibliography is good. The most valuable part of this book is the clear way in which the author has presented to entomologists of one country the problems which face his fellow-workers in other countries.

The authors of these books have done excellent and much-needed work in bringing together such complete views of applied biology. Although it is not realised among laymen and by many scientists, what is needed to-day are men of wide vision to collate the results of the many research workers, to sift the gold from the dross, and to present their results in a form that appeals and attracts. Prof. Wardle has done this in the field of applied zoology; he has also done it in applied entomology, and in addition set out clearly the problems (a rather remarkable piece of work for one book), while Mr. Martin has shown us the principles of plant protection.

We recommend these three books as models of what is needed to-day, just as much as original research. There is a serious danger that through over specialisation research workers fail to keep in touch with what is being done in other fields.

THE ORIGIN OF MAN. By B. R. M. Saner, B.A., being a review of **The Travels and Settlements of Early Man**, by T. S. Foster, M.A. [Pp. 320.] (London: Ernest Benn, 1929. Price 21*s*. net.)

Modern science, growing up since the Renaissance, is marked by its specialisation. This specialisation is the outcome of the enormous increase in our ordered knowledge of nature; Aristotle was the master of all the science of his day, because the natural phenomena observed, and the facts proved, were relatively few.

The study of man as a natural phenomenon occupies several branches of modern science. Anthropology, *sensu stricto*, is the Science of Man, but the meaning is usually limited to the study of man as a unit in the animal kingdom, while ethnology is concerned with man as a racial unit. But the distribution of races is ethnography, and anatomy, physiology, psychology, philology and sociology all contribute to the knowledge of man. Archæology is concerned with the remains, the material results of his activities in the past, and geography studies his activities in relation to his surroundings to-day.

Formerly the Church undertook to account for man's origin as well as his future. It was only after a human and naturalistic view of the universe began to replace the mystical aspect encouraged by the Church in the Dark Ages that speculation as to man's origin and the origin of the lower animals finally led up to Darwin's work, and the theory of evolution became a working hypothesis.

The outlines of man's physical evolution are now understood, so far as the present evidence will allow. Archæological discovery has also proved his antiquity, and furnishes the basis for the study of his social and intellectual evolution. But many problems in connection with the differentiation of races, and the distribution of cultures, remain unsolved.

At the present time there is a tendency to break down the barriers between related branches of science, and to correlate the knowledge relating to one or more aspects of the same phenomenon. Such borderline studies often form important contributions to science, but if they attempt to cover too wide a field they appear unsatisfactory to the specialist. Another outcome of specialisation in modern science has been the growth of "popular" writings, works of more general character which outline the results obtained by detailed work.

The Travels and Settlements of Early Man, by T. S. Foster, belongs, in part, to both these classes. It is, according to the subtitle, a study of the origins of human progress, and is written from the point of view of a student of education—a history of primitive peoples as they approach civilisation. In his introduction Mr. Foster reviews Historical Method, showing that the interpretation of historical facts depends on the philosophy of the historian. He suggests that the study of history to-day is the study of education.

The first two chapters of the book are concerned with the origin of *Homo sapiens*, correlating the evidence of Tertiary geology and climatology with that of archæology. He describes widespread faunal migrations resulting from physiographic changes. There is a tendency to gloss over the incomplete nature of the evidence in order to complete the story. In the following three chapters Mr. Foster describes the succession of cultures in Europe during the late Pleistocene and the origin of racial types in Asia and Africa.

The second half of the book is concerned with the settlement of America by emigrants from the coasts of Asia and the succession of peoples who entered the Pacific Islands. Mr. Foster stresses the importance of the part played by the Anatolian trading race, as an agent for the widespread distribution of a megalithic, metal-using culture through the Pacific to America, as well as in Europe.

There are no illustrations; a few diagrams showing lines of movement, and the controlling geographical factors would have supplied a summary of the narrative. There is, however, a useful bibliography for each chapter.

Mr. Foster's book is very general in character, covering a wide field; because it is largely descriptive it would have been improved had the facts been related to some dominant theme. Nevertheless, it gives a panoramic sketch of the history of primitive races.

BELLS AND BELL RINGING. By Dr. E. G. Richardson, being a review of "**Bells Thro' the Ages,**" by J. R. Nichols. [Pp. xi + 320 with 129 illustrations.] (London: Chapman & Hall, 1928. Price 21s. net.)

The vibration of bells presents many points of interest and several of difficulty. Thanks to Chladni the study of the vibrations of plates supported at the centre is comparatively simple. It is even possible to bend the plate and still its acoustics is not difficult. The modes of vibration of such bent plates as finger-bowls and violin backs have been brought into line with theory. But as soon as the metal is moulded into a shape of varying curvature and thickness, such as the church bell takes, it is another story. As Prof. Taber Jones remarks, "Even on so elementary a matter as the number and position of nodal lines the very meagre published values are not in satisfactory agreement. Moreover, the different partial tones of a single bell usually bear inharmonious relations to each other, and the relative frequencies of the partial tones in one bell often differ from the relative frequencies in other bells—even when the bells were cast by the same founder." (*Phys. Rev.*, p. 1092, 1928.)

The present bell shape has been reached after many years of cut and try methods, and it is uncertain whether the shape is governed by acoustic desiderata, or by considerations of economy of metal and security in moulding. In Germany, during the late War, when everything had to be *ersatz*, Herr Biehle obtained considerable success in founding steel bells. Mr. Nichols has a drawing of an ancient bell from Manningford Abbots which is longer in proportion to its circumference than the present one, and of uniform thickness of section, and which is thought by some to be of superior tone. One would like records of the partial tones of such old bells. Nowadays, the crown of the bell is flattened, while the thickness of section continually increases down to a level near to the base of the bell where the inside and outside rapidly diverge, and then converge upon the rim. This level where the inside and outside surfaces have opposite curvature is known as the "sound-

bow," and plays an important function in determining the pitch and timbre of the bell. This fact is the more interesting as the old bells had no "soundbow." The maker tunes a bell by shaving off metal on the inside of the soundbow by a machine which is practically a vertical lathe; the bell is always moulded a little flat to allow of this procedure.

It is only lately that care has been taken to get the partial tones of a bell into something like a harmonic series. The partial tones of a uniform bent plate would be inharmonic, and no doubt were so, in most bells which were cast up to thirty years ago. By having parts thicker than others it is possible to lick the lower partials into shape. All of these partial tones, except one, are sufficiently close to the series 1 : 2 : 3 : 4 : 5 : 6 to be dragged into harmony. The exception is the third partial, which should be 2·5 or 2·4 (the number varies with different makers) times the fundamental. The agreement actually obtained is within 5 per cent. of these values.

Beside these partial tones, which can all be picked up by suitably tuned resonators, there is another note which has baffled scientists for many years. This is the "strike note," which, for the first few seconds after the clapper has struck the bell, predominates in intensity, and in fact determines the nominal pitch of the bell. The curious point about this tone is that it cannot be picked up by a resonator, although the ear judges it to lie in the neighbourhood of the second partial, *i.e.* the octave of the fundamental or hum-note, and it has been stated that it may be excited by a tuning-fork of the requisite pitch having its stem pressed on the rim. As the intensity of the partial tones in the neighbourhood of a recently struck bell is very large, it is, of course, possible that it may be one of those subjective combination tones which are formed in the ear when the ear-drum is acted upon by periodic forces of large intensity. Perhaps tests of the distance at which this "imaginary tone" is audible compared with the partial tones might settle this question; at any rate, the rapid dying out as the partials are damped is in favour of this hypothesis, although Taber Jones thinks the "strike note" is the fifth partial (double octave) misjudged as to location by the ear, and he supports this suggestion by finding initially large intensity and subsequent rapid damping out of the fifth partial.

The above outline of the acoustics of bells is supplementary to, rather than abstracted from, *Bells Thro' the Ages*. One is disappointed to find that Mr. Nichols dismisses this aspect in a page, but the book is a mine of strange lore and interesting photographs. We have, firstly, photographs of the oldest survivors, and a list of the various names under which the instrument was known; we read the relation of "bell" to

"bellow"; and one might suggest that the intimate use of the bells as signals for the performance of public and domestic duties by the dwellers of the campaign accounts for the name "campana." It is then shown how bells increased in size and number as churches spread after the recognition of the Christian religion by the Roman State.

There are chapters on bells, celebrated and obscure, with their weights, inscriptions and founders' cabalistic marks. The inscriptions range from Latin Augustan or decadent, to English inchoate or modern. The spelling of English belongs to the good old go-as-you-like days, of which anyone may imagine specimens. The inscriptions give the idea that the itinerant bell-founders who set up their temporary foundries in the churchyard or neighbourhood left their spelling-books and Latin grammars at home when they set out on their wanderings. Animabus occurs often, along with the more intimate a'i'bus and ai'abs. Indeed, a comma or mordant ~ above the print fulfils all the uses of an abbreviated or short-hand Latin. In d'no co'fido = In domino confido. This mordant stands variously for om, am, an, ann, ro, or, er, etc.

The curious cases mentioned where nothing but letters, alphabetically or indiscriminately arranged, are found on the bells, seem to mean that the founder did not know enough of Latin to misuse the case-endings, or was not sufficiently remunerated to exercise his English inspiration and drop into poetry. He may have wished to point to the sharpness of his letters as a proof of the accuracy of his casting. The word "casting" calls to memory the Bell-yeter of Billiter Street. Is yeter cognate to jeter, to throw, to cast?

One is tempted to quote many of these old inscriptions. Our ancestors had no false modesty, and an inscription gave the donor the opportunity for a little self-advertisement. Thus, at Bath Abbey,

> "All you of Bathe that heare mee sound,
> Thank Lady Hopton's hundred pound."

Nor did the founders neglect the opportunity of advertising their merits. At Bruton a recast bell bears the couplet,

> "Once I'd a note that none could bear,
> But Bilbie made me sweet and clear."

A full description of the founding and hanging of bells shows by diagrams how the swinging bell is arrested in an inverted position while it awaits its turn to sweep round and hit the clapper. A man, even of the present day, might be justly proud of inventing such a simple and efficient means as the slider, with its saving of energy. Yet it was invented in the remote past. Our forebears were wonderful people.

The chapter on change ringing recalls the algebra book of school-days; the permutations of *n* objects are tabulated up to 12, amounting to some hundreds of millions. There should be another column of the necessary hundreds of years for each peal. The diagrams for the use of the ringers look like cross-stitch patterns, or knights' moves in chess.

Although carillons have long been famous in the Low Countries and are now being introduced into America, carillon playing does not find much scope in this country, because most peals are tuned here to the diatonic scale for change ringing. A recent transatlantic book pours scorn on the peculiarly English custom of change ringing, which is perhaps more of a mathematical and physical exercise for the ringers, than a form of music. But if we have not carillons in the true sense of the word, does not every large cinema organ possess its tubular bell stop? In this connection one would like greater mention, among secular uses, of the signal bell. In particular, the submarine bell has made great advances in recent years, and has been given a shape more closely resembling the ancient bell. Presumably Trinity House does not credit the mariner with a sensitive musical ear.

The author, who is an enthusiastic ringer of several guilds, has evidently found occasion to visit and ring in many churches. Everywhere he has interviewed bells and bellringers, and consulted authorities and records. He has copied rules for promoting good, and penalising bad, behaviour in the belfry; the expenses of bells and ringers; contests for money prizes (now discontinued); bequests for ringing; legends and lore of the district. It is all so complete that one feels that even the smallest village may find an account of its beloved bells.

The author makes an eloquent appeal to young men to join a guild and learn the art and mystery of change ringing. He gives a sad account of the manners of the early ringers and contrasts them with the ringers of to-day, who do not ring for money and beer; though the picture of an old belfry with the ringers pulling in a circle round a black jack is more amusing than a photograph of severely correct gentlemen ringing with nothing in the middle of their circle but conscious rectitude.

REVIEWS

MATHEMATICS

Mathematical Tables and Formulas. By PERCEY F. SMITH, Ph.D., and WILLIAM RAYMOND LONGLEY, Ph.D. [Pp. v + 66.] (New York: John Wiley & Sons; London: Chapman & Hall, 1929. Price 8s. net.)

SEVERAL reviews of books of mathematical tables have appeared from time to time in this quarterly. This American publication is probably the best that we have seen, because it deals not only with logarithms and trigonometrical tables, but adds many useful formulæ. In fact, it may almost be called a compendium of mathematics for the use of readers who are not professional mathematicians, but who often require to deal mathematically with various branches of knowledge. It even gives a valuable table of integrals, and another of approximate evaluations, with a third on differential equations—with books on which our libraries sometimes groan. But this little work by Dr. Percey F. Smith and William Raymond Longley is only a small octavo volume of sixty-six pages, with three blank pages for readers' notes and a thumb-index.

R. R.

ASTRONOMY

Life and Work of Sir Norman Lockyer. By T. MARY LOCKYER and WINIFRED L. LOCKYER, with the assistance of Prof. H. DINGLE, and contributions by Dr. CHAS. E. ST. JOHN, Prof. MEGH NAD SAHA, Sir NAPIER SHAW, Prof. H. N. RUSSELL, the Rev. J. GRIFFITH, Sir RICHARD GREGORY and Prof. A. FOWLER. [Pp. xii + 474, with 17 plates.] (London: Macmillan & Co. Ltd. 1928. 18s. net.)

SIR NORMAN LOCKYER apparently inherited a love of experimental science from his father, Joseph Hooley Lockyer, who was interested in various applications of chemistry and electricity in Kensington and later in Rugby, in which town he founded the Rugby Literary and Scientific Institution, and where Joseph Norman Lockyer was born on May 17, 1836. His introduction to astronomy occurred after the family had removed to Leicester, and an amusing instance is given showing his early development of an independent nature. At about the age of thirteen he went to school at Kenilworth, and later at Weston-super-Mare. In 1856, at the age of twenty, a visit to Switzerland enabled him to study French and German, and the next year he attended lectures in Paris at the Sorbonne.

His introduction to official life occurred in May 1857, on his being granted a temporary appointment at the War Office, this being changed to an established clerkship when he passed the necessary competitive examination in February 1858. Considering himself thus established on a satisfactory career, he married in the same year Miss Winifred James, of Leamington. Settling at Wimbledon, he appears to have formed several lasting friendships, perhaps the most important from the resulting influence on his astronomical career being his introduction to Thomas Cooke, the famous optician at York. In 1861 he purchased a 3¾-inch Cooke telescope and started his first obser-

vatory on Wimbledon Hill; later he obtained a larger instrument of 6¼ inches aperture, and this remained in his possession all his life. His early work with this telescope appears to have induced his first literary contribution to the *London Review* in 1862, which led to correspondence with the noted astronomical observer, the Rev. W. R. Dawes. Joining the Royal Astronomical Society in March 1862, he made a valuable series of observations of the planet Mars during the opposition of that year, and these are published in the Memoirs of the Society.

From very early days Lockyer had the ability not only to make useful practical observations and supply adequate accounts of them to learned societies, but the further prescience to provide simpler descriptions adapted to more popular requirements. This ability was of special value to him in his association with the *Reader*, started in January 1863 by Lockyer and several friends, and although the venture only lasted some two and a half years, it convinced him that a journal epitomising the current knowledge and discussions of scientific inquiry was deserving of every effort. This wish he was able to fulfil a few years later, when in 1869 he established the weekly journal *Nature* with the generous co-operation of Messrs. Macmillan. His first visit to the meetings of the British Association appears to have been at Newcastle in 1863, and his account of the proceedings was printed in the *Reader*. Soon after this he had several severe breakdowns in health, but only with difficulty could he be persuaded to take the necessary rest.

In 1864 he made inquiries regarding spectroscopic apparatus, and obtained a modest equipment from the optician John Browning. This was later augmented by the purchase of a more powerful spectroscope with seven prisms. The well-known story of the dual discovery in 1868 by Janssen and Lockyer of the method of observing the solar atmosphere without an eclipse is elaborated, this being followed soon after by Lockyer's discovery of the brilliant yellow spectrum line, slightly more refrangible than the sodium "D" lines, which he persisted in regarding as due to some element not then known on the earth, and therefore he christened it *Helium*. His view was triumphantly vindicated twenty-six years later by Sir William Ramsay's discovery of terrestrial helium in certain rare minerals.

Until 1869 most of his work had been done in a purely private capacity in addition to his clerical duties at the War Office. In that year, however, his pioneer work was recognised by his election into the Royal Society, and this led to his becoming a very popular lecturer in various parts of the country. In 1870 his appointment as Secretary of the Royal Commission on Scientific Inquiry greatly widened his influence, and it was largely due to his efforts that the Scientific Collections at South Kensington were commenced, now so magnificently housed in the new Science Museum.

Soon after this he became associated with Prof. Frankland, who provided him with laboratory facilities in the new college in Exhibition Road, and a few instruments with which regular observations of the sun were organised. From this small beginning developed the Solar Physics Observatory, which, although provided with shelters of dilapidated and temporary appearance, was later equipped with most valuable apparatus for astrophysical investigation, much of it being presented by friends of Lockyer; *e.g.* many valuable glass mirrors, from 6 inches to 36 inches aperture were made and given to the Observatory by Dr. A. A. Common, another enthusiastic amateur astronomer. His appointment as Professor of Astrophysics in the Royal College of Science marked the inclusion of practical and theoretical instruction in astronomy in the curriculum of the institution. After his foundation of the British Science Guild in 1905 he conducted an intensified campaign for the more thorough recognition of scientific training in connection with educational programmes. He also resumed work on the orientation of ancient monuments, with the object of assigning to them an astronomical association of

similar nature to that he had previously described for the Egyptian temples.

From 1907 to 1911 he was troubled by the impending necessity of the Observatory buildings being removed to allow for extensions of the Science Museum. For some time it was hoped that the transference would be to a hill site overlooking Surrey, but when it was decided to move the Observatory to Cambridge he at once organised a new institution, now called the "Norman Lockyer Observatory," on a site adjoining his country residence at Sidmouth, in Devonshire, and in a short time, with the generous aid of several friends, he had the satisfaction of its successful inauguration. There he had the pleasure of continuing his investigations until his death in 1920, in his eighty-fifth year. The Directorate of the Observatory then passed to his son, Dr. W. J. S. Lockyer, who had been working with him as chief assistant since 1898.

The second portion of the book consists of several short articles, each dealing with a special phase of Lockyer's work. Prof. Dingle contributes articles on "Constitution of the Sun," "Dissociation Hypothesis," "Sun and Meteorology," and "Meteoric Hypothesis"; Sir Napier Shaw on "Solar Physics in relation to Meteorology"; Prof. M. Saha on "Dissociation Equilibrium"; Dr. C. E. St. John on "Modern View of the Constitution of the Sun"; Prof. H. N. Russell on "Lockyer's work on Stellar Evolution"; Rev. J. Griffith on "Lockyer's work on Orientation of Egyptian Temples and Ancient British Stone Monuments." Sir Richard Gregory describes the influence of Lockyer's many activities in the furtherance of scientific education and national development, and an account of the development of scientific centres at South Kensington in connection with the Royal Commission of the 1851 Exhibition. Very fitly the volume is concluded with an appreciative retrospect by Prof. A. Fowler, who was so intimately associated with Lockyer's work for many years.

In any subsequent edition it would add greatly to the value of the work if more illustrations known to be available could be introduced, and most readers would doubtless welcome some amplification of the rather meagre index. To the general reader the volume should make a strong appeal as the story of a more or less self-taught observer, succeeding in pioneer work of considerable difficulty; to the more specialist student it is interesting as a record of persistency in the prosecution of original research, often against strong opposition. The admitted fact that several branches of the work led to untenable conclusions was mainly due to the inadequate power of much of the apparatus available at the time, and there is still the positive advance evidenced by the confirmation of important parts of his theory of stellar evolution by other investigators in recent years.

C. P. Butler.

The Sun, the Stars, and the Universe. By W. M. Smart, M.A., D.Sc. [Pp. xii + 291, with 20 plates and 108 diagrams.] (London: Longmans, Green & Co., 1928. Price *12s. 6d.* net.)

From both sides of the Atlantic there have appeared in recent years numerous popular and semi-popular works on astronomy, similar in design but differing widely in treatment. The compression within 300 pages of an account of modern discoveries and their resultant theories is a difficult task requiring a keen sense of comparative values, and the difficulty is increased when it is necessary to include an explanation of principles and methods sufficient to make the whole narrative interesting and intelligible to the general reader.

Dr. Smart is to be congratulated on the success with which he has carried out his task. The book is essentially up to date, the general principles are

described clearly and concisely, and the reader is not wearied with too detailed and technical accounts of methods and instruments, while the style and language are naturally more picturesque and exuberant than would be expected in an ordinary text-book. The author shows a refreshing pride in his profession and his co-workers; to him "astronomers are nothing if not enthusiastic and pioneering," and his admiration for their achievements is contagious.

Four introductory chapters are devoted to a necessary groundwork, explanatory of the principles of celestial measurements on the sphere and the various instruments employed, together with a brief but interesting summary of the history of the science. The special contributions of Copernicus, Kepler, and Newton are described and lead naturally to an account of the discovery of Neptune.

A single chapter is sufficient to bring the reader into touch with the most recent researches on the planets. The modern ideas regarding Mars, its atmosphere, physical characteristics, and surface temperature are well summarised in nine pages, while the mention of Mercury's orbit affords an opportunity for a brief reference to the Einstein Theory and its astronomical implications. Similarly, in the chapters on the sun Dr. Smart finds room for an account of the theory of the atom and Milne's researches regarding the nature of the components of the solar atmosphere.

The latter half of the book is devoted to the stellar universe. The measurements of brightness, of proper motions, of radial velocities, of distances, of the periods of double and variable stars, and the differentiation of spectral types are all described in proper perspective with regard to their contributions to the great task of explaining the evolution of the stars, and elucidating the riddle of the universe. The broad principles of the latest theories of Russell, Eddington, and Jeans are here accessible and intelligible to the general reader.

The book is well printed (except in the few cases where Greek letters are used), and the plates, reproduced from photographs, are well chosen. The numerous diagrams should be very helpful in explaining the text. The errors are few in number and generally of small importance, such as the rotation speed at Jupiter's equator (p. 128), the magnitude of Sirius (p. 187), and the substitution of Jupiter for Saturn (p. 286), while diagrams 83 and 84 are incorrectly oriented. If expansion should be necessary in another edition considerable space might be saved if the author were rather less punctilious towards his authorities. One famous astronomer, mentioned by name nearly 30 times, is invariably prefixed by the title Professor. But these are very minor blemishes on an excellent book, the value of which would be still further enhanced by a rather fuller index.

R. W. W.

I. **Collected Papers on Wave Mechanics.** By Prof. E. Schrödinger. [Pp. xiii + 146.] (Price 25*s*. net.)

II. **Selected Papers on Wave Mechanics.** By L. de Broglie and L. Brillouin. [Pp. 151.] (London and Glasgow: Blackie & Son. Price 15*s*. net.)

These two works on the recent developments in atomic problems are of the nature of companion volumes and are best reviewed together.

In the former we have the sequence of Schrödinger's papers from the first published in 1926 to one of June 1927. In the latter we have important original papers and expositions of de Broglie and Brillouin.

Schrödinger's papers are rather of the nature of a series of dispatches on

the progress of a campaign whose consequences are not foreseen and which now and then appears almost to get out of hand. There is something of the spirit of adventure throughout the book and the story is by no means ended when the last page is read. We need further developments before we can be satisfied that the ending is a happy one.

The individual papers produce varying impressions upon the reader. Here is one whose beauty of form and success in detail give both pleasure and satisfaction, here another indicating a groping in the dark ending in a find of uncertain importance to the main theme.

This makes the book fascinating to those who enjoy the study of the development of scientific theories.

We may describe the first paper as the discovery of the wave equation and of its importance on account of the character of its solutions. It opens up a new field of research and makes possible a classical attack upon atomic problems.

The second paper shows the close relation, which Hamilton had previously noticed, which exists between mechanics and optics. It is pointed out that in atomic problems the dimensions are of the order of the wave-lengths and that we must therefore expect to find in atomic physics a branch of mechanics corresponding to diffraction in optics. Calculations of energy values in the important examples occurring in the theory of spectra are worked out.

The method of treatment of the wave problem in this paper makes it perhaps the most elegant of the series, but, alas, the waves are waves in dynamical space! Our number of dimensions must change with the number of degrees of freedom of the system considered. But is this a disadvantage after all? It is no greater than that associated with the treatment of systems in dynamics.

We may still cling to the classical ideas of three dimensional space even though a system is more conveniently treated by generalised dynamical methods. So we may, if we wish, associate waves and particles in ordinary space.

Another paper shows the relation between matrix mechanics and Schrödinger's theory and is very important because it has strengthened both theories. Other papers deal with non-conservative systems and apply themselves to the perturbation theory.

Finally there is an attempt at a generalisation, somewhat after the method of Gordon and de Donder, in the hope of uniting wave mechanics and electrodynamics. It is concluded that even the rather restricted problem considered, *i.e.* with neglect of electronic rotation, is not satisfactorily described by the theory proposed. We are at the present moment at the stage when these difficulties appear likely to be set aside as a consequence of the work of Dirac and Darwin on quantum equations of the first order.

In the second of these volumes we read again in one of de Broglie's papers of the analogy between mechanics and optics. We have here at first hand some of the ideas which inspired Schrödinger. The account is particularly clear and is associated with definite physical ideas.

There is a very good exposition of the Heisenberg matrix theory by Brillouin, together with Dirac's generalisations.

A very interesting paper is that on the five dimensional universe, in which the theory of Kaluza and Klein is described with some modifications. Remarkable points of the theory are the form of the geodesics of the space which correspond to the tracks of an electron in an electro-magnetic field, and the symmetry of the wave equation in this system of co-ordinates. It is, however, somewhat disappointing that the wave function and its equation do not fit intimately into the geometrical scheme.

The last paper of the volume is on the new statistical methods and gives a comparison of the classical, the Bose-Einstein, and the Fermi-Dirac methods.

It is pointed out that different definitions lead to very similar formulæ and the investigation shows the relation between the three methods.

The appearance of these important works in English is very welcome and the translators are to be congratulated upon their success, especially in the difficult task of interpreting the many technicalities of the first volume.

H. T. FLINT.

Wave Mechanics and the New Quantum Theory. By ARTHUR HAAS, Ph.D. [Pp. xviii + 124.] (London: Constable & Co., 1928. Price 7s. 6d. net.)

THIS book is the English translation of a work in German based on a course of lectures delivered in the University of Vienna about a year ago. The author's object is to deal with the recent advances in atomic physics from a general point of view so as to explain broadly the different theories developed and to show the connection between them. This purpose has been well carried out. The author has avoided the tedious paths and has indicated the high ground from which the view is good.

As the title indicates, the two methods of treatment developed by Schrödinger and by Heisenberg and Dirac are discussed, but the connection between the methods is pointed out and the reader will appreciate that he is being introduced to a unified subject.

In addition an introduction to the new statistical methods is given. The later chapters deal with the causal and statistical views in physics, a result of the recent developments which promises to be perhaps the most interesting.

The question is whether the so-called microscopic phenomena of physics are of a statistical character. Is the passage of an electron from a point A to a point B a subject for the theory of probability and not for dynamics as we have been accustomed to suppose?

We thus come to the question whether our observed laws are after all only average laws analogous to the laws we deduce about an assembly of gas molecules. The analogy here is possibly not very good; for in the dynamical theory of gases we do not deny to each molecule an actual position and velocity. We confess failure to follow the history of each molecule. The question in relation to atomic physics is more profound. It would appear that we must not ascribe position and velocity to our electron in the same way as to the molecules; we must not suppose that there is any causal phenomenon at all underlying our theory.

There is a very short chapter on the relation of the theory to philosophy. The newer views of physics will doubtless influence philosophy; history teaches us that this is likely to be the case. One hears already some expression of philosophic opinion. It seems rather difficult to understand how our difficulties concerning the question of free will can be removed by the assumption that an electron is not sure where it will be in the next minute fraction of time. In any case the freedom is not great for the odds are strongly in favour of a particular result. But doubtless in these days the physicist must study philosophy and the philosopher physics. One cannot help remembering an occasion on which a famous chairman introduced the founder of the theory of relativity with a rather long philosophic address to an audience which was told immediately afterwards by the principal speaker that the principle of relativity was a physical theory.

The reader of Prof. Haas's book will be encouraged by the view spread out before him to stop and examine the details of the scenery, and because there is no royal road across this territory a more vigorous effort will be demanded for the appreciation of those delights for which there is no "guter Aussichtspunkt."

H T. FLINT.

Physics in Industry. By H. E. Wimperis and F. E. Smith, F.R.S. [Pp. 54.] (London : Oxford University Press, 1927. Price 2s. 6d. net.)

The important reactions between physics and most, if not all of our material industries are no longer matters of doubt. Whereas in pre-War days, save in the practice of one or two enlightened firms, the great weight of such applied research as existed fell on the shoulders of a few chemists, much of whose time was consumed in routine analytical work, we now find that physics and mathematics claim with chemistry a due share of recognition, and that physical research laboratories are doing work of first-rate importance in industries of the most varied character—in textiles and in building, in applied electricity and in photography, in the steel and the glass industries, in the manufacture of gas and in the separation of ores—in scores of trades indeed in which but a short generation ago rule of thumb and empiricism reigned supreme.

The Institute of Physics has taken upon itself to give its members and the world at large some concept of the amount of this revolutionary change. We have already been told of the physicist in engineering, in the rubber, the textile, and the ceramic industries, in agriculture and in metallurgy, and now we have before us a neat volume telling the tale of physics in aeronautics and of physics in navigation.

It goes without saying that Mr. Wimperis tells a fascinating story in a fascinating manner. Even though his physics is in the main mathematics the layman will find little difficulty in following the general principles laid down. The introduction of viscosity terms into the ordinary hydrodynamical equations adds considerably to the complexity of problems already far from simple even where an inviscid fluid is concerned ; Mr. Wimperis sketches simply but clearly the manner in which the addition of a circulation to the streaming of the inviscid fluid past a body placed therein will simulate the reactions actually existing when the body is placed in the viscous fluids of our real world.

For the most part Mr. Wimperis is suggestive rather than simply descriptive and in his accounts of the autogiro, of wind channel experiments, of the troubles attendant on small scale experiments, of the relation between detonation and dope, and of the possibilities of super-compression, he leaves us with the feeling that " for the world of physicists in general, aeronautics teems with problems of great complexity and intense interest which await their study."

Which is entirely as it should be.

Dr. F. E. Smith, faced with the problem of an address on Physics and the Navy found that there was no branch of applied physics that had not on occasion been called in to the solution of some vexing naval problem. He has therefore restricted himself to the consideration of Physics and Navigation, and within the limits of his subject discourses with his accustomed lucidity and thoroughness in detail of instruments ancient and modern for the measurement of position at sea, gives a vividly interesting and scholarly account of the development of the compass and of the gyro-compass, and then proceeds to a discussion of sounding methods, old and new, of position-finding during fog by the aid of radio-acoustic methods, of directional wireless, of leader cables, and of directional fog-signals. The address concludes with a description of the attack that has already been made on the important problem of the detection of icefields.

Relatively restricted though his subject may be, Dr. Smith had a wide field to cover and he has accomplished his survey in masterly fashion. The two addresses set a high standard of achievement for succeeding lecturers and may be thoroughly recommended, alike to expert and to layman.

A. Ferguson.

Volumetric Analysis. By Dr. I. M. Kolthoff, with the collaboration of Dr. Ing. H. Menzel. An Authorised Translation based upon the German Edition by N. Howell Furman, Ph.D. Vol. I. Theoretical Principles. [Pp. xvii + 289, with 20 figures in the text and 6 tables in appendix.] (New York: John Wiley & Sons; London: Chapman & Hall. Price 15*s*.)

"Volumetric analysis at present is of slight reputation among physical chemists; they frequently give—in most cases wrongly—the preference to a gravimetric determination. Both methods are subject to the errors that are caused by the scales, the set of weights, and the operation of weighing. In gravimetric analysis there are also the complications of washing (the precipitate goes into solution), and of adsorption (occlusion, etc.); into volumetric analysis the systematic and titration errors enter" (p. 245). Adsorption by a precipitate can be a source of error in a volumetric determination, and a chapter is devoted to a discussion of this question in the light of recent work. In most cases this source of error can be eliminated by appropriate procedure. Systematic and titration errors then remain and these can be estimated in neutralisations and precipitations if the dissociation constant and solubility products of the substances concerned (including the indicator) are known. By making such an estimate and so determining the factors upon which the error depends, the most suitable conditions (*i.e.* of concentration, temperature, solvent, etc.) can be discovered. In the earlier part of the book, the methods, based on the law of mass action, by which such calculations can be made, are developed. Later, in dealing with oxidation-reduction and the titration of organic substances, such a direct attack is not possible owing to irreversibility and slow reaction velocity entering as disturbing factors. Under the first head an account is given of recent work on oxidation and reduction indicators, particular attention being paid to the recent excellent work of W. Mansfield Clark and his co-workers. Organic titrations are discussed in so far as they can be elucidated by theoretical considerations. For instance, the influence of the decomposition constant of the bisulphite compounds of different aldehydes upon the error in volumetric estimation based upon their formation is discussed.

It should not be inferred from his insistence on a theoretical basis that the author is merely dealing out "writing desk procedure." He is very emphatic (p. 115) in urging that "one should never fail to test the theoretical deductions experimentally, and should not be content merely with the results of the computation." The author's general attitude can be judged by a quotation from the introduction: "It must be obvious to every chemist that it is by all means necessary to make a practical test of the theoretical deductions. It is well known that complications often arise in practice, so that one cannot only rely on theory alone. On the other hand, one should never be content with the practical results alone; a knowledge of the theoretical foundations is necessary to a decision as to the general applicability of a method." The book should do much to encourage a more enlightened outlook on volumetric analysis. The author and translator are to be congratulated on the third of their joint productions. An interesting feature of the book is the constant use of Sφrensen's negative exponents in place of concentrations which makes for conciseness in the presentation of formulæ.

R. K. Schofield.

The Fundamentals of Chemical Thermodynamics. By J. A. V. Butler, D.Sc. [Pp. xi + 207, with 51 figures in the text.] (London: Macmillan & Co. Price 6*s*.)

The importance of the thermodynamic method in chemistry is being increasingly recognised by teachers in this country. It is not, however, at all an easy

matter to present this side of chemistry to the student at an early stage in a form which is at once accurate and readily comprehensible. Apart from some half-dozen slips in formulæ which the author should correct without delay, the text is accurate and thoroughly reliable. The brevity of the deduction by which the author arrives at the conception of free energy and demonstrates its usefulness in surveying physico-chemical problems is generally commendable. But there is one stage in the deduction which would be improved by a little expansion. This is the point at which the conception of a free energy change during a chemical action is identified with the isothermal work obtained from a reversible process. Students usually find it difficult at first to see what bearing a deduction from idealised processes such as a Carnot cycle can have upon a spontaneous chemical reaction which cannot in practice be made to perform work. No doubt spontaneous processes will be dealt with more thoroughly in his second volume when the concept of entropy is considered.

The author is to be congratulated on the use he makes of the galvanic cell and electrolytic phenomena generally in illustrating thermodynamic method; a study of this part, which comprises practically half the book, should lead a student to a better understanding of electro-chemistry as well as thermodynamics. The conceptions of activity and activity coefficient are introduced simply and naturally in connection with concentration cells. The illustrations and numerical examples throughout the book keep the practical aspect and physical meaning of the various concepts constantly before the reader.

This little book is thoroughly good and deserves to be widely used.

R. K. SCHOFIELD.

Calculations in Physical Chemistry. By J. R. PARTINGTON, D.Sc. and S. K. TWEEDY, B.Sc. [Pp. viii + 152, with 6 figures.] (London: Blackie & Son. Price 7*s.* 6*d.* net.)

THE benefit derived from working out numerical examples while studying physical chemistry would be difficult to exaggerate. It is so easy to imagine, after reading some pages of theory, that its meaning has been grasped, when in reality many important points have been quite insufficiently understood. An attempt at working out one or two numerical calculations is the surest step towards the detection and remedy of deficiencies. The value of an inexpensive book of exercises is, therefore, obvious.

The authors have confined their attention to the more advanced work. Each set of exercises is preceded by a brief theoretical section in which the working formulæ and units are set out and one or two examples are worked through. Answers to the exercises are given. Especial care has been taken to emphasise the differences between English and American nomenclature where these exist. The book is primarily designed for the use of students, but there are many who have long passed their student days who will find it helpful. To work through a few appropriate exercises is most effective in refreshing the memory on a branch of the subject which has not been touched for many years.

R. K. SCHOFIELD.

The Determination of Hydrogen Ions. By W. MANSFIELD CLARK. Third Edition. [Pp. xiv + 717.] (London: Baillière, Tindall and Cox, 1928. Price 30*s.* net.)

THIS, the third edition of Prof. Clark's well-known volume, contains, as would be anticipated, a very considerable amount of matter additional to that of the former editions. At the present time two very distinct recent theoretical developments, as well as a large addition to the number of reversible electrode

systems, drawn not only from the world of organic chemistry, but also from base metal and metallic oxide systems, are causing serious difficulty to those who, whilst wishing to incorporate in extenso the views and data of the school of Arrhenius, van't Hoff, and Ostwald, yet wish to give a comprehensive treatment on modern lines. This difficulty is apparent in this case and a large volume is the result. It is indeed a debatable point whether it would not be better not only to eliminate entirely the older conception of bases as substances yielding hydroxyl ions in aqueous solution, but also to commence with the theory of ionic dissociation and to apply the concept of ionic *association* to weak electrolytes, *i.e.* to consider the state of a strong acid in solution before extending the ionic hypothesis to the case of a weak one. This volume, like its predecessors, is written in Prof. Clark's characteristic and trenchant style, a welcome contrast to the average modern text-book. It contains not only a detailed account of both the indicator and hydrogen electrode method for the accurate determination of hydrion activities, but also the more recently developed oxidation reduction systems including the quinhydrone and the allied electrodes as well as metal, metal-oxide systems. A very complete bibliography is included, the rate of growth of which appears to be exponential with the advancing years. At no point does Prof. Clark commit himself to any mechanistic interpretation of electrode reactions, as a rigid thermodynamic treatment coupled with precise measurement supply all that is actually necessary for the determination of hydrogen ions, but to those who do possess a keen curiosity as to what is happening in the solution the short paragraph on non-aqueous solutions (ch. xxix), together with Brönsted's investigations, proves singularly interesting and appropriate. The print is clear and pleasant to read, whilst the colour chart does give the observer some indication of the changes actually observed in solutions, a result not frequently obtained.

ERIC K. RIDEAL.

Fixation of Atmospheric Nitrogen. By FRANK A. ERNST. Fixed Nitrogen Research Laboratory, U.S. Department of Agriculture. [Pp. viii + 154.] (London: Chapman & Hall. Price 12*s.* 6*d.* net.)

THIS volume is intended as a semi-popular exposition of what is generally known as the nitrogen problem. The importance of nitrogen fixation looms now so largely in our economic life that volumes of this character are to be welcomed, but it is a little doubtful whether either the chemical or engineering abilities of the general public, the potential readers of this book, are as high as Mr. Ernst believes them to be. Schoolmasters and those entrusted with the general education of youth might, however, read it with profit. The book contains a general survey of the problem of nitrogen fixation and of its importance, followed by descriptions of the three principal industrial solutions by the arc, cyanamide, and ammonia processes respectively. Brief statements are made as to the economic possibilities inherent in these processes, together with a description of the principal products of the ammonia industry, and the volume concludes with a chapter on statistics of production and appendices on the literature. The book is well printed and the illustrations are excellent. Some mention of the electrical synthesis of nitric oxide as contrasted with the purely thermal synthesis in the arc process, pp. 20–24, might have been made and the fixed nitrogen production of the British Isles is considerably underestimated.

ERIC K. RIDEAL.

Principles and Applications of Electrochemistry. Vol. I. **Principles.** By H. JERMAIN CREIGHTON. Second Edition. [Pp. xvi + 488.] (London: Chapman & Hall. Price 20*s.* net.)

THIS, the second edition of a well-known text-book, includes, in addition to numerous points of detail not present in the first edition, new material in the

form of discussions on the polarity of molecules, on the Debye-Hückel theory of strong electrolytes, and on the concept of activity. The difficulty of writing a text-book on the electrochemistry of solutions at the present time is clear to those who are interested in the change that is taking place since the concept of partial ionic dissociation in solution was replaced by that of partial ionic association. We note that since the old assumption of the mobilities of ions being independent of the ionic strength must be modified, it is a little confusing to find no discussion on this point in the chapter on the migration of the ions (Ch. VI). Most of the various dilution law equations, with the exception of that of Ostwald and of Storch, might well be eliminated as well as the speculations of Ghosh, whose mathematical calculations are unsound and suggestions without novelty. Apart from these points, which tend to render the present position of electrochemistry somewhat vague and uncertain to the reader, the volume contains much that is interesting; containing as it does many tables, problems, and examples, it is eminently suitable as a text-book on the subject.

ERIC K. RIDEAL.

GEOLOGY

Theory of Continental Drift. A Symposium on the Origin and Movement of Land Masses both Inter-Continental and Intra-Continental, as Proposed by Alfred Wegener. By W. A. J. M. VAN DER GRACHT *et alia*. [Pp. x + 240, with 29 figures.] (Tulsa, Oklahoma: American Association of Petroleum Geologists. London: T. Murby & Co., 1928. Price 15*s*. net.)

THIS valuable symposium on the theory of continental drift is the outcome of a discussion at the New York meeting of the American Association of Petroleum Geologists in November 1926. One must admire the catholicity of the geological spirit of this Society in sponsoring a book on a subject which, at first sight, has little to do with petroleum. However, E. de Golyer, in a prefatory note, explains the matter by pointing out the very broad geological basis upon which the science of petroleum geology rests, and the value of discussion of the fundamental principles for applied science. With pleasure, too, we note that the publication of this volume has been made possible by the generous financial support of several oil companies having offices in New York.

The book has been edited by Dr. van der Gracht, who introduces the subject with a long and well-written paper in which he discusses the Taylor, Wegener, and Daly ideas of continental drift, and Joly's views concerning the drift of the entire crust at periods of complete liquidity of the subcrustal stratum. He only mentions J. W. Evans's view of drift towards the hollow of the Pacific in the final summing-up which he has also contributed to the volume. Van der Gracht makes out a very plausible case for both inter- and intra-continental drift, and has effected an ingenious combination of the hypotheses of Wegener and Joly.

A large number of eminent Old World and New World geologists have contributed to the discussion. It must be admitted that the great majority of them appear to be hostile to the idea of continental drift; or rather, as van der Gracht points out in his closing review, they express themselves as not fundamentally opposed to continental drift in itself, but to Wegener's hypothesis, and are especially critical of Wegener's partial and unscientific modes of presentation of the evidence. Bailey Willis and R. T. Chamberlin both voice the weighty objection that folding due to continental drift ought to take place in the weaker "sima" rather than in the "strong and rigid" sheets of "sial"; but in the summary that ends the book van der Gracht shows that this objection is based on a very common confusion between rigidity and strength.

The most powerful and effective onslaught on Wegener is made by

C. Schuchert from the stratigraphical and palæontological side. An especially well-written contribution is that by C. R. Longwell, who discusses some physical tests of the Wegener hypothesis. F. B. Taylor expounds his theory of continents sliding Equator-wards, but burdens it with the rather extravagant hypothesis that the drift was due to accelerating tidal forces caused by the capture of the moon by the earth out of space during the Cretaceous period.

"Pentti Eskola" is twice misspelt on p. 10, also "omphacite" on the same page. "H. Holmes" on p. 15 should be "A. Holmes." "Pallasite" is misspelt on the same page, and "troilite" on p. 16. On p. 29 "constituency" should be "constitution." The book is illustrated by a number of clear and informative maps and diagrams. The whole work will prove of the greatest value to geological science. The hypothesis of continental drift has, we think, come to stay, and nothing but good can come of the candid and searching criticism on the one hand, and of the enthusiastic advocacy on the other hand, which the hypothesis receives in the book under review.

G. W. T.

The Geology of Malayan Ore-Deposits. By J. B. Scrivenor, M.A. (Oxon), F.G.S. [Pp. xv + 206, with 4 plates and 47 figures.] (London: Macmillan & Co., 1928. Price 16*s*. net.)

Much controversy has arisen concerning the geology of Malaya and the origin of its tin deposits. The book under review is by the geologist to the Federated Malay States governments, who has been one of the principal controversialists; but while Mr. Scrivenor naturally prefers his own views in his book he is scrupulously fair to his antagonists, and indeed announces the abandonment of a theory of glacial origin of certain deposits in the Kinta Valley which has aroused much opposition. The Malay States now produce approximately 60 per cent. of the world's tin. Hence the compilation of the facts concerning the geological occurrence of tin ores in the peninsula, which is the main object of the work, was an exceedingly useful and worthy task. Few people realise that, as during the early days of Portuguese domination, Malaya is still a gold-producing country. The only other minerals of any real economic importance are tungsten and iron ores.

The first chapter contains a general geological sketch of the country, and of the distribution of its ore deposits. The two succeeding chapters deal respectively with gold and cassiterite; the further chapters describe the western tin belt, the eastern tin belt, tungsten and other ores, the non-detrital deposits, and the detrital deposits, the last two being general discussions of tin and tungsten veins, and of detrital tin and tungsten deposits respectively. A good bibliography is provided in the last chapter.

The three plates comprise two geological maps and a table of non-detrital deposits with examples. Forty-seven excellent illustrations, mostly photographs, adorn the text. Mr. Scrivenor thinks there is no ground for pessimism in the outlook for tin mining, but for continued gold mining the prospects are far from encouraging. Tungsten ore is available in large quantity, but at present there is only a small demand. Iron ore of excellent quality is known, and sooner or later it will prove important for British enterprise.

This book is a remarkably useful production, and we hope Mr. Scrivenor will follow it up with a general Geology of Malaya.

G. W. T.

The Plant-Life of the Balkan Peninsula. A Phytogeographical Study. By W. B. Turrill, M.Sc. (Lond.), F.L.S. [Pp. xxiv + 490, with 10 plates and 11 maps.] (Oxford: Clarendon Press, 1929. Price 30*s*. net.)

This very complete survey of the vegetation of the Balkan area from both the taxonomic and ecological standpoints supplies a connected account of

an area concerning which there is an extensive but very scattered literature. After a preliminary survey of the physical features of the area, its geology, soils, and climate, the author deals with the physiognomy, altitudinal zonation, and character of the plant communities, their successional features, and modification due to the influence of man. The more floristic aspects include consideration of the geographical distribution within the Balkan Peninsula and the relation which the flora of this region bears to that of adjacent areas.

The very large area involved, over 187,000 sq. miles, the diversity of soil and climate are responsible for a flora rich in species and containing many endemic types. The latter constitute one of the most interesting taxonomic and phytogeographical features of the flora. Of the 1754 species which are endemic to the Balkan Peninsula most are perennial herbs and the chief centres of their occurrence are the comparatively isolated regions of Crete and the Grecian Peninsula. Whilst many of these endemics may be of recent origin, an appreciable proportion are probably relics. The affinities of the entire flora are with that of Western Asia rather than with that of Europe, but the species and the communities which they constitute are mostly either Mediterranean or Central European.

Much of the vegetation now existing occupies areas which were formerly forest. In 1096 the country between Belgrade and Nish, a distance of some two hundred kilometres, was covered with dense forest where now there is only deciduous brushwood or open country. The chief causes of forest destruction have been felling for timber, as fuel for domestic use, ore smelting, lime-burning, etc., whilst regeneration has been prevented by the extensive herds of sheep and goats, aggravated by the practice of transhumance. In this way many of the lowland forests of *Pinus halipensis* have become replaced by macchie consisting of leathery-leaved shrubs amongst which *Arbutus unedo, Erica arborea, Spartium junceum, Olea europæa, Quercus ilex, Q. coccifera,* and *Pistacia lentiscus* are conspicuous. Again, in the Central European conditions the lowland forests of deciduous oaks such as *Quercus cerris, Q. conferta, Q. robur,* and *Q. sessiliflora* have been extensively replaced by deciduous scrub (Shiblyak), including such familiar species as *Syringa vulgaris, Berberis vulgaris, Cornus mas, Acer campestre, Viburnum lantana,* and *Corylus avellana.* Still greater biotic pressure has led to sparse vegetation, and a consequence of this destruction of the natural cover, in areas where heavy falls of rain are not infrequent, has been erosion of soil and the establishment of desert conditions. A natural outcome is the high proportion of species characteristic of stony and rocky places which total nearly a third of the entire flora.

The plant successions and consequences of man's direct and indirect influence on the vegetation are well presented and form one of the most interesting features of this book. The consideration of the geographical relations of the flora as a whole, and the significance of the endemic element in particular, are also ably dealt with, whilst the wealth of data and wide outlook adopted will render the work a standard book of reference on the flora and vegetation of this region.

E. J. S.

ZOOLOGY

Destructive and Useful Insects, their Habits and Control. By C. L. Metcalf and W. P. Flint. [Pp. xii + 918, with 561 figures and 14 tables.] (London and New York: McGraw-Hill Publishing Co., 1928. Price 37*s*. 6*d*.)

The authors, one of whom has had twenty-five years of practical work in combating destructive insects and the other fifteen years' experience in University teaching, state in their preface that this book is intended as a text-book for

the beginning student in entomology and also as a guide or reference book for practical farmers, gardeners, fruit growers, farm advisers, physicians, and general readers.

The book starts with two chapters of a general nature dealing with insects as enemies of and friends to man. Both these are excellent essays and can but impress the reader with the great importance of insects in the world.

Next follow chapters on the external morphology, internal anatomy and physiology, mouth parts, development and metamorphosis, the place of insects in the animal kingdom, and the orders of insects. The chapter on mouth parts is characterised by some very good drawings by Antonio M. Paterno.

Having aroused one to the importance of insects, shown one what they are, how they live, their position in relation to other animals, and dealt with the classification of about half the orders, which is quite sufficient, the authors proceed to discuss insect control. This chapter deals first with applied control under five sub-headings: chemical, physical or mechanical, cultural, biological, and legal; and, secondly, with natural control under four sub-headings: climatic factors, topographical features, predators and parasites, and fourthly, insect diseases. Then follows a chapter on apparatus for applying insecticides.

The remaining thirteen chapters are devoted to dealing with pests of corn, small grain and forage crops, cotton, tobacco, citrus, shade trees, greenhouses, domestic animals, man, etc. This part of the book is written in very much the same style as Lochhead's *Economic Entomology*, but is in much more detail. Each chapter starts with a key which should serve as a valuable aid in "spotting" the particular pest with which one may have to deal. We use the word "spotting" to denote that there is an element of risk in using keys that only include the major pests, as frequently in a particular district or season a normally minor pest, hence not included here, may assume the status of a major one. However, all the chief American pests come under notice.

In such a book, and there have been many as this, it is hard to be original, but the authors have adopted a new order in which to cover the subject. We admire the clarity and directness of the way in which each problem is attacked. The practice of putting the scientific names of the pests as footnotes is worthy of plagiarism. The illustrations are good and well chosen. The authors have not claimed too much in their preface and the book is worthy of a place in all reference and entomological libraries. We fear, however, that its cost will prohibit many students from possessing it.

H. F. B.

Typical Flies, a Photographic Atlas of Diptera. Series III. By E. K. Pearce. [Pp. xv + 64, with 162 figures.] (Cambridge: at the University Press, 1928. Price 10s. net.)

As is indicated by the title this book consists mainly of photographs, although there are, in addition, brief notes describing the species illustrated. In some cases there are photographs of the larvæ and pupæ as well as those of the flies themselves. Thirty-nine families are represented in this series. There is no need to say more than that these photographs are quite up to the high standard set by the two preceding series. The book should act as an incentive to students of zoology and collectors to pay more attention to the Diptera. To the Dipterist it is a "luxury" book which one could do without, but only with regret. The bibliography is not up to date and should have been confined to the standard text-books or else included a much more complete list of recent papers.

H. F. B.

Science of the Sea. An Elementary Handbook of Practical Oceanography for Travellers, Sailors, and Yachtsmen. Prepared by the Challenger Society for the Promotion of the Study of Oceanography. Originally edited by G. HERBERT FOWLER, B.A., Ph.D. Second Edition, edited by E. J. ALLEN, D.Sc., F.R.S. Marine Biological Association of the United Kingdom, Plymouth. [Pp. xxiii + 502, with 220 illustrations.] (Oxford: at the Clarendon Press, 1928. Price 15*s*. net.)

IN the few years between the first edition of this work (1912) and the second, which is now before us, so much has been added to our knowledge of oceanography that it was necessary to re-write a large portion of the book and to thoroughly revise the whole. Nevertheless it is wonderfully little changed in plan and appearance. The chapter on the sea floor by the late Sir John Murray, whose portrait forms a frontispiece, has been left as it was originally written, with only an occasional footnote, and nearly all the figures from the first edition are included. The present editor, Dr. E. J. Allen, Director of the Plymouth Marine Biological Laboratory, has himself contributed largely to many of the chapters besides having the heavy task of bringing together those written by other workers. The Challenger Society is to be congratulated on the re-publication of this the standard textbook on the subject which is peculiarly their own; a book beautifully got up and thoroughly up to date.

All sides of oceanography are embraced. Separate chapters, written by experts, deal with The Air, The Water, The Tropical Shores, Floating Plants and Animals, Sea-weeds, The Sea Floor, Animals of the Sea Floor, Equipment, Dredging, Trawling and other Methods of Fishing, Fishes, Preservation of Marine Organisms, Whales, Seals and Sea Serpents, and finally Logs, Notes and Labels, etc. In addition to all this a list of marine stations is included, tables of classification of plants and animals, literature, a classified list of recommended firms for gear, apparatus, chemicals, etc., and a good index.

The object of the book when first published was to help those, particularly yachtsmen or officers in the Royal Navy and in the Merchant Service, not being professional oceanographers, who on their voyages wished to know something about the sea and its inhabitants. At the present time it appeals to a larger and steadily growing public which demands exact and definite information on all kinds of marine research. These demands, whether made by physicist, chemist, or naturalist, are well satisfied here and all these workers will find real help in whichever branch they wish to follow.

The chapter on air has been re-written by Mr. Brunt and Commander Garbett, the superintendents respectively of the Army and Navy Meteorological Services at the Air Ministry. Ch. II, by Mr. D. J. Matthews, of the Hydrographical Department of the Admiralty, gives a new and up-to-date account of the physical and chemical problems of sea-water, with sections on alkalinity and on the minor constituents of the water by Dr. W. R. G. Atkins, of the Plymouth Marine Laboratory. The remainder is equally interesting. We may specially mention the instructions for equipment and for the preservation of marine organisms. Those who go far afield and encounter strange animals and plants will be able to place them approximately in their proper positions. Prof. D'Arcy Thompson makes out a good case for the sea-serpent in a most entertaining chapter. There are two tables of classification, one on animals and one on plants. It is convenient to find that in these certain unicellular organisms (dinoflagellates, coccolithophores, and others) are placed in both: thus the botanist or zoologist may annex whichever he specially desires, or, preferably, regard them as borderland forms.

This book is thoroughly recommended as indispensable to any marine library, public or private.

M. V. LEBOUR.

The Natural History of Canterbury. [Pp. x + 299, with 27 plates.] (Christchurch: Simpson & Williams. Stocked by Weldon & Wesley Ltd. Price 17*s*. 6*d*.)

This volume consists of a series of twenty-one chapters by eighteen authors, covering a wide range of subjects connected with the history and present state of the Canterbury district of New Zealand. This region, renowned for its sheep, has many claims upon our interest, whether it be as the former home of Samuel Butler when he owned a sheep run, as the provenance of numerous remains of the Moa which became extinct some 300–400 years ago, or as the present habitat of many beautiful endemics such as the lovely Celmisias.

Several chapters are allocated to the historical aspects of the area in respect to its survey and exploration, its botanical and zoological investigation, and the introduction of stock and trout. The last named is inaccurately described as acclimatisation, but the difficulties arose in the one case from the distance of transport and in the other from the poisonous character of certain native plants, in neither from climatic conditions.

The account of the Mackenzie Country is full of interest and it is instructive to note the nature of the "historical events" recorded, such as, for example, the first sheep (1857), the first rabbit (1861), the introduction of the sweet-briar, and the first wire fence (1864), or the first appearance of the ox-eye daisy. What a contrast to the events which figure so largely in the annals of more sophisticated regions!

The floristic and ecological aspects of the flora are dealt with by Allen, Cockayne, Laing, and Wall. The chapter on the vegetation, by Cockayne, not only gives a concise summary of the chief communities but includes also a useful table of the constituent species, their life-forms and the communities in which they occur. The number of flowering plants is small compared with European standards. The area of the Canterbury district comprises some 15,000 sq. miles and the total number of vascular plants is only 341 species. Contrast this with an English county such as Hertfordshire, with only 633 sq. miles and 913 species of vascular plants.

Other chapters treat of the geology, the origin and structure of the Canterbury plains, the forestry, and agriculture. The zoological aspects include chapters on the invertebrate zoology, entomological investigation, the native fishes, and the native and introduced birds.

The work is full of points of interest, but from the number of writers it is naturally to be expected that the text lacks continuity and one therefore deplores the more the entire absence of an index.

E. J. S.

(1) **The Evolution of Charles Darwin.** By George A. Dorsey. [Pp. xi + 294.] (London: George Allen & Unwin. Price 7*s*. 6*d*.)

(2) **Darwinism and What it Implies.** By Prof. Sir Arthur Keith, M.D., D.Sc., F.R.S. [Pp. vi + 56.] (London: Watts & Co. 1928. Price 1*s*. net.)

The natural explanation of the numbers of different species found among animals and plants has probably been to all thinking men the idea that one species evolves in some manner from another species; but as very few persons are thinking persons this idea was lost sight of when people began to imagine that some kind of deity took the trouble to make species by a differential creation. As if any deity would take the trouble to create, let us say, some hundreds of different species of grasshoppers. From the deity's point of view, a grasshopper would always be a useless sort of creature; though, from the grasshopper's point of view, grasshoppers might be very important. We can therefore scarcely imagine a special creation of grasshoppers; but it

is easy to imagine their evolution from species to species by some natural method.

The merit of Darwin consisted not in suggesting evolution, but in trying to discover the steps by which evolution takes place. When one notes that certain States in America actually forbid the teaching of Darwinism this only forces one to ask whether the Americans are not tending to sink downwards in the human scale, rather than to rise upwards.

Both the books under review should be read and studied by people who oppose Darwinism. Perhaps this is really asking too much, because such people seldom read any books at all; or at least I should think not. Mr. Dorsey's book is rather a full description of Darwin's life and writings, and is well written and suitable for the most humble intelligence. Even Macaulay's Schoolboy or a politician could grasp it. Sir Arthur Keith's work is a much smaller one, but is very well written and covers similar ground without entering into so many biographical details. It contains only a Preface and three chapters, namely, The Implications of Darwinism, Concerning the Nature of Man's Brain, Modern Critics of Evolution, and a useful Epilogue.

R. R.

Vertebrate Zoology. By G. R. DE BEER, with an Introduction by JULIAN S. HUXLEY. [Pp. xx + 505, with 185 text-figures.] (London: Sidgwick and Jackson, 1928. Price 15*s*. net.)

IN his opening sentence the author points out that the term Chordate would be more correct in the title, and we are in entire agreement, for the book is obviously intended for readers with some knowledge of zoology and the use of the word Vertebrate is unnecessary. Indeed, throughout the book Chordate is almost always used. The subject matter is divided into five parts: descriptions of a number of types, accounts of four types of development, discussions of various topics of comparative morphology, discussions of evolutionary problems, and lastly conclusions and a general classification.

Within the limits of a book of this size it would not be possible to follow in detail all the multiplicity of structures in the Chordata that lend themselves to comparative treatment and can be arranged in what may be termed "evolutionary" series. On the whole the author has selected wisely, but certain topics, *e.g.* the origin of the paired fins, etc., would well bear expansion.

The descriptions of the different types are necessarily brief; indeed, the reader will find better accounts of most of them readily available in elementary texts in any zoological laboratory. This part might have been reduced or even omitted to make more room for the useful parts that follow. A number of points that need restatement or correction could be pointed out. Throughout, the term "auricle" is used instead of atrium. We are told (p. 147) that the mammalian heart has two auricles, which is quite correct, but it also has two atria. The retention of the term "soft commissure" (pp. 153–154) for massa intermedia is a little unexpected. On p. 51 it states that "the sides . . . (of the pharynx) are pierced by the spiracle and gill-slits," and on p. 37, speaking of the first pair of slits, it says "*it* is the spiracle" as if only one spiracle were present instead of one on each side. It is stated that the ductus caroticus disappears (p. 197) and that there is no ductus caroticus in the adult frog (p. 334). If by this is meant that it disappears as a duct it would be much better to say so, but as both statements stand they imply that the ductus entirely disappears and is not represented at all in the adult frog, and this is incorrect for it is present as a ligamentum.

A number of the illustrations are useful, but some are poor and the one on p. 59 of the venous system of *Scyllium*, even as a diagram, is inaccurate, not representing *Scyllium* or indeed any other Selachian.

The larger part of the book dealing with embryological, comparative

morphological, and evolutionary problems is, as suggested above, useful. Here the student will find descriptions written from a modern point of view, discussions of topics that have been elucidated by recent researches, much information, and many ideas that are not available in text-books. Most of this could only be obtained at the price of more time spent in reading and digesting original literature than the average student can afford. Even then we doubt whether he would have his ideas so well sorted and so clearly expressed as in the present volume. It should certainly be in the hands of all students studying chordate morphology beyond an elementary stage.

C. H. O'D.

Biology of the Vertebrates. A Comparative Study of Man and his Animal Allies. By Prof. H. E. Walter. [Pp. xxv + 789, with 687 text-figures.] (London: Macmillan & Co., 1928. Price 21*s*. net.)

The author states that the book is based on a course of lectures to pre-medical students and it is clear that it is a fuller course than is given in most universities. The term Biology is used because, as the writer rightly points out, it is undesirable in such a course to separate function from structure, for the one is intimately bound up with the other. This point of view underlies the three main divisions of the book. The first portion deals with a number of closely related disciplines that have a general bearing upon comparative anatomy, *e.g.* Palæontology, Pathology, Embryology, etc., and in a manner sets the stage for the succeeding parts. "In part two are grouped chapters dealing with the mechanisms of metabolism and reproduction, including the integument, systems of digestion, circulation, respiration, excretion, and reproduction, together with the glands of internal secretion." The third part deals with the mechanisms of motion and sensation.

In each topic taken up the structures concerned are dealt with in a comparative series from the lower vertebrata up to and including man.

A number of minor points call for attention. *Alunatta* on p. 61 should be *Aluatta*. It is rather misleading in dealing with bilateral symmetry to say that an animal exhibiting it may be divided into halves by three planes when the essential point is that only one plane can divide it into approximately equal halves. The law of priority is dealt with on p. 14, but no mention is made of ed. x of *Systema naturæ*. The classification of the fish scales used on p. 214 is old-fashioned and does not indicate the various fundamentally different structural types that have been described. On p. 598 it is stated that *Brontosaurus* had a great lumbar enlargement of the spinal cord whereas the description obviously applies to *Stegosaurus*, etc.

Apart from such slips the book is well written and easy to read, and contains a remarkable amount of information which is on the whole sound and, particularly in the matter of head segmentation and the constitution of the nervous system, quite up to date. The author enlivens his pages here and there with flashes of humour, perhaps nowhere more striking than in the tail piece on the last page which presents a young child photographed in the middle of a great yawn. It will hardly serve as a text for medical students as their curriculum is ordinarily arranged, but certainly the more keen among them may well turn to it with profit and pleasure. The book is well printed, fully illustrated, and the figures well reproduced.

C. H. O'D.

The Indian Zoological Memoirs. I. Pheretima. By K. N. Bahl, D.Sc., D.Phil. [Pp. iv + 72, with 2 plates and 29 text-figures.] (Lucknow: 1926.) II. Scoliodon. By E. M. Thillayampalam, M.A., M.Sc. [Pp. xi + 116, with 10 plates and 32 text-figures.] (Lucknow: 1928.)

The University student of Zoology in India is faced with a serious handicap in his work, for while he utilises British text-books, containing as a rule detailed

descriptions of British forms, his practical work must of necessity be based mainly upon Indian types. This was pointed out in 1924 by Prof. K. N. Bahl, who suggested the publication of a series of volumes on the lines of the Liverpool Marine Biological Committee's Memoirs. A committee was appointed to put this suggestion into effect and the present volumes are the first two in the series, and are published under the editorship of Prof. Bahl. They are of the same size and general format as the L.M.B.C. memoirs but differ in that the illustrations are not mainly reserved for plates at the end but are distributed in their appropriate places in the text in the form of line blocks or plates, sometimes coloured. In works of this description the placing of the illustrations in the text has certain advantages. If any general criticisms are to be made, they are that the paper might be of a somewhat better quality and that the books should be made available through a British publishing house and bear a standard price, say, in Indian and British currency.

I. Perhaps the commonest of Indian genera of earthworms is *Pheretima*, externally distinguishable by the presence of a complete ring of setæ on each typical segment and a clitellum on segments 14–16. The species chosen is *P. posthuma*, because it or its closely similar allies are in general use in the laboratories. The volume presents a readable and well illustrated account of the Bionomics, general external and internal morphology, more detailed anatomy of the systems of organs, an outline of the development, and a set of clear, practical directions. The book indicates considerable care on the part of the author and sets a high standard for the beginning of the series. It is plainly written and singularly free from typographical errors; one such is found on p. 68 where normal saline is stated to be a 75 per cent. solution.

II. *Scoliodon*. This work maintains the standard of its precursor in the series and gives a very useful account of a common Indian Elasmobranch belonging to the family Carcharinidæ. While it agrees in general characters with *Scyllium* it naturally differs in numerous points of detail. Perhaps the most striking difference is that not only is it viviparous but the young are actually attached to the uterine wall by a small "yolk sac placenta" and the long yolk stalk is provided with a coating of close set, short, appendicula. Three species of the genus *Scoliodon* are very common in Indian seas and the one chosen for description is *S. sorrakowah* which is also the genotype. The whole account is well written and quite up to date, but it is rather a pity that the term metanephros is used as it is far better to restrict this name to the type of kidney occurring in the higher animals, *e.g.* mammals.

It must be obvious that both of the above mentioned works contain a great deal of original investigation and they are not in any sense of the word compilations. They will undoubtedly be of considerable use to Indian students, but in addition they will provide, for comparative purposes, standard descriptions of a series of types. The crucial test of books of this nature is that of extensive and critical use in the laboratory, but there is every reason to suppose that these excellent memoirs will pass this ordeal successfully.

C. H. O'D.

The Seas. Our Knowledge of Life in the Sea and How it is Gained. By F. S. Russell and C. M. Yonge. [Pp. xii + 379.] (London: F. Warne & Co. Price 12*s*. 6*d*. net.)

Many books, as the useful bibliography appended to this volume testifies, are to be obtained which deal with specific parts of this subject. The authors, who are marine biologists at present engaged upon the Great Barrier Reef Expedition, have contrived to treat the whole subject of the life within the sea and methods of research in that connection in one volume.

In the small compass of this book the reader will discover much concerning the richness of the sea in forms of life. The descriptions of the different zones of life and the characteristics of their inhabitants are of the kind which will

present no difficulty to the lay reader. Such chapters as Colour and Phosphorescence, Coral Reefs, and Sea Fisheries reveal the ability of the authors to give scientific facts and theories in an attractive fashion. The only misfortune will be found in the shortness of the space into which such a quantity of material had to be arranged. The work of the marine laboratory and the economic products of the whale illustrate the variety of the material.

The preparation of the book has been well executed. The wealth of illustrations—384, of which 167 are in colours, must be specially mentioned. The illustrations are exceedingly good both as regards artistic quality and originality.

The work is an authoritative account written in a way which will appeal to students and lay readers. There are few who will not appreciate more fully " the sea and all that therein is " as a result of reading this volume.

J. Eling Colleclough.

How Animals find their way about: A Study of Distant Orientation and Places-recognition. By Étienne Rabaud, Professor of Experimental Biology in the University of Paris. Translated by J. H. Meyers, M.A. [Pp. 142.] (London: Kegan Paul, Trench, Trübner & Co.; New York: Harcourt, Brace & Co., 1928. Price 7*s*. 6*d*.)

To state the problem which the author of this volume sets out to elucidate in his own words: " A bee comes out of the hive; it flies away, and then at some distance, often at a great distance, alights on a cluster of flowers and forages. It goes from one flower to another and works about in divers directions, describing many turns and meanderings, without any fixed order; at the end of a certain time, it flies off, and almost immediately takes the direction of the hive, which it regains by the shortest route. An ant proceeds in an analogous way. . . . The birds also do the same. . . . Numerous animals would appear to perform similar feats. How do they discover or rediscover their way? "

The main part of the volume is devoted to the description of experiments devised to throw light on the problem for the case of flying insects, of which bees and wasps are taken as examples, and for walking insects, as exemplified by ants; this section of the work, containing descriptions of experiments devised and observations made by the author himself, is full of interest and would appear to definitely point the way to the solution of the problem for the particular cases considered. Remaining sections of the work dealing with termites, molluscs, and the carrier pigeon are slight; the author briefly comments upon observations made by other investigators; but these additions are by no means without interest, and indeed greatly add to the value of the volume by the glimpses they give into the scope and magnitude of the problems.

Prof. Rabaud concludes that for all creatures memory plays a probably preponderant rôle, cues being registered by means of the sense organs and not by any special sense unfamiliar to man, the predominantly guiding sense, whether it be vision, scent, or is tactile, or muscular, varying to some extent for different classes of animals and with the conditions of the experiment.

We should be able to praise the volume perhaps more wholeheartedly if its title were less general or if its scope were less restricted: for the consideration of the problem for all creatures except bees and ants is so scanty and the investigation, even for bees and ants, concerns itself almost entirely with the means by which the return to its nest is achieved after a journey from it. We should have welcomed from Prof. Rabaud some consideration of the many closely allied problems, such as how a bee is guided to a honey-laden flower, to give one example only; nevertheless, we agree heartily with the notice on the cover that " the book as a whole forms a valuable introduction to a fascinating branch of natural history or animal psychology."

B.

BACTERIOLOGY

Physiology and Biochemistry of Bacteria. By R. E. Buchanan and E. I. Fulmer. [Pp. xi + 516, with 78 illustrations.] (London: Baillière, Tindall, & Cox, 1928. Price 34s. net.)

This work, which is to be completed in two volumes, appears at an opportune moment in the research activities of all micro-biologists. Dr. Buchanan is Professor of Bacteriology, and Dr. Fulmer Professor of Biophysical Chemistry, at the Iowa State College, and both are to be congratulated upon their successful co-operation in producing this work.

Volume I deals with growth phases; composition and biophysical chemistry of bacteria and their environment; and energetics. The 460 pages of text are unequally divided into five chapters. A short introduction of two pages, in reality a summary of the succeeding chapters, is followed by two chapters of about sixty and seventy-five pages, treating respectively the growth phenomena of micro-organisms and the chemical composition of their cells. Two hundred and thirty pages are taken up, as Chapter IV, by a study of the physico-chemical and physical characteristics of micro-organisms and their environment. Chapter V, of seventy-three pages, considers energetics, especially in relation to the utilisation of energy by micro-organisms. An appendix of the literature cited runs to about 670 titles, and there are three indexes, an index of authors, a subject index, and an index of the names of micro-organisms mentioned in the text.

This first volume may be considered as the essential background for Volume II, of the contents of which, through the kindness of Prof. Buchanan, the reviewer has received an advance indication. An aspect of this work which is noticeable all through this volume is the careful subdivision into the various subsections, with the result that a large amount of information is well organised with little or no over-lapping. It would have been of assistance to readers to have had the chapter, section, and subsection indicated by number at the top of each page. A further point of value lies in the authors' care to indicate at what stage in their analysis of phenomena the information given is or may be of use in bacteriological work.

Micro-organisms have rarely been studied except in bulk and in close contact with an artificial environment. Most of the information that we have on the growth rates of bacteria arose from cultural studies on artificial, generally liquid, media. A reaction is initiated by living cells, but the reaction is continued even when the death-rate exceeds the reproduction rate; indeed, various phases of a culture's growth may exercise a variable effect upon the rate of chemical change; these various phases of a bacterial culture are treated in detail in Chapter II. Studies of bacteria in their natural environment are chiefly those of water and sewage bacteria, though, for reasons connected with public health, these are almost confined to tests for *Bact. coli*. No figures are available for growth rates in soil, the chief limitation here being that on no one synthetic media will the whole viable soil population develop. Information not referred to in this volume has of recent years become available which suggests that non-proliferating bacteria are potent activators of chemical change. This is a point of extreme importance in studies of the soil population, where the total number of bacterial cells appear normally to be about 100 times the "plate population." These facts suggest that the authors' phrase on p. 56, "accumulation of products of metabolism is frequently more important in inhibiting growth than in exhaustion of food," might have been sponsored with some references to the literature.

In discussing the chemical composition of cells in Chapter III, the authors quote several figures for water content of bacteria; but the table given is qualified by the statement (p. 69) to the effect that the difficulty of estimating true water content (water content less water of contact and surface films) has

probably pushed these figures too high. This has always been a drawback to accepting figures given as analyses of the dry matter of micro-organisms. After discussing the constituents of the ash, the carbon and nitrogen content of the cells, and their fatty bodies, the authors give 20 pages to bacterial pigments. The following sentence is, however, significant in this section: "in not a single case do we have a satisfactory knowledge of a pigment produced by an organism." A reference on p. 118 to Beijerinck (1890) is probably intended for his paper of 1891 on *Bacillus cyaneo-fuscus*, omitted from consideration on pp. 134, 135. The reviewer suggests that this is probably one case in which the nature of the (blue) pigment was ascertained.

Chapter IV is almost wholly a discussion of the physical and physico-chemical characteristics of systems of various degrees of dispersion: true solutions, 153 pages; colloidal solutions and the colloidal state, 74 pages; a few pages are taken up with specific gravity and conductivity, etc., of cells. The authors do us a service by defining the various designations of the concentrations of solutes; designations apparently of elementary nature yet easily misleading unless clearly defined. After discussing surface tension, adsorption phenomena (reference to the source of the statement on p. 172, about immunisation by bacterial extract has been omitted), and osmotic pressure, the Donnan equilibrium is considered in fifteen pages in a series of schematic equations; but the statement on p. 201 to the effect that such simple and restricted conditions under which equilibrium, as studied, can be established do not occur between the bacterial cell and its usual environment, should have been more emphasised. The last seventy-one pages of this first section are taken up with a consideration of ionic concentration. The adjustment of the reaction of cultural media by alteration of the pH : pOH ratio has by now become universal in bacteriological technique; improvements such as Biilmann's quinhydrone electrode for soil solutions should have received notice. The second section of this chapter deals with phenomena which are of importance to students of bacteria in mass-culture; *e.g.* agglutination, precipitation, behaviour in electric fields, and concentrations of electrolytes. A reference is required to the formol and quinol media noted on p. 301, and also to the heat of turgescence on p. 305. We are still unable to distinguish viable from dead bacteria with any degree of accuracy; on media such as soil, where bacteria have multiplied and died *in situ*, tests involving "net conductance" (p. 371) are not applicable. A reference to Brooks (1922) is omitted from p. 371.

The portion of the volume so far dealt with may be considered as background to the remainder of the whole work, and Chapter V as the first chapter of the study of the biochemistry and physiology of bacteria. Forty pages are taken up with definitions (energy is not defined) and equations for calculating free-energy values; the sources of energy for micro-organisms through oxidation of chemical compounds, and the utilisation and transformation of the energy so obtained complete the chapter.

A few obvious errors have been noted, but do not detract from the value of the volume. If the second volume be built on the same lines as this the authors will be thanked for having so thoroughly sifted our present knowledge of the fundamental facts concerning bacteria. While serving as a source of reference this volume will also remain as a thoroughly readable textbook.

P. H. H. Gray.

Practical Bacteriology, an Introduction to Bacteriological Technic. By F. W. Tanner, Ph.D. (Pp. xiv + 235, with 67 figures in text.] (New York: John Wiley & Sons; London: Chapman & Hall, 1928. Price 12*s*. 6*d*. net.)

Prof. Tanner's previous work "The Bacteriology and Mycology of Foods" has no doubt proved its value in many laboratories and lecture-rooms.

The author's aim has evidently been to amplify this work at the same time separating the practical part from the general. Much of this volume is therefore reminiscent of the former work, but the improvement due to separation and expansion is evident. The general part is due to appear in a separate volume. Students are assumed to be at the beginning of their studies in bacteriology, and the arrangement is adapted to their needs. We are glad to note that Prof. Tanner recognises the futility of students being presented for study with an organism of pathogenic habits. The plan of training will give a grounding in methods for the study of several branches of bacteriology, such as canned food, sanitation, milk, etc. The "exercise" form of study allows for the scheme of study to be introduced into curricula, but the author states that "no printed manual exactly fits the requirements of all laboratories."

After apparatus, the microscope, and culture media have been described in the first three chapters (74 pages), the microscopic examination of bacteria is dealt with in Ch. IV. We would hesitate to instruct students to mix the liquids for the cleaning solution in the order described on p. 3. In the section on staining the author has devoted seven page-faces to the Gram method of staining. This appears unnecessary for students in respect of a method upon which a considerable amount of argument and research may yet be expended. The author draws attention to probable causes for failure to stain flagella. Ch. V (of 17 pages) treats of the isolation of bacteria. Breed's microscopic counting method and the hæmocytometer method are not discussed, though their addition would have increased the teaching value of the book. No reference is made to Mackintosh and Fildes' method for the growth of anærobes, the methods described are certainly cheaper in apparatus. Procedures for studying bacteria in pure culture (Ch. VI, pp. 121 to 151) introduces the class-exercise method of instruction. Much of this falls in with the Manual of Methods, prepared by the Society of American Bacteriologists, the value of which probably lies mostly in providing mental discipline; certainly few forms of bacteria have yet been classified by the "Index number" method. This chapter is followed by two short chapters on the study of yeasts and molds, while the remainder of the book (pp. 162 to 210) is given up to a plan of work and exercises.

Useful information on staining solutions, indicators, etc., is detailed in an appendix. There is a list of books for reference, and an index.

P. H. H. GRAY.

Laboratory Manual of General Microbiology. By FRED. E. B. and S. A. WAKSMAN. [Pp. viii + 145, with 19 figures in text.] (London: McGraw-Hill Publishing Co., 1928. Price 10s. net.)

THIS laboratory manual is devised, the authors state in their preface, for students who have had a previous training in general botany, zoology, bacteriology, and chemistry. On p. 87 is a suggested arrangement for classes in soil-microbiology, extending over 18 weeks; judging from this, the previous training referred to would have been in milk, water, or pathogenic bacteriology. Yet this manual contains a large volume of matter which from the above consideration should have been assimilated in the previous training.

The manual is broken up into four parts. Part I, pp. 1 to 43, contains a list of about 100 culture media; part II, of five pages, details the preparation of staining solutions, and directions for making stained films; part III, pp. 52 to 82, contains details for the preparation of reagents, and methods for determining nitrogen, carbohydrates, evolution of CO_2, and total carbon. Part IV is headed "The Study of Micro-organisms in the Soil," and consists of fifty-eight "exercises." A list of books for reference and an index are appended.

Logically the manual should begin with part IV; the details for media (pp. 8 to 46) should be relegated to the particular "exercise" with which each is concerned. Reagents are best left to an appendix. Authorship of recipes for media and staining methods is generally stated; the erythrosin method, on pp. 49 and 108, is that of Gangulee (*Ann. Appl. Biol.* 1926); the "congo-red for negative mounts," pp. 51 and 96, is that of Henrici; the erythrosin method for soil is that of Conn. Methods for staining flagella are omitted, as also those for staining fungi and protozoa. The preparation of Normal HCl is omitted, while Normal NaOH is included. On p. 114 it is stated that direct examination of stained films of soil to which mannitol has been added will reveal cells of *Azotobacter*; this is quite likely, but is making the wish the father to the thought; there is no proof that the cells seen are those of this organism. In exercise 14 on p. 104 the word "sugar" is an evident error for "agar."

The Manual will probably be a useful reference book for methods used in analysis, as detailed in part III.

P. H. H. GRAY.

MEDICINE

Handbook of Physiology. By W. D. HALLIBURTON, M.D., F.R.S. and R. J. S. McDOWALL, M.B., D.Sc., F.R.C.P. (Edin.). Eighteenth revised edition. [Pp. xxiv + 902, with over 500 illustrations.] (London: John Murray, 1928. Price 18*s*. net.)

THE fact that this well-known textbook in thirty-two years has passed through eighteen editions is a convincing proof of its value. For generations of medical students "Halliburton" has been the accepted textbook of physiology. The reasons for this well-deserved popularity are many and various, but perhaps the outstanding features which characterise this particular textbook over and above its clear and complete exposition are balance and impartiality in the discussion of the various explanations of physiological activities. These valuable features of the book are due no doubt to Prof. Halliburton's exceptional knowledge of physiological literature and his philosophic outlook, which have enabled him many times to form extremely accurate judgments of the value of new methods and of fresh views. In this latest edition, in which Prof. Halliburton's successor at King's College has joined him as part author, the volume has been slightly shortened, though by rearrangement a considerable amount of new matter has been added. In its new form it certainly lives up to its reputation as a pre-eminently reliable and satisfactory textbook, and can be heartily recommended to all students of physiology, medical and non-medical alike. An interesting publisher's note gives an account of the history of this classic textbook. It appeared in 1848 as *Kirkes' Physiology*, under various editors ran through 13 editions, and then in 1896 was given into the able hands of Prof. Halliburton. During the 29 years of his editorship 17 editions, totalling 116,000 copies, were published, the book having a great sale in the United States as well as in this country. We wish to the book under its new joint editorship a continuation of the prosperity and popularity which has been won for it under the guidance of Prof. Halliburton.

W. C. CULLIS.

Blood. A Study in General Physiology. By LAWRENCE J. HENDERSON, Prof. of Biological Chemistry in Harvard University. [Pp. xix + 397, with more than 200 illustrations.] (New Haven: Yale University Press; London: Oxford University Press, 1928. Price 23*s*.)

"THE subject of this book is the red blood of vertebrates. We shall study this substance as a physico-chemical system and as a tissue, seeking in its proper-

ties the exemplification of some of the general properties of protoplasm. In its physiological function and interrelation with other parts of the body we shall look for an illustration of organic integration and adaptation. We shall also study it comparatively from species to species, in rest and activity and in health and disease. So far as possible these studies will be quantitative and mathematical." This first paragraph of the opening chapter gives in brief the problem attacked in this expanded form of the Silliman Lectures delivered by Prof. Henderson at Yale University in 1928. The essential features of its treatment are indicated in the words "quantitative and mathematical." As the author points out, the few valid and useful abstractions found in biology, such as species, evolution, organism, heredity, protoplasm, and metabolism, fall short in definition of successful physical abstractions like velocity, inertia, temperature, component, phase, where the abstractions have taken the form of precise terms. He is convinced that the time has come for the application to the problems of physiology and of pathology of the methods which have made clear and abstract the conceptions as to mechanical and chemical phenomena. "General physiology" affords almost the only opportunity for their application to biological studies. This term, as introduced by Claude Bernard and as used in modern times, covers the study of the phenomena of life in both the animal and vegetable world, and since these are in the main physical and chemical phenomena they should be investigated by physical and chemical methods.

Such a study might have begun with "protoplasm," the stuff of living phenomena, but the word as it is used connotes a system or class of systems rather than a substance, and though in that sense it may be fair to assume certain types of physico-chemical phenomena as the "elementary condition of the phenomena of life," the substance itself has too little uniformity of composition to make it a suitable starting point for such an investigation. A certain stability of this system is necessary for the adequate carrying on of its functions and for the manifestations of life, and in man and in many other animals protection against excessive change is provided by the constancy in composition of the two fluids, blood and lymph, which bathe the cells and consequently the protoplasm. The blood, complex though it may be, is, relatively to the other parts of the body, a simple system and was the one chosen by Henderson and his co-workers for their investigation along these new and mathematical lines. But the component parts of even this relatively simple fluid have interactions upon one another, so that its fundamental property, its capacity for carrying oxygen, is not a question of hæmoglobin and oxygen only but of many other factors such as the amount of carbon dioxide, of other acids, of bases, and of other proteins. To study a system in which there are a number of variables in a state of mutual dependence is possible only with the aid of mathematics. Blood for the purpose of the investigations here recorded is regarded as a system of eight variables of which the values of two of them, oxygen and carbon dioxide, can be accurately determined, and it is from this angle that its properties have been worked out. Nomograms are given which show the results of a combined experimental and mathematical attack on the problems of its composition and give its composition in terms of the eight variables. Then come discussions, in the light of these findings, on the nature of the respiratory cycle, on the relations between the properties of the blood and circulation and metabolism, on variations in blood in work and in disease and in the blood of various animals. There is a discussion of these properties in their bearing on circulatory adaptations in general and finally a summary of methods and results.

The work that has gone to the achievement of the results given in this book is enormous and could only have been accomplished by a team of workers whose "interactions" must almost have needed regulation on a mathematical

basis themselves. But the work has been worth while, for though it has not given a final answer to the question which the investigators asked themselves at the beginning of their study, it has given *an* answer and in so doing has shown the value of this method of approach to physiological problems. An important and interesting study of blood and of the study of blood.

W. C. CULLIS.

The Journal of Nutrition. Vol. I, No. 1. Edited by Dr. EUGENE DUBOIS and others. (Published bi-monthly by the American Institute of Nutrition. European Agents: Baillière, Tindall & Cox. Price 21*s*. annually.)

YET another scientific journal and yet another specialisation within the wide bounds of physiology! Gone are the peaceful days when in a country a single journal could contain the publications coming from its physiological laboratories. Increase in the number of workers and in the range of problems attacked must inevitably produce a demand for further facilities for publication. The practical and scientific importance of the problems of nutrition have already attracted and will no doubt attract many more workers to that field, so that the numerous papers that will emerge will not easily find space in the physiological and biochemical journals already in existence. But though this process of growth is inevitable it has its unsatisfactory side. The number of journals that can be assimilated, or indeed subscribed to, by any one worker has its limits, and the segregation of subjects, though useful for the specialist is bad for the general worker, who tends to lose touch with the wider developments of his subject. At present, too, the scientific world is suffering from a plethora of papers with, often, a plethora of words, and seems to be in need of a curbing and critical attitude towards publication, so that a new journal cannot appear without rousing a fear that it may open up another field for papers which might be shortened or deferred. Having made this protest it is only fair to say that the members of the editorial board of the new journal are all workers of world-wide reputation in this particular field and that the first number contains papers of interest on vitamins, on general metabolism, and on the economics of food production, and that the journal will no doubt be welcomed by the workers specialising in this field.

W. C. CULLIS.

Food and Health. By A. BARBARA CALLOW. [Pp. 96, with illustrations.] (London: Oxford University Press, 1928. Price 2*s*. 6*d*. net.)

THIS is a simple little book and a good one. It gives in very compact and readable form and in simple language many of the fundamental points to be considered in the selection of a suitable dietary, together with a slight but useful account of the more important points as to the nature of foods and the physiology of digestion. This section includes an interesting table showing the relative number of calories of different foodstuffs that can be obtained for a shilling, expressed in terms of the prices of a 1927 catalogue of a London store, the food being reckoned as it is bought, including waste.

There are sections on vitamins, rickets, the choice of foods, how to arrange a diet, diets for mothers and children, and food as a cure for disease. In conclusion there are some tables giving much useful information as to the distribution of vitamins in natural foodstuffs, analyses of the staple foods, and their energy values. Altogether an admirable little book giving the information it sets out to give, correctly, simply, clearly, and concisely.

W. C. CULLIS.

Muscular Movement in Man. By A. V. Hill, F.R.S. [Pp. 104.] (London: McGraw-Hill Publishing Co., 1927. Price 12s. 6d. net.)

It has been said that the time is long past when one man could, like Humboldt, take all knowledge for his province. That may be; but it is equally true that the worker in almost any domain of natural science outside the realm of pure classification must have an intimate knowledge of chemistry and physics and more than a nodding acquaintance with mathematics. Prof. Hill's delightful volume on muscular movement, originally delivered as a series of lectures at Cornell University, amply illustrates this thesis. The story is a fascinating one and is told in that clear and vigorous style of which Prof. Hill is so completely a master.

An introductory chapter on methods emphasises the machine-like structure of man, and describes in detail how the energy expenditure of the machine may be measured. Oxygen and carbon dioxide relations under different conditions are next considered, and the meaning of work and efficiency, physical and physiological, is discussed in sufficient detail. "Recovery"; the viscosity of muscle, its mechanical efficiency, and the theoretical maximum work obtainable therefrom; the dynamics of sprint-running, and the delightfully simple yet exact experimental arrangements by means of which the space-time curve (we use the term in its Newtonian, or common, sense) of a short-distance sprinter may be easily and accurately obtained; the remarkable mechanism of adjustment by which the respiratory centre of the brain controls the rate and depth of breathing, so that the hydrogen-ion concentration of the blood, disturbed by the lactic acid liberated in the muscles during exercise, is again brought to its normal condition by the lowering of the carbon dioxide pressure in the lungs produced by deep breathing; these form but a selection of the topics treated in this admirable series of lectures.

There are here and there statements at which a captious physicist may cavil. Thus if a man "may employ his effort in producing kinetic energy . . . in . . . a flywheel," the kinetic energy produced is certainly *not* $\frac{1}{2}Mv^2g$ foot pounds "where M is the mass of the object moved . . . and v its velocity."

Indeed, all through there is a neglect of the difference between the formulæ for rotational and translational kinetic energy which affects the quantitative value of the conclusions deduced. The dimensional argument (p. 41) is so weakly put that the conclusion deduced therefrom would be far more convincing if stated as a fact of experience, and the dynamical argument of pp. 51 and 52, leading to an exponential expression for the relation between velocity and time for a runner making his maximum effort is, to say the least, obscurely stated.

But these are minor points, and all laymen in physiology should be deeply grateful to Prof. Hill for the vivid and delightful manner in which he has treated a subject of fundamental importance to each and every one.

A. Ferguson.

MISCELLANEOUS

Autolycus; or, The Future for Miscreant Youth. By R. G. Gordon, M.D., D.Sc., F.R.C.P.Ed. [Pp. 94] (London: Kegan Paul, Trench, Trübner & Co., Ltd., 1928. Price 2s. 6d. net.)

This book belongs to the To-day and To-morrow series of books, and in it the author compresses a considerable amount of knowledge. He considers the nature of Crime and Punishment, the Delinquent Personality, the Social, Educational, Psychological, and Medical factors involved and the Aims of the Future. It is clearly and interestingly written, and will be useful to those not already conversant with the subject and the work being done, as well as to those who wish to have the results of much scattered information in handy

form. The author has avoided the common weakness of writers on the subject of delinquency, of seeking and finding some one cause. A bibliography added to each chapter would have added to its value.

MAY SMITH.

Emotion and Delinquency: A Clinical Study of Five Hundred Criminals in the Making. By L. GRIMBERG, M.D. [Pp. ix + 147.] (London: Kegan Paul, Trench, Trübner & Co., Ltd., 1928. Price 7*s.* 6*d.* net.)

THIS book aims at discussing the delinquent from the standpoint of the medical man. The author has obviously been able to investigate many cases and has had access to much material, but he has little power of presenting his evidence in a convincing manner, nor does he clearly distinguish between evidence, hypothesis, and opinion. He states in the Preface that the nature of the work itself " prohibits the display of an array of statistics " and that he has been interested in the delinquent as a human being. Surely there is no inherent antagonism between treating the patient as a human being during the interview and then expressing the general trends shown in a number of cases in such a way as to enable others to verify or use the results. Actually he gives thirteen tables, presumably therefore using statistical methods; but they are inadequate. Nor does he give any control groups. From the point of view of method the author might gain much from Dr. Cyril Burt's careful survey of a similar field. It would be very interesting scientifically if we could have a study of one hundred families of well-established piety, as a control to these delinquent families.

The author has followed the lure of organ inferiority as a causative factor. Now it may be true that the Great War taught us to value the importance of the endocrine system, but the evidence so far available does not permit of the interpretation frequently put upon it. Does the emotion of fear cause the thyroid disturbance and lead to the ultimate breakdown, or an initial thyroid inferiority? Much yet remains to be done with regard to the endocrine system before far-reaching psychological generalisations can be based upon its functions. Assertions of the existence of endocrine defects should be put in such a form that a clinician can judge their value just as a writer on cardiac or visual defect would present his evidence. It is helpful neither to the doctor nor any other reader to be told that in a large number of cases the investigator had discovered endocrine defects.

Is it evidence suitable for insertion in a chapter on Heredity to assert that children taken away from their immoral parents at an early age and brought up in an institution in " an atmosphere of religious reverence, discipline, and altruism " should at puberty become delinquents?

The author is clearly interested in his subject, and doubtless can deal with individuals, but his presentation of material lacks method and clearness of thought.

MAY SMITH.

Geography of North America. By GEORGE J. MILLER and ALMON E. PARKINS. [Pp. xv + 605.] (London: Chapman & Hall, 1928. Price 22*s.* 6*d.* net.)

THIS is another competent textbook of the type we have become accustomed to expect from geographers in the United States. Copiously illustrated, both with photographs and with diagrams, the latter mainly of Government origin, it fulfils exactly the claims which are made for it by the authors. It is intended as a textbook for American students in training colleges or in their first year of university reading, and it could be equally well used by English colleges for students at the same stage.

The attitude of approach is admirable, as might be expected from the

wide experience of the authors in methods of presentation. The great emphasis placed on the "problem method" cannot fail to be both useful and stimulating to young students. Characteristic of the problems propounded at the end of the chapters are the following: "Can the United States maintain its position as the leading agricultural and manufacturing nation of the world?" (p. 78), "What States may equal or excel Wisconsin in dairying?" (p. 202).

The authors are also to be congratulated on the bold manner in which they face the difficulty that there is not always a discernible geographic reason to explain a geographic fact. It cannot be said, however, that the chapters on manufacturing provide an entirely satisfactory solution of this difficulty, in spite of such statements as "There seems to be no outstanding geographic reason for the prominence of small metal goods in the industries of this region" [i.e. Connecticut] (p. 144). The development of the New England textile industry is fairly well handled, but the treatment of most of the other industries fails, mainly owing to three reasons: (*a*) to over-compression, (*b*) to the difficulty of obtaining authentic material regarding the history of their growth, and (*c*) to the absence of indication that there is a field for research in this direction. The chapters on agriculture and natural resources, on climate and physiography, do not suffer in the same way, for obvious reasons. There is not only a good selection of facts, but an abundance of illuminating comment, e.g. the explanation given for the extension of winter wheat (p. 186), and there is special reference throughout to "human" or "social" geography.

Although this is the third substantial textbook on the subject which has been published within the last four years, the work under review should meet a real need, as it is designed for a somewhat younger audience than the other two.

M. R. Shackleton.

Astrologie, Alchemie, Mystik. Ein Beitrag zur Geschichte der Naturwissenschaften. By Franz Strunz. [8vo, pp. 351.] (Munich, 1927, Otto Wilhelm Barth-Verlag G.m.b.H. Price, paper covers, RM. 6; cloth, RM. 7.50.)

That alchemy has always been intimately associated with astrology and mysticism is a matter of common knowledge, though at times the extent of this association has been over-estimated. It is by no means unusual to find that, while an alchemist was also an astrologer, he was more or less successful in keeping the two subjects separate from one another. One may instance the author of the two eleventh-century works known as *The Sage's Step* and *The Sage's Goal*. His name is unknown, but he was a Spanish Moor. *The Sage's Goal* is purely magical, and achieved some circulation in mediæval Europe in the form of a Latin translation under the name of *Picatrix*. *The Sage's Step*, on the other hand, deals entirely with alchemy, and much of it gives the clearest possible instructions for cupellation and other chemical operations. Yet from the earliest days astrological theory has profoundly influenced the theory of alchemy, while the effective birth of alchemy in the mystical clouds of Alexandria imbued it with a character which remained with it through the centuries. Prof. Strunz treats his subject not from the point of view of the pure historian of science, but as one whose main interest lies in the development, growth, and inter-relationship of ideas. He shows how alchemy, which started as a practical art of metal-working and alloy-making, gradually became a doctrine of the Elements, and finally developed into a philosophy of the Cosmos. His book forms a useful complement to Mr. A. E. Waite's *Secret Tradition in Alchemy*.

E. J. H.

Coloured Thinking. By D. F. Fraser-Harris, M.D., D.Sc., etc. [Pp. vii + 269. (London: George Routledge & Sons, Ltd., 1928. Price 5s. net.)

Coloured Thinking is the title of the first of twelve essays on various topics in praise of science and literature. Those which deal with scientific subjects are of an elementary character intended for popular reading. Dr. Fraser-Harris appears to have an unlimited enthusiasm for science and poetry. His enthusiasm for science rises in places to such a pitch that some readers may find it hard to refrain from blushing. In the fifth essay, for example, entitled "Science and Character-building," science is defined as "that training of the mind imparted by a rigorous, unbiased, and sympathetic study of nature" which is "second to no other intellectual exercise." The author then proceeds to explain how science teaches us (1) care, (2) courage, (3) dignity, (4) that nothing is trivial, (5) heroism, (6) humility, (7) respect for nature, (8) reverence for life, (9) sympathy, (10) respect for other people's opinions and prejudices, (11) that a woman's behaviour is due to neurasthenia, (12) a new criminology, (13) restraint, and (14) reveals to us beauty. People who follow other pursuits (except certain poets) may be lamentably lacking in these virtues. "Your painter or musician may be a perfect barbarian, ignorant, superstitious, self-satisfied, and intolerant." Nevertheless something more, says Dr. Fraser-Harris, is needed, namely morality. He condemns the "science without morality of the Germans," and says: "Not science, but a hideous, preposterous, soul-destroying ethic it was that made possible the barbarities of the Great War."

The author's own "respect for other people's opinions, rights, beliefs, doubts, prejudices, tastes, and foibles" appears to be severely strained by the people condemned in the second essay on "Childishness in Adult Life." These include people who believe "that a small figure—usually ugly—stuck on the bonnet of their car as a 'mascot' can do anything to ward off bad luck," those given to excessive novel-reading, those addicted to the repetition of slogans, anti-vaccinators, people who sign petitions to reprieve murderers in spite of the fact that "legal experts have considered all the aspects of the case," those who admire, or have their attention attracted by, electric signs in Piccadilly, and finally those who indulged in the "ebullition of emotional infantilism" which characterised Armistice night, when (according to Dr. Fraser-Harris) "Grave and reverend seniors joined in the commotion" and even "respectable people danced on restaurant tables and deliberately threw the crockery about the room." If only such people could be given a proper scientific education such blots on our civilisation would disappear, although Dr. Fraser-Harris believes that "education in itself does not eradicate hereditary tendencies to superstition," nor abolish "the influence of racial functional momentum." Thus until eugenics has completed its beneficent work the "lady-like tea-table elysium" predicted by Herbert Spencer as the final goal of man will be beyond our reach. Let us hope the Home Secretary has received a copy of this book.

J. H. W.

Birds and Beasts of the Greek Anthology. By Norman Douglas. [Pp. vii + 215.] (London: Chapman & Hall, 1928. Price 7s. 6d. net.)

This book makes no profession to be scientific. It is an attractively written commentary on references to mammals, birds, reptiles, and amphibians, marine creatures, and "creeping things" in the anthology. The author calls it "An undertaking, for the rest, of the gentlemanly kind; quite useless." Nevertheless, and apart from its literary merits, the author has attempted to identify the species mentioned, and the systematic zoologist may well find something of interest in these pages. The reader will also find sundry

culinary details, including a "recipe for boar's flesh," of which, writes the author, "if anybody will take the trouble to dress a saddle of mutton in the same manner, he will be pleasantly surprised at the result." Mr. Douglas finds "fifty or more" references to the bee and its products. He does not share the veneration in which men have held "this cantankerous and fussy insect which, by dint of specialising in communistic habits, has lost every shred of individuality." The bee's honey he calls "an overrated article of diet which ruins with its perfumery flavour whatever it is used for sweetening, be it liquid or solid." A bibliography is given and the book has an index.

J. H. W.

Studies of Quality in Cotton. By W. Lawrence Balls, Sc.D., F.R.S. [Pp. xxvii + 376, with frontispieces and 130 figures.] (London: Macmillan, 1928. Price, 20s. net.)

It is inevitable that sooner or later the older industries must pass under the rigid control of scientific method and scientific reasoning, but as the transfer is yet by no means complete, there are passages in this book at which the scientist will chuckle and the industrialist gnash his teeth. On the other hand, there are confessions at which the industrialist might well grin and the scientist scratch a puzzled head.

Dr. W. L. Balls has devoted twenty years of his life to the study of cotton in all its stages from the raw material to the finished yarn. The results of his earlier researches he has described in "The Development and Properties of Raw Cotton." *Studies of Quality in Cotton* is an account of the last ten years of his stewardship, held under the auspices of the Fine Cotton Spinners' and Doublers' Association, Ltd. It is a most inspiring record, and one which no student of textile science should fail to read—and read thoroughly from end to end. The researches described were carried out on cotton, but the methods and arguments used are applicable to textiles in general.

Studies of Quality in Cotton is "an outline of the relationships which connect the properties of cotton yarn with those of the raw material from which the yarn is made." It is above all the work of a scientific mind of high order brought to bear on the problems of an ancient but still largely empirical industry; and for this reason is written in a style which, though familiar enough to "pure" science, must, in parts, undoubtedly prove irritating to the almost childlike scientific innocence to be met with so frequently in the textile trades. For instance: "The mule seems to stand in the same relation to us of to-day as does a Dinosaur—except that we happen to be used to mules. If regarded simply as a piece of physical apparatus for producing yarn under controlled conditions of tension and twist, so that we may hunt down the relationship between cotton and yarn, it is seen to be nearly as defective as any apparatus could be." And again: "This was an indication that the wisdom of experience was not yet quite complete." Such outspoken statements are without doubt distressingly true; yet the *rapprochement* of Science and Industry is a delicate task, and it would be a thousand pities if such an outstanding book as this should receive the cold shoulder in industrial quarters through excess of frankness.—But perhaps that is, after all, the best way of dealing with the issue; it is a difficult point to decide. At any rate, Dr. Balls' book should be eagerly welcomed in scientific quarters. As already stated, it is a truly inspiring work, and the publishers have produced it admirably. There appear to be one or two small misprints in the section on "The Strength Gradient."

W. T. Astbury.

An Introduction to the Theory and Use of the Microscope. By H. D. Griffith, B.A., and G. B. Marshall, M.A., M.D. [Pp. 90, with 29 figures and 3 plates.] (London: Geo. Routledge & Sons. Price 3*s*. 6*d*. net.)

The purpose of this little book is so well indicated in a foreword that it may fittingly be repeated here.

"The inspiration of this brochure was the institution of lectures and practical work on Microscopy as part of the class of Medical Physics in the University of Aberdeen. It was felt that a small textbook covering the work of the systematic lectures would be helpful to the student and might aid him to realise the capabilities, limitations, and proper method of use of the instrument.

"A chapter on the elementary mathematical treatment of certain problems discussed in the text has been added.

"It is hoped that the work will prove of value to all students who require a microscope in their studies, as well as to those amateur microscopists who wish to understand the fundamental principles on which Microscopy is based."

There are far more pretentious books on microscopy that do not give such useful information as that contained in this one. It has the merit of being concise and the directions given are just those that a student requires. The elements of the theory of microscopic optics are set out as an appendix, forming a valuable guide for the worker who wishes to understand the principles on which the work he is doing are founded. The book should be cordially welcomed as filling a much-felt want and may be commended to all microscopists as a most useful guide.

J. E. B.

Textile Microscopy. By L. G. Lawrie. [Pp. x + 144, with 84 figures and 3 plates.] (London: Ernest Benn, 1928. Price 25*s*. net.)

This book provides in one volume a concise account of the microscope and its accessories, together with a description of the various operations involved in the preparation of textile fibres for microscopic examination. The earlier portion describes the usual appliances and accessories that are used in conjunction with the microscope, and some accompanying instruction is given on their manipulation and adjustment. It is probably hardly intended to be exhaustive, and it may be sufficient for those who use a microscope rather as a works tool than as an instrument of research. The advanced worker will no doubt expect to refer to some standard work on microscopy for further information and instruction. It would not be difficult to mention examples of misleading instructional directions, but two must suffice. On page 17 it is stated that, instead of using a high-power eye-piece to obtain an enlarged image, one can lengthen the draw-tube. It would be difficult to suggest a more dangerous line of procedure; in fact, the very foundation of image formation in the microscope would then be undermined. Even with medium power objectives a change of a few millimetres in tube length has an appreciable effect on the image, and the adjustment of tube-length should be understood by all microscopists before they can claim any order of efficiency. In the portion devoted to illumination there are references to the use of ultra-violet light, from which it might be assumed that light of short wavelength can be satisfactorily used with ordinary optical equipment. Such an assumption would only lead to disappointment; such work requires special apparatus which is available in but few laboratories. Suitable types of microscope are described, and micrometry, which is of considerable importance in all work with fibres, is adequately dealt with. The more important part of the book is that dealing with methods of preparing and staining fibres for examination. This section is of greater interest, and the writer apparently handles the subject with confidence. The microscopy of textile

fibres is not easy; difficulties that are peculiar to the subject arise in many directions. The book is well produced, and will no doubt be a useful introduction to those interested in research work on textiles.

J. E. B.

The Rôle of Scientific Societies in the Seventeenth Century. By MARTHA ORNSTEIN. [Pp. xiv + 308.] (Chicago, Illinois: The University of Chicago Press. Price 15*s*. net.)

THIS book is a dissertation by M. Ornstein which was accepted by the authorities of Barnard College for the degree of Ph.D. in 1913.

It was published in May of this year in memory of the author—she was killed in a motor accident in 1915—by grateful and devoted friends.

The book is divided into three parts.

The first part gives a general survey of the advance made in science during the seventeenth century, and also a brief account of the individual work done by a few of the great men of the century, *viz.*: Gilbert, Galilei, Torricelli, etc.

The influence of Bacon and Descartes on the mind of the seventeenth century is also briefly dealt with, and apt quotations are given.

The scientific information which a man familiar with the whole range of science possessed in 1600 and 1700 respectively, is given, and it provides an interesting and startling contrast, showing the stupendous advance made during the century.

The second part of the book deals with the various scientific societies which sprang up in Europe at this time and their significance to the science of the day.

The chief societies dealt with are the Accademia del Cimento in Italy, the Royal Society in Britain, the Académie des Sciences in France, and the Berlin Academy; many other societies are briefly mentioned. The societies are compared and contrasted both as to their origins and their methods of procedure, and the interrelation between them is also discussed. The aim of the various societies from the start was the perfecting of instruments and the cultivation of experimentation, and many of them tackled the same problems; thus we find the Royal Society and the French Academy investigating problems first suggested by the Accademia del Cimento, yet their interests were not so entirely scientific as that of the Cimento, for, side by side with purely scientific questions, they considered problems of trade, commerce, and manufacture. Also the individuals were not submerged into a corporate effort of the society in the English and French societies as they were in the Italian, but individuals played their own part, and an effort was made to give each individual experimenter his due proportion of fame for his discoveries.

A fairly long and accurate account is given of the foundation and early history of the Royal Society, *i.e.*, from its informal beginning to the publication of the *Principia*, and it is shown to have played a leading part in the fostering and furthering of the scientific spirit in Europe during the seventeenth century.

"A body of experimenters and science-loving amateurs, not supported by or affiliated with any learned body—indeed, hardly helped by the King—created a center where the new science could be fostered; . . . experimenting along most varied lines of research; constantly communicating with foreign workers, and establishing the first organ of international scientific communication in the *Philosophical Transactions*."

The author regards the Royal Society as first among the pioneer reforming bodies of the century.

The part played by the scientific journals of the time is shown to be no mean one.

The third section of the book deals with Science and the Universities, and it is shown that they were the last citadels to fall under the spell of the new experimental method. In fact, the Universities of Europe cut but a sorry figure and were dead to the influence of early science, "except that they felt occasionally called upon to rise in defence against the invaders." The scientific societies, however, ultimately triumphed over the Universities, and rendered them friends and promoters of the experimental science, instead of the stubborn foes they had so long been.

This book gives an exceedingly interesting and thorough account of the spread of experimental science at a time when certain men had as much enthusiasm for it as the advocates of a new religion have for their cause. It is an excellent account, and should adorn the shelves of all lovers of science and truth.

J. R. Morgan.

The Corridors of Time v. The Steppe and the Sown. By Harold Peake and Herbert John Fleure. [Pp. vi + 160, with 84 illustrations.] (Oxford: at the Clarendon Press, 1928. Price 5*s.* net.)

This is the fifth volume of the series entitled *The Corridors of Time*—written by Messrs. Peake and Fleure. *The Steppe and the Sown*, which will be as acceptable to students of ancient humanity as have been the earlier parts of this series, deals with the nomadic people dwelling in the great steppe regions of the earth, and their dispersal, through various causes, to other areas. The authors deal very fully with this dispersal, which they regard as marking one of the crises of the world's history, and as representing a period of widespread activity, with increased mobility (due to the use of the horse) and communication, with discipline. The effect of the penetration of old centres of civilisation by the people of the steppes is clearly indicated, and in the Danube basin the invaders made themselves masters over the peasant settlements on the Hungarian Plain. This overlordship, in its turn, resulted in the dispersal of some part of the peasant population in this region, and the course of their migrations can be traced by discoveries of the well-marked types of pottery vessels which they made. The cultural remains of the Swiss Lake Dwellings are possibly to be traced to the migrating peasantry, and to the epipalæolithic descendants of the men of Ofnet. The movements of peoples then in progress affected other and more distant areas, and these repercussions upon the existing cultures are dealt with in an attractive and convincing manner in the volume under review. The book is well and profusely illustrated, and contains a series of valuable maps showing the distribution of the Danubian civilisation from 2500 B.C. to 2200 B.C. At the end of each chapter there is an adequate bibliography.

J. Reid Moir.

The Great Engineers. By Ivor B. Hart. [Pp. viii + 136.] (London: Methuen & Co. Price 3*s.* 6*d.* net.)

This book is another volume of the Great Scientists Series, published by Methuen, and is written by the author of the *Great Physicists* and carries out a parallel function for Engineering as that did for Physics.

It contains thirty-three diagrams of the important instruments employed at the various periods covered by the book, and it is quite easy to follow the working of these from the descriptions given.

It is neatly bound, but the print is somewhat trying to the eyes.

The book covers the development of engineering science from classical antiquity to modern times; it contains a mass of curious and interesting information, much of which must be new even to a man thoroughly familiar with the ordinary history of science.

Some early instruments, such as the Groma, the Dioptra, the Levelling Staff, and the Chorobates, are briefly but clearly described, and some mechanical devices of antiquity are given. A concise account is also given of mediæval technology, which consisted of the production of iron, lead, silver, tin, and coal.

It is shown that the mechanical sciences take up a modern aspect under the able guidance of the great Italian engineer and painter, Leonardo da Vinci (1452–1519); it was largely due to his influence that machinery was applied to do work previously done by hand, or not done at all. Many of da Vinci's mechanical devices are described in some detail, and are rendered quite clear by diagrams.

During the fifteenth century an advance, corresponding to that which began to modernise machinery, took place in metallurgy, thus providing the great engineers with the materials for their work.

Georgius Agricola (1494–1555) is rightly given the place of honour in this work, and a short summary (very brief) of his great book, *De Re Metallica,* is given.

The story of the development of the steam engine from the days of Heron to the days of Watt makes very interesting reading, and should be extremely useful to those students of the history of Science, or any other for that matter, who want a concise account of the development of the use of steam as a motive power. The evolution of mechanical transport which gave the world the locomotive and the steamship is not gone into at all, owing, as the author points out, to the limitation of space. This is a great pity, as even a few pages, treating this development in a very general way (as of course it would have to be treated in a small volume such as this), would give the story an air of completeness.

A summary is also given of the evolution of the gas and oil engine from the proposal by Huygens, in 1680, to use gunpowder in the cylinder chamber to the advent of the Otto engine which formed the basis of all subsequent development.

Two chapters are devoted to the production of iron and steel—the Bessemer Process, the Siemens Process, and the Basic Process are given.

In this little volume a great deal of ground is covered in a very short space, consequently, as the author himself points out in his conclusion, he has been able to do little more than skim the surface of his vast subject.

The story is told, however, brief though it is, in a simple, clear style that even a man of no scientific knowledge can readily follow, and it is worthy of the perusal of all who have any interest in the gradual development of the control of nature exercised by man.

J. R. MORGAN.

The Ordinall of Alchimy. By THOMAS NORTON. Being a facsimile reproduction from *Theatrum Chemicum, Britannicum*, with annotations by Elias Ashmole. [Pp. viii + 125.] (London: Edward Arnold & Co., 1928. Price 10s. 6d. net.)

The Ordinall of Alchimy was an anonymous manuscript written in the fifteenth century. It was begun in 1477, and so is one of the earliest English alchemical writings. It was first printed in 1652 by Elias Ashmole in his *Theatrum Chemicum Britannicum*, of which it occupies the first 106 pages. To these Ashmole appended twenty pages of annotations and explanations, and now Dr. Holmyard has prefaced a facsimile reproduction from a copy of Ashmole's *Theatrum Chemicum Britannicum.*

The authorship is readily traced, as Ashmole says, by *Acromonosyllabiques* and *Syllabiques Acrostiques*, to Thomas Norton. The author is discovered by combining the initial syllables of the "Proheme" and the first six chapters,

which, together with the first line of the seventh (final) chapter, combine to read:

Tomais Norton of Briseto,
A parfet Master ye maie him call trowe.

Thomas Norton was probably born about 1400 at Bristol, and eventually attained to a position of some importance and was a privy councillor in Edward IV's reign. From Chapter II of the *Ordinall* we learn that he had acquired a knowledge of Alchemy at the early age of twenty-eight years, having been instructed in it by a master (most probably Sir George Ripley), to whom he refers in terms of great admiration. By the end of forty days he had learned the secret of preparing the "red stone," or Elixir of Gold. He relates the difficulties and troubles that beset the adept and mentions that his preparations were stolen from him on two occasions, once by servants and once by a merchant's wife.

The Ordinall of Alchimy proclaims Norton's *Crede Mihi*, and although it contains relatively little of chemical value and was eclipsed not long after its publication in 1652 by the more exact scientific achievements of the Fellows of the newly founded Royal Society, it is still full of interest. It is prefaced by a bilingual (left hand Latin, right hand English) poem, and is illustrated with several very fine woodcuts. The one on page 102 represents two men engaged in alchemical processes, and may illustrate the furnace described on page 97 with which Norton says he attained "threescore degrees" and which enabled many operations to be conducted at once.

In some respects there has not been great progress since Norton wrote. Thus little is still known about the senses of Taste and Smell, although a deeper knowledge concerning Colour has been attained.

From page 69 we learn:

Odor is a smokish vapour resolved with heate,
Out of substance, by an invisible sweate;
Which in the Aier hath free entringe,
And chaungeth the Aier and your Smellinge;
As Sapor of Meates chaungeth your Tastinge,
And as Sounds chaungeth your Hearinge,
And as Colour chaungeth your Sight,
So Odor chaungeth Smelling by might.

The facsimile reproduction of the old letters and archaic spelling add an attractive charm to the book and will scarcely present any difficulty to the reader. A curious printer's error appears on page 95 where "Chap. 9" should read "Chap. 6."

Chemists, and men of science generally, will feel grateful to the publishers for reproducing this quaint work and to Dr. Holmyard for his interesting and explanatory Introduction.

J. G. F. DRUCE.

The Notation of Movement. By MARGARET MORRIS. [Pp. 103.] (London: Kegan Paul, Trench, Trübner & Co., 1928. Price 2*s*. 6*d*. net.)

SYMBOLISM has more to do with our daily life than we imagine, and a suggestive symbolism, in art equally as in science, may play an important part in cultural development. Without the ordered symbolism of mathematics our knowledge of the laws of the cosmos would be sketchy and superficial, and our outlook on our perceptual world poor and unimaginative at best.

The efficiency of the symbolic method adopted may have reactions of incalculable importance—within the science of mathematics, for example, it is no small matter whether we choose Cartesian or vector symbolism as the

vehicle of our work. And in another direction, as Prof. Levy hints in his suggestive introduction, though the sol-fa notation may be very convenient for the expression of a simple melody, we shall not go far in the matter of complex orchestration while we are tied to such a relatively crude symbolism.

Speech, song and movement are three great outlets for the artistic expression of the emotions—all such expressions are evanescent, and symbolism of some kind is necessary for their permanent record.

Speech and song have their well-developed symbolisms and now, when the dance has long passed its primitive stage and is to be considered, with poetry and music, to be equally an expression of creative energy and one of the fine arts, the necessity for an adequate symbolism descriptive of movement is apparent.

In the symbolic expression of such an art the grave danger to avoid is that of becoming pictorial, and Miss Morris herself tells us that her own method only became practical in so far as it ceased to be ideographic. Whether her method is the simplest that can be devised we cannot say; but her system of a special stave on which is placed a number of conventional signs for different positions of the body, head, and limbs is certainly adequate to represent very complex movements and poses.

Miss Morris's work in this direction, whatever may be its outcome, deserves the earnest attention of all serious students of the dance.

A. Ferguson.

Culture. By G. Elliot Smith and others. [Pp. 89.] (London: Kegan Paul, Trench, Trübner & Co., 1928. Price 2*s*. 6*d*. net.)

The quarrels of two opposing schools of anthropology are well set out in this interesting symposium. Prof. Elliot Smith pours forth a wealth of evidence to show that culture is diffused from one source, that Egypt is the home of our civilisation, that the theory of the independent invention of its main cultural elements is a delusion and a snare, and that even the beliefs and customs of Lo the poor Indian show indelible signs of a contact with the banks of the Nile.

Dr. Malinowski, with equal enthusiasm and, to the mind of the reviewer, considerably more logical rigour, asserts that " culture is something which is constantly at work, which is there for the satisfaction of elementary human needs, which in turn creates new wants and provides means for their fulfilment," and that " the nature of the cultural process is mixed borrowing and invention."

Dr. Spinden opens his thesis with the pertinent query, " Does man think or merely remember ? " and proceeds with infinite tact to drive a few more nails into the coffin of the diffusionists. Dr. Goldenweiser sums up the controversy with eminent courtesy and fairness, not hesitating, however, to show that his sympathies are not with the upholders of the diffusion theory.

Quot homines, tot sententiæ is an ancient and well-worn tag, but it is thoroughly well exemplified in the contents of this little book, which can be recommended as an absorbing and lucid representation of the views of two schools of thought which differ *toto cælo*.

A. Ferguson.

Science for You. By J. G. Crowther. [Pp. x + 241.] (London: George Routledge & Sons, 1928. Price 5*s*. net.)

Mr. Crowther ranges from China to Peru, discoursing engagingly the while. All is fish that comes to his net, and if you desire to know the latest about cosmic rays, new problems in astrophysics, ultra-sonics, the health of miners, electric lamps and their diseases, why bread goes stale, why flat-irons iron, and why jelly swells you will find in his pages a treatment of these and of a score of kindred problems that is certainly popular and interesting and is on the whole adequate.

Mr. Crowther is to be congratulated on the skill with which he has mingled old and new in his *olla podrida*. The writer who can give a clear account of Sir Isaac Newton's life and work, or of the principles which govern the heating and ventilation of houses is not necessarily at his best when discussing atmospherics or Kapitza's remarkable engineering feat in the design of an electro-magnet and dynamo for the momentary production of intense magnetic fields; Mr. Crowther, however, moves easily and happily amid these very varied topics.

But while he hits off neatly one aspect of the late Lord Rayleigh's work in the statement that he "had a remarkable power of getting interesting results out of unpromising researches," the author goes very much astray when, after giving an account of Rayleigh's discovery of the discrepancy between the density of nitrogen obtained from chemical and from atmospheric sources, he describes Lord Rayleigh as asking Sir William Ramsay to examine nitrogen obtained from these different sources, and ascribes the discovery of the reason for these differences to Ramsay.

Without in any way depreciating Ramsay's genius and remarkable energy, it is morally certain that argon would have been isolated when it was isolated, though possibly by slower and less effective methods had Ramsay never collaborated with Rayleigh.

Mr. Crowther's book can be strongly recommended for popular perusal.

A. Ferguson.

India's Hope. By Francis Henry Skrine, F.R.Hist.S., Indian Civil Service (Retired). [Pp. xiii + 61.] (London: W. Thacker & Co.; Calcutta and Simla: Thacker Spink & Co.)

A scientific Quarterly is scarcely the place for an exhaustive review of a work on Indian recollections and politics, but this little book deals very ably with the sociological problems provided by India since 1870. Mr. Skrine joined the Indian Civil Service and arrived in India in that year, and was afterwards on duty up country in Bengal and on famine duty in the Madras Presidency in 1877, remaining there until the end of 1881. Mr. Skrine is an able champion of the Bengali intelligentsia (called the Bengalee Babu). His writing is brief but clear, and the book contains much good matter.

For myself it is very interesting because I was born in India 13 years before Mr. Skrine arrived there and well remember Calcutta in 1865 and can endorse his descriptions of that great city. As I remember it the maidan was a wide green plain between the rows of houses on one side and the Hugli on the other side—though the river was scarcely visible from the plain owing to the forest of masts of ships at anchor near the city. These have now been replaced chiefly by funnels. Though I was only a boy at the time I was struck by the number of carriages which frequented the maidan in the evening. When I was in Calcutta again in 1927, the numerous changes which had been made struck me very forcibly. The Victoria Memorial is a beautiful building on the maidan opposite to the General Hospital and my old laboratory of 1898, where there are now flattering inscriptions to my work, on the gate and on the wall of the laboratory.

I returned to India from England eleven years after Mr. Skrine had arrived there and was appointed to the Madras Presidency; but I had an opportunity for seeing Calcutta again about 1885. The town has now developed greatly since both of these old dates, and is a very fine city, but of course possesses the dreadful Indian slums which are such a common feature of all Indian towns and even of many villages.

As Mr. Skrine says, the Indian intelligentsia were often despised or looked down upon by the British, but are now fast rising into esteem. Some of the best scientific work is being done by them, as for instance shown in the pages

of that fine medical publication *The Indian Medical Gazette*. Indian medical men often contribute valuable papers and are rapidly rising in ability. We associate the word Babu with a former type, namely the fat and somewhat lazy and effeminate person of old days, who is being replaced by men who are often just as capable and energetic as the British doctors. Not only this, but the general standard of Indian achievement is progressing very fast. When I was last in Calcutta at the end of 1926 and the beginning of 1927 I went to see a Bengali play by Tagore, even then a Nobel Laureate. It was admirably performed in his own theatre, chiefly by a party of Indian ladies whose acting was very fine. The play was a tragedy and the actresses showed a serious interest in it—instead of looking about the theatre and nodding to their friends in the stalls. Here, too, we have to note the emancipation of Hindu femininity. The actresses did not wear veils, as they would have been obliged to do by custom a few years previously.

On the other hand I have not been so much pleased with the progress in India as regards the malaria question, which almost dominates that country as much as the Malthusian problem. In fact friends of India have actually argued that malaria control should not be used because it would increase over-population. That evil is scarcely a sufficient excuse at present. Let us do good where we can even if it may lead to evil in the future. The enormous malaria-rates in India should be combated as strongly as possible, and the Malthusian difficulty may be subdued by future generations. The reader of the book will find many acute and many just observations by the author of it, which I need not mention in detail.

R. R.

Tracking Down the Enemies of Man. By ARTHUR TORRANCE, M.D., Ph.D. [Pp. xiii + 300.] (New York: J. H. Sears & Co. Price $3.50.)

A PHYSICIAN who provides an introduction to this work says rightly that "Romance pours from the tropics as from an overflowing cauldron of some magical brew." Possibly this gentleman has never lived there; but many falsities certainly do exude from the tropics, like the old miasm which people used to think occupied the air of such localities. These falsities bubble up from the marshes of ignorance, rise in the air, and burst into various unpleasant gases—just as they would from the most magical brew—not to mention numerous books we have seen. Fairly interesting as a narrative of life in parts of Africa and Malaya, etc., to those who are not familiar, or too familiar, with those parts, this work adds little to the knowledge or the romance of tropical disease. The author tells us on his title page that he is a fellow of a certain society of tropical medicine—almost every duly qualified medical man can hold this privilege so long as he pays his subscription.

Roger Bacon. By A. G. LITTLE, D.Litt. Annual Lecture on a Master Mind. From the Proceedings of the British Academy. Volume XIV. [pp. 34.] (London: Humphrey Milford. Price 2*s*. net.)

LIKE many men of science Roger Bacon (1214–1294) seems to have been more interested in ideas than in persons, and like many scientific men again he probably suffered for his predilection. There is one kind of man whom their fellows tend to dislike, those precisely who prefer ideas to individuals. The courtier is inborn in most men, and it is a quality which brings prosperity. Ideas are far-off divinities, which do little or nothing for their devotees. This little book gives a short but well-written account of a man who might reasonably be considered as the father of British science, and it should be read by other devotees of the same far-off gods.

R. R.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Mathematical and Physical Papers. By Sir Joseph Larmor, Sc.D., F.R.S., Cambridge: at the University Press, 1929. (Pp., Vol. I, xii + 679, Vol. II, xiv + 831.) Price £6 6*s*. net.

Mathematical Tables and Formulas. By Percey F. Smith, Ph.D. and William Raymond Longley, Ph.D., Professors of Mathematics in Yale University. New York: John Wiley & Sons; London: Chapman & Hall, 1929. (Pp. v + 66.) Price 8*s*. net.

Publications of the Yerkes Observatory. Volume IV, Part VII. Astrometric and Photometric Statistics of Certain of Hagen's Fields Photographed with the 24-inch Reflector. By Harriet McWilliams Parsons. Chicago, Illinois: at the University Press. (Pp. 32, with 2 plates.) Price 7*s*. 6*d*. net.

A Source Book in Astronomy. By Harlow Shapley, Ph.D., LL.D., Director of the Harvard Observatory, and Helen E. Howarth, A.B., A.M., Research Assistant at the Harvard Observatory. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1929. (Pp. xvi + 412.) Price 20*s*. net.

Introduction to Theoretical Physics. By Leigh Page, Ph.D., Professor of Mathematical Physics in Yale University. London: Macmillan & Co., 1929. (Pp. x + 587.) Price 25*s*. net.

Dynamics. A Text-book for the Use of the Higher Divisions in Schools and for First Year Students at the Universities. By A. S. Ramsey, M.A., President of Magdalene College, Cambridge. Cambridge: at the University Press, 1929. (Pp. xii + 259.) Price 10*s*. 6*d*. net.

The Theory of Heat. By Thomas Preston, M.A., D.Sc., F.R.S., Fellow of the Royal University of Ireland. Fourth Edition. Edited by J. Rogerson Cotter, M.A. London: Macmillan & Co., St. Martin's Street, 1929. (Pp. xix + 836.) Price 25*s*. net.

The Principles of Mechanics. An Elementary Course. By H. C. Plummer, M.A., F.R.S., late Royal Astronomer of Ireland. London: G. Bell & Sons, 1929. (Pp. xii + 307.) Price 15*s*. net.

Light. An Introductory Text-Book. By C. G. Vernon, M.A., B.Sc., Head of the Science Department, Bedales School. Cambridge: at the University Press, 1929. (Pp. vii + 191.) Price 3*s*. 6*d*. net; with Exercises, 4*s*.

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On the Origin of Species. By Means of Natural Selection. By Charles Darwin. A Reprint of the Second Edition. London: Oxford University Press. (Pp. lxi + 454.) Price cloth 2*s.*, leather 3*s.* 6*d.* net.

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The Zoological Section of the Nuzhatu-l-Qulūb of Hamdullāh Al-Mustaufī Al-Qazwīnī. Edited, Translated, and Annotated by Lieut.-Colonel J. Stephenson, C.I.E., M.B., D.Sc., F.R.C.S., Lecturer in Zoology, Edinburgh University. Oriental Translation Fund, New Series, Vol. XXX. London: Royal Asiatic Society, 74 Grosvenor Street, W.1, 1928. (Pp. xix + 100.)

A Brief Course in Biology. By Walter H. Wellhouse, Associate Professor of Entomology, Iowa State College, and George O. Hendrickson, Instructor in Zoology, Iowa State College. New York: The Macmillan Company, 1928. (Pp. xii + 200, with 53 figures.) Price 7*s*. 6*d*. net.

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Organic Laboratory Methods. By the late Professor Lassar-Cohn (Königsberg). Authorised Translation from the General Part of Fifth Revised Edition. By Ralph E. Olsper, Ph.D. Edited by Roger Adams and Hans T. Clarke. London: Baillière, Tindall & Cox, 8 Henrietta Street, Covent Garden, W.C.2, 1928. (Pp. xi + 469, with 186 figures.) Price 30*s*. net.

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Roger Bacon. By A. G. Little, D.Litt., Fellow of the Academy. Annual Lecture on a Master Mind. From the Proceedings of the British Academy. Volume XIV. London: Humphrey Milford, Amen House, E.C. (Pp. 34.) Price 2*s*. net.

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SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

MATHEMATICS. By E. C. TITCHMARSH, M.A., University College, London.

A LONG and obviously important memoir on the theory of functions by Prof. G. Pólya of Zürich has just appeared in the *Mathematische Zeitschrift*, vol. xxix. (1929), pp. 549–640. It is entitled "Untersuchungen über Lücken und Singularitaten von Potenzreihen," and is concerned primarily with the well-known problem, first considered by Hadamard, of determining the singularities of a function

$$f(z) = a_0 + a_1 z + a_2 z^2 + \ldots$$

from a knowledge of the coefficients a_0, a_1, . . . in its power series. It is already known that there are connections between the singularities of the function and the occurrence of gaps in the series, due to the vanishing of some of the coefficients; and the memoir gives a systematic account of this phenomenon, and extends it in various new directions. We shall give a brief account of its contents.

We denote by $N(r)$ the number of non-zero coefficients a_n in the above series for which $n \leqq r$. If none of the coefficients are zero, then, as r tends to infinity, the limit of $N(r)/r$ is one. This limit, lim $N(r)/r$, is known as the density (*Dichte*) of the coefficients. It exists for the most familiar and regular types of series, but not for the more irregular types which present some of the most interesting problems. So we also consider the following more general limits:

$$\lim_{\xi \to 1-0} \varliminf_{r \to \infty} \frac{N(r) - N(r\xi)}{r - r\xi}, \quad \varliminf_{r \to \infty} \frac{N(r)}{r},$$

$$\varlimsup_{r \to \infty} \frac{N(r)}{r}, \quad \lim_{\xi \to 1-0} \varlimsup_{r \to \infty} \frac{N(r) - N(r\xi)}{r - r\xi}.$$

These limits always exist, whether the density exists or not. They are known as the minimal density, lower density, upper

density, and maximal density respectively. They are non-decreasing from left to right as they are written here. If the density exists they are all equal to it, and in any case they all lie between 0 and 1. It is these four generalised densities which appear to be most closely connected with the distribution of the singularities of the function $f(z)$.

Some of the results about them can be stated quite simply. If $f(z)$ has just one singularity on the circle of convergence of the series, then the maximal density is unity; and the upper density is positive, though it may be arbitrarily near to zero. If, further, this singularity s is "almost isolated," *i.e.* if a concentric circle of slightly larger radius contains no singularities except on the radius-vector through s, then not only the maximal density but also the upper density is unity. The lower density may, however, still be zero. If the singularity s is isolated and is not a branch-point, *i.e.* is a pole or essential singularity, then the density exists and is equal to unity.

Now consider the case of a function with several singularities on the circle of convergence of its power-series. If they are all algebraic-logarithmic branch-points, the minimal density of the coefficient is positive. If at least one of them is isolated, the lower density is positive. If an arc of angle α of the circle of convergence is free from singularities, then the minimal density is not less than $\alpha/2\pi$.

There are also results of the opposite type. If the density of the coefficients of a power-series is zero, the function cannot be continued beyond the circle of convergence of the series. We are thus led to a new proof of the result known as Fabry's gap-theorem, that a power series

$$\sum_{n=0}^{\infty} C_n z^{\lambda_n},$$

in which $\lambda_n/n \to \infty$, cannot be continued beyond its circle of convergence. Even if we only know that the lower density is zero, we can still say that the function is one-valued and that its region of existence is simply connected. If we are merely given that the minimal density is zero, the function need not be one-valued, nor need its region of existence be simply connected.

The second section deals with integral functions of exponential type, *i.e.* integral functions $F(z)$ which satisfy an inequality of the form

$$|F(z)| < Ae^{a|z|}$$

An important idea in the theory of these functions is that of their "indicator diagrams." Suppose that

$$F(z) = a_0 + \frac{a_1}{1!} z + \frac{a_2}{2!} z^2 + \dots,$$

and consider at the same time the function

$$g(z) = \frac{a_0}{z} + \frac{a_1}{z^2} + \dots,$$

which is regular for sufficiently large values of z. We define the " conjugate diagram " roughly as the smallest convex region which includes all the singularities of $g(z)$; or more precisely as the greatest common part of all convex regions outside which $g(z)$ is regular. We define $k(\phi)$ to be the length of the intercept on a line making an angle ϕ with the positive real axis, by the tangent to the conjugate diagram which is perpendicular to it ; $k(\phi)$ is called the " *Stutzfunktion* " of the conjugate diagram.

The indicator diagram is the convex region of which $h(\phi) = k(-\phi)$ is the " *Stutzfunktion*." This function $h(\phi)$ indicates the order of magnitude of the function $F(z)$ in the direction arg $z = \phi$; more precisely

$$h(\phi) = \overline{\lim_{r \to \infty}} \frac{\log|F(re^{i\phi})|}{r}.$$

The object of this chapter is to use the indicator diagram to connect various new properties of the functions of this type. Another function which can also be dealt with by means of the same indicator-diagram is

$$F(0) + F(1)e^{-s} + F(2)e^{-2s} + \dots.$$

We now come to some remarkable results which bring the above investigations into close connection with recent work on the extensions of Picard's theorem on functions which omit certain particular values. Picard's theorem is that every integral function takes every value, with at most one exception, an infinity of times. Julia obtained the more precise result that to every integral function $G(z)$ corresponds at least one half-line through the origin, such that, in any angle, however small, containing this half-line, $G(z)$ takes every value with at most one exception an infinity of times. The directions of such half-lines are called the Julia's directions of $G(z)$. For example, the function e^z has two such directions, the positive and negative imaginary axes. Now it has been remarked by Bloch that there is a certain analogy between integral functions, which have at least one Julia's direction, and power series with a finite radius of convergence, which have at least one singularity on the circle of convergence.

Prof. Pólya has discovered that there is a case in which this somewhat vague analogy becomes a precise connection. The connection is suggested by a remark of Borel, that the radii-vectores through the origin to the singularities of $f(z)$ on its circle of convergence give the directions in which the integral function $F(z)$ increases most rapidly. Hence every theorem about the singularities on the circle of convergence of $f(z)$ can be translated into a theorem about the directions of most rapid increase of $F(z)$. This, of course, only applies directly to functions of exponential type, which are the only ones related to functions $f(z)$ in the above way; but the argument can be extended so as to yield theorems about functions of any finite order.

Now there is a case in which the directions of most rapid increase are certainly Julia's directions, the case of integral functions of infinite order. This suggests the following procedure. Take any theorem on the coefficients and singularities on the circle of convergence of $f(z)$, for example, Fabry's gap theorem. "If the density of the coefficients of a power series of finite radius of convergence is zero, every point on the circle of convergence is a singular point." This can be translated into a statement about integral functions of exponential type. But the proof can be extended to integral functions in general, and we obtain a theorem which may be expressed roughly as follows: "An integral function for which the density of the coefficients is zero increases equally rapidly in all directions." For functions of infinite order we deduce the following theorem: an integral function of infinite order for which the density of the coefficient is zero has all directions as Julia's directions.

Dirichlet Series.—A paper by Srivastava, "Sur les singularités d'une classe de séries de Dirichlet" (*Comptes Rendus*, **188**, Jan. 14, 1929), deals with transformations of analytic functions not unlike some of those in Pólya's paper. He considers the Dirichlet series

$$H(s) = \sum_{n=0}^{\infty} \phi(\log n) n^{-(s+1)}$$

and the power series in e^{-s}

$$F(s) = \sum_{n=0}^{\infty} \phi(n) e^{-ns}.$$

Here $s = \sigma + i t$ is a complex variable, and the function $\phi(z)$ is an analytic function of $z = re^{i\theta}$ in the angle $|\theta| \leqq \alpha$, where $\alpha \geqq \frac{1}{2}\pi$; and $\phi(z) = O(e^{kr})$ in this angle, where $k < \pi$.

We can express $H(s)$ in terms of integrals involving $\phi(z)$, and so prove that

$$\mathrm{H}(s) = \mathrm{G}(s) + \int_0^\infty \phi(z)e^{-sz}\,dz = \mathrm{G}(s) + \mathrm{J}(s),$$

where $\mathrm{G}(s)$ is an integral function of s. By varying the contour of integration we can prove that $\mathrm{J}(s)$ is regular outside the region consisting of the semi-circle $\sigma^2 + t^2 = (k + \epsilon)^2$, $\sigma > 0$, together with the strip between the tangents at its ends. Hence all the singularities of $\mathrm{J}(s)$ lie in the strip $|t| \leqq \pi$.

Treating $\mathrm{F}(s)$ in the same way we find that

$$\mathrm{F}(s) = g(s) + \mathrm{J}(s),$$

where $g(s)$ is regular in any finite part of the strip $|t| \leqq \pi$. Comparing these two results, we see that the singularities of $\mathrm{H}(s)$ are those of $\mathrm{F}(s)$ which lie in the strip $|t| \leqq \pi$. Hence the singularities of $\mathrm{H}(s)$ can always be determined if those of $\mathrm{F}(s)$ are known.

Another paper on Dirichlet series, by V. Bernstein, " Sur les points singuliers des fonctions représentées par des séries de Dirichlet " (*Comptes Rendus*, **188**, Feb. 18, 1929), deals with the singularities of Dirichlet series of the type

$$\sum_{n=1}^{\infty} \mathrm{A}_n e^{-\lambda_n s}.$$

We suppose that $\lambda_1 < \lambda_2 < \ldots$ are real numbers such that

$$\lim \frac{n}{\lambda_n} = \mathrm{D},$$

and that

$$\mathrm{C}(z) = \prod_1^\infty \left(1 - \frac{z^2}{\lambda_n^2}\right),\quad \delta = \lim_{n\to\infty} \log\frac{|\mathrm{C}'(\lambda_n)|}{n}.$$

It is known from the work of Carlson that this number δ cannot be positive. Bernstein's results are as follows :

(i) If, and only if, $\delta = 0$, every Dirichlet series of the above type with a finite abscissa of convergence has singularities in every interval on the line of convergence of length greater than $2\pi\mathrm{D}$.

(ii) If δ is negative, every series of the type considered has singularities in the finite part of the plane, but they may not be on the line of convergence. There are certainly singularities in every isosceles triangle whose base of length $2l$ is on the line of convergence, whose vertex is to the left of this line, and whose base-angle α satisfies the condition $(l - \pi)\tan\alpha \geqq |\delta|$.

(iii) If $\delta = -\infty$, these are series of the type considered which represent integral functions, though the abscissa of convergence of the series may be finite.

Orthogonal Functions.—In a note in the *Comptes Rendus*, **188**, Jan. 14, 1929, entitled "Sur l'unicité du système, de fonctions orthogonales invariant relativement à la dérivation," B. Gagaeff solves the following curious problem. We know that the functions

$$1, \cos x, \sin x, \ldots \cos n x, \sin n x \ldots$$

form an orthogonal system over the interval $(0, 2\pi)$. Also if we differentiate each of these functions we obtain the same system again, apart from multiplication by numerical factors. The problem is to discover whether there is any other system of functions with the same property, viz. that

$$\phi_0(x), \phi_1(x), \ldots \phi_n(x) \ldots$$

are orthogonal, and that

$$\phi_0'(x), \phi_1'(x), \ldots \phi_n'(x) \ldots$$

gives the same system again, apart from numerical factors and the order of the functions.

The answer is, as we might expect, in the negative. If the functions $\phi_n(x)$ satisfy the conditions, then they are the well-known system of sines and cosines.

Some quite simple remarks put us on the way towards the solution. In the first place, if the order of ϕ_n in the sequence is not altered by differentiation, then there is a constant λ such that $\phi_n'(x) = \lambda \phi_n(x)$; hence $\phi_n(x) = Ce^{\lambda x}$, where C is a constant. Since all the functions are supposed to be real, λ is real; and two functions $e^{\lambda x}$, $e^{\mu x}$, are not orthogonal over $(0, 1)$. Hence at most one of the functions can preserve its order after differentiation. We call this function $\phi_0(x)$.

Now consider the functions $\phi_n(x)$, $\phi_n'(x)$, where $n > 0$. As they are different functions of a orthogonal system

$$\int_0^1 \phi_n(x)\phi_n'(x)dx = 0.$$

Hence $\phi_n^2(1) = \phi_n^2(0)$, *i.e.* $\phi_n(1) = \epsilon_n \phi_n(0)$, where $\epsilon_n = \pm 1$. Similarly $\phi_n'(1) = \epsilon_n' \phi_n'(0$, where $\epsilon_n' = \pm 1$.

Again, integration by parts gives

$$\int_0^1 \phi_n \phi_n'' \, dx = \phi_n(0) \phi_n'(0) (\epsilon_n \epsilon_n' - 1) - \int_0^1 (\phi_n')^2 \, dx.$$

Suppose that the integrated term is zero, which will evidently happen in a variety of cases. Then the whole right-hand side is plainly negative. Hence ϕ_n'' is not orthogonal to ϕ_n, and so, by the general property of our sequence, it must be a multiple of ϕ_n, and clearly from the above equation a

negative multiple. Thus $\phi_n''(x) = -k^2 \phi_n(x)$, and consequently $\phi_n(x)$ is a trigonometrical function.

There is, of course, more in the proof than this, but the way in which we arrive at trigonometrical functions is now clear.

This problem reminds us of another question about orthogonal functions which we have never seen answered. Suppose that we take the system of cosines only,

$$1, \cos x, \cos 2x, \ldots \cos nx, \ldots$$

which is orthogonal over the interval $(0, \pi)$. These functions have the peculiarity that the nth is of the form $\phi(nx)$, *i.e.* it involves n and x in the form of their product only, whereas this is not true of orthogonal functions in general. It may reasonably be conjectured that this property also is characteristic of trigonometrical functions; that is to say, if $\phi(x)$ is a function, defined for all positive values of x, such that

$$\phi(x), \phi(2x), \ldots \phi(nx)$$

is an orthogonal system over a given finite interval, then $\phi(x)$ must be a sine or cosine. However, we have never seen this subject discussed, and it may possibly be much more difficult than the problem of differentiation.

Functions of écart fini.—The idea of a function of *écart fini* is due to Hadamard, and is of interest in the theory of Fourier series. A function $f(x)$ which is integrable in the sense of Lebesgue over the interval $(-\pi, \pi)$ is said to be of *écart fini* in this interval if the integrals

$$n\int_a^b f(x) \sin nx\, dx, \quad n\int_a^b f(x) \cos nx\, dx$$

are bounded as $n \to \infty$, uniformly with respect to a and b. A well-known result is that every function of bounded variation is of *écart fini*; but there are functions of *écart fini* which are not of bounded variation.

The main object of a paper on this subject by H. E. Bray (*American Journal of Mathematics*, **51**, 1929, 149–64) is to obtain necessary and sufficient conditions for a function to be of *écart fini*. The writer begins by proving that a function of *écart fini* is "equivalent" to a bounded function. This means that it is equal almost everywhere to a function which is bounded. In fact, it is proved that, at every point where $f(x)$ is the derivative of its indefinite integral $F(x)$ (and so almost everywhere), $|f(x)|$ is less than a constant depending on the above integrals.

As may be expected, the necessary and sufficient condi-

tions are not particularly simple. The first such condition, obtained in terms of $F(x)$, is that the sum

$$\sum_{r=1}^{m} (-1)^r [F(x+r\delta) - F\{x+(r-1)\delta\}],$$

where $m\delta \leqq 2\pi$, should be $O(\delta)$ as $\delta \to 0$, uniformly with respect to m. If $f(x)$ is bounded, the necessary and sufficient condition that it should be of *écart fini* is that

$$F(x) - 2F(x+\pi/n) + 2F(x+2\pi/n) - \ldots - F\{x+(2n-1)\pi/n\} + F(x+2\pi)$$

should be $O(1/n)$, uniformly with respect to x.

We can also state conditions that a function defined as an integral should be of *écart fini*. If

$$f(x) = f(x_0) + \int_{x_0}^{x} g(t)dt \quad (0 < x_0 < 2\pi),$$

where $g(x)$ is of bounded variation over $0 < \epsilon \leqq x \leqq \pi - \epsilon$, and if its total variation over this interval is $O(1/\epsilon)$, then $f(x)$ is of *écart fini* over the whole interval. A typical theorem of another type is that if $f(x)$ is of *écart fini*, and $\alpha(x)$ of bounded variation, then $\alpha(x) f(x)$ is of *écart fini*.

The author gives a number of applications of this idea to the theory of Fourier series, and proves in conclusion that the Fourier series of a continuous periodic function of *écart fini* converges uniformly to that function.

PHYSICS. By L. F. Bates, B.Sc., Ph.D., F.Inst.P., University College, London.

The Striated Discharge.—In the July issue of the *Philosophical Magazine* Sir J. J. Thomson contributes a very interesting article on striations, the cathode dark space, and the negative glow. The basis of the article is that the losses of electrons in a discharge tube, which are due to recombination of electrons with positive ions, to their attachment to neutral molecules or to diffusion, must be counterbalanced by the production of free electrons, and that the most stable form of the discharge will be one in which the necessary free electrons are produced with a minimum expenditure of energy. Under these circumstances, the current through the discharge tube will be maintained by a minimum difference of potential between the electrodes. The free electrons thus produced must, of course, be liberated by collisions between molecules and other electrons. Now, we have to remember that many of these collisions will not result in the production of free electrons, although an impacting electron will in general lose energy by placing the impacted molecule in an excited state. The energy which

the impacting electrons lose in this way may be considerable, in fact, the energy spent per ion produced is not measured by V_0, the ionisation potential, but by $V_0 + \beta \cdot R$, where R is the radiation potential and β is the number of inelastic resonance collisions which the impacting electron makes to one collision resulting in ionisation. The term $\beta \cdot R$ may be greater than V_0, and the value of β will depend on the velocity of the impacting electrons. It appears that the probability of an ionisation collision is very small when this energy is not much greater than that corresponding to the ionisation potential, and the probability of an ionisation collision increases until this energy is of the order of that corresponding to a fall of 150 to 200 volts, after which it decreases with increase in energy (*cf.* Kossel, *Ann. der Phys.*, vol. 37, p. 393, 1912). So we see that the swift electrons (up to 200 volts) are the most efficient ionising agents. Now, the probability of an inelastic resonance collision appears to be a maximum when the energy of the impacting electron is only a little greater than that corresponding to the resonance potential, and to diminish as the energy increases (*cf.* K. T. Compton and Mohler, *Bull. Nat. Res. Coun.*, vol. 9, p. 52, 1924). The author, therefore, proposes that in view of these considerations a striated discharge is one in which the potential difference in the striation is less than that for the uniform positive column ; in other words, striation occurs because the energy spent in non-ionising collisions under these conditions is less than under conditions of uniform electric force. We may now briefly review the considerations which determine the manner in which the electric force in a striated discharge varies from point to point. Commencing with the negative glow, the electric force here is extremely small and the energy for the intense ionisation which occurs there is supplied by cathode rays which start from the cathode and the dark space, and not from processes peculiar to the glow itself. Now, beyond the glow, ionisation will not be produced unless an electric field is established between the glow and the anode. The electrons which emerge from the negative glow produce a negative space charge in the region beyond the end of the glow, and hence the electric force increases as the distance from the end of the glow increases. Until this distance is sufficient to permit the establishment of a difference of potential equal to the resonance potential the discharge will remain dark. Hence, we have a dark interval, the Faraday dark space. As the distance increases the luminosity is maintained, but no ionisation occurs for some distance, so that no positive ions are formed, and the electric field continues to increase with distance. Indeed, the field will increase rather more

rapidly owing to the loss of energy by inelastic collisions. Eventually, the ionisation potential is reached, and ionisation sets in. This means the formation of positive ions and more free electrons, but the latter move away so very quickly that the positive ions practically remain behind, and serve to neutralise the effect of the negative charge and so decrease the field, but eventually a point will be reached where the positive and negative charges will balance each other. Beyond this point we shall have a positive space charge and the field will decrease as the distance from the cathode increases, so that ionisation will soon cease. The negative glow will therefore cease, and a new Faraday dark space will appear, where the energy of the electron stream is less than that corresponding to the resonance potential, and the above processes will then be repeated. Hence, according to the above argument, the kinetic energy of an electron in the striated discharge at the cathode end, or head of the striation, rises above the value necessary to supply local electron losses, and falls to a very low value at the anode end or tail of the striation. This type of discharge, however, is not the only possible type of striated discharge, and other types of discharge are mentioned in the paper.

The above arguments lead us to suppose that anything which tends to increase the wastage of energy in resonance collisions will increase the tendency to form striations. Hence, the introduction of an impurity which possesses a complicated molecule, or which tends to combine with the gas in the tube, should increase the tendency to form striations. It may be added that the presence of an electronegative gas would certainly shorten the free life of an electron, but it need not necessarily increase the tendency to form striations. This can only happen if the probability of an electron being captured by a neutral molecule diminishes as the energy of the electron increases.

The assumption that an electron is only able to excite resonance radiation when its energy closely approximates to that corresponding to a radiation potential also provides a simple explanation of the cathode dark space and the negative glow. If an electron only produces radiation when its energy lies between certain limits, then it is easy to see that the length of the path it describes whilst producing radiation is inversely proportional to the strength of the electric field in which it travels. This means that the probability of a radiation collision and, therefore, the luminosity, will be proportional to the length of path. Thus the luminosity produced by electrons in an electric field will vary inversely as the field strength. In this connection, Aston has shown that the electric field in

the cathode dark space at any point is directly proportional to the distance from the boundary of this space, so that the efficiency of the electron in producing light will be inversely proportional to its distance from the boundary. We ought, therefore, to observe a very great increase in luminosity near the boundary. The luminosity in this region will also be increased by the effects produced by electrons liberated in the dark space near the boundary. These acquire energy corresponding to the radiation potential in travelling to the boundary. This means that the luminosity at the boundary, *i.e.* at the commencement of the negative glow, will be very considerable over a region corresponding to the collision-free path of the electron. Should the gas in the tube possess several radiation potentials, then we should expect to observe the establishment of a series of luminous layers of different colours. Another interesting point arises if the light emitted by an electron is proportional to the length of path it traverses before acquiring a definite increment of energy, and that is the luminosity should be increased in the presence of a magnetic field perpendicular to the path of the discharge. In these circumstances the electron would be forced to travel along a path inclined to the lines of electric force, and, therefore, the length of path necessary to acquire a definite increment of potential would be increased. The author states that preliminary experiments have given results in accordance with these views.

Let us now turn our attention to the faster electrons which possess sufficient energy to ionise the gas. It has already been mentioned that the probability of an ionisation collision increases to a maximum for energies corresponding to about 200 volts, and then decreases rapidly. The electrons which cross the cathode dark space will exhibit a range of energy from that corresponding to the cathode fall of potential down to very small values. Hence the various electrons will lose energy by ionisation at different rates, and therefore the energy distribution in the electron stream will change as it passes through the negative glow. In fact, we can see that energy losses through ionisation will result in an increase in the numbers of very swift and of very slow electrons. The latter will be easily lost by scattering and absorption by the sides of the tube, and, finally, we shall be left with a stream of high speed electrons of low ionising power. Such a stream is frequently observed to pass through the negative glow, the positive column, and the striations. The electric force in the negative glow is very small, and the electrons and positive ions liberated by collision will accumulate there until they are plentiful enough to carry the current by diffusion. As the

distance from the boundary of the cathode dark space increases the densities of the positive ions and electrons will decrease, so that their gradient will slope towards the anode. In addition there will be a very steep gradient at the junction of the negative glow and the dark space. The electric field prevents the diffusion of electrons into the dark space if they pass the boundary, but the field will aid the diffusion of positive ions into this space, and thus serve to maintain a steep potential gradient, and, consequently, cause still more positive ions to travel into it.

Throughout his paper Sir J. J. Thomson emphasises our lack of information of the efficiency of ionisation by electron impact, and it is therefore interesting to turn to a paper by W. Bleakney (*Phys. Rev.*, vol. 34, p. 157, 1929), where a new method of positive ray analysis is described. The method may be quite generally applied to the study of the nature of the ions formed by single electron impacts in gases, the efficiency of these impacts, and the measurement of ionisation potentials. In this new method a narrow beam of electrons from a hot filament is made to travel along a straight path under the influence of a strong longitudinal magnetic field. The beam passes through gas at such a low pressure that only a fraction of the electrons are able to collide with molecules of gas. The path lies between two parallel plates, between which is maintained an electric field sufficient to draw out the positive ions formed by collision. In other words, a linear source of positive rays of almost uniform distribution is provided. The presence of the magnetic field ensures that the electric field does not appreciably modify the path or velocity of the electron beam. A long narrow slit is cut in one of the plates, so that a sheet of positive ions may pass through and be subjected to analysis. The form of the analysing apparatus depends, of course, on the problem under investigation. The form actually used in the determination of the first four ionisation potentials of mercury, was one in which the analysing magnetic field was provided by a long solenoid, and the analysing electric field was maintained between two parallel plates, so that ions of a chosen e/m could pass through the slit, and without suffering deviation in the analysing fields, fall upon a collector. The metallic portions of the apparatus were made entirely of copper, with pyrex glass insulation, and it could be well baked out and exhausted. The experimental procedure was to maintain a fixed magnetic field and to analyse the ions by varying the analysing electric field. The total ionisation was measured by the current passing to the slit plate, and the collector measured the relative number of the ions of each kind formed. The new method appears to

possess several advantages. For example, the electron velocity is uniform and well defined, ionisation can be studied at very low pressures *i.e.* with single electron impacts, only one analysing field need be varied, the gas pressure in the tube can be accurately measured and the effects of space charges are negligible. Preliminary results give the values of the ionisation potentials for the production of mercury ions with two, three, and four charges to be 30, 71, and 143 volts respectively. Ions bearing five charges were also obtained, and these were also considered to arise from single impacts, owing to the low current densities employed.

New Apparatus.—Among recently designed apparatus we may note the details of a high temperature molybdenum or tungsten furnace designed by Duffendack and Black (*Phys. Rev.*, vol. 34, p. 85, 1929) for the investigation of the arc and spark spectra of elements with high boiling points. Furnaces for such purposes have previously been designed. For example, King (*Astrophys. Journ.*, vol. 28, p. 300, 1908) employed a thin horizontal tube of graphite, heated by a current of about 1,500 amperes, and enclosed in a chamber which could be evacuated down to a few mm. of mercury. Zumstein (*Phys. Rev.*, vol. 27, p. 562, 1926) also used a horizontal carbon tube, but he mounted it beneath a ventilating hood and heated it by means of a large oxyacetylene blower, no attempt at evacuation being made. The furnace of McLennan and McLay (*Trans. Roy. Soc. Can.*, Sec. III, p. 89, 1925) also consisted of a horizontal carbon tube, which formed the upper electrode of a carbon arc, the lower electrode being touched to the middle of the upper electrode and the arc maintained between them. An earlier tungsten furnace designed by Duffendack (*Phys. Rev.*, vol. 20, p. 665, 1922) consisted of a thin horizontal tube of tungsten clamped between heavy water-cooled leads, enclosed in an evacuated chamber and heated electrically. In the latest furnace, a sheet of tungsten or molybdenum, about $\frac{1}{1000}$th of an inch thick, is rolled into a cylinder of about 2 cm. diameter and 9 cm. long. It is clamped horizontally between two heavy water-cooled copper rods and heated by a heavy current, up to 375 amperes, from a low voltage transformer. The ends of the cylinder are split and flared into a fan shape to permit clamping with soft steel clamps. A tungsten filament extends through the cylinder and serves as a cathode for low voltage arc studies, the anode being a tungsten trough running through the cylinder. The trough is supported by insulating arms and connected to the base of the furnace. It contains the substance to be investigated. The whole is mounted inside a large evacuated chamber, through the base of which the insulated leads are sealed. The chamber

is provided with quartz windows and surrounded by a water jacket. The vertical walls of the chamber consist of seamless copper tube, the remaining walls being plates of pure copper. The inner walls are heavily nickel-plated as an additional precaution against diffusion of water vapour and gas through the walls.

A new receiver for Hertzian waves, constructed on the principle of the bolometer, is described in a paper by H. Dänzer (*Ann. der Phys.*, vol. 2, p. 27, 1929). Two resonators were placed in diametrically opposite arms of a Wheatstone bridge, so that when electric waves fell upon them the resistances of these bridge arms were increased and the balance upset. In order to obtain maximum change of resistance, Wollaston wire was used. A piece of this wire was taken and the middle portion was bent into the form of a loop. It was soldered to two brass rods, which were mounted about 2 mm. apart on insulating material. The free ends of the Wollaston wire formed the antennæ of the resonator. The loop-shaped portion was then exposed to nitric acid, and the silver dissolved, leaving a thin platinum wire to form a bolometer. The acid process was continued until a wire of suitable resistance, about 150 ohms, was produced, when the wire was washed in dilute alcohol. If the silver coating on the Wollaston wire was insufficient, pieces of thin silver foil could be attached to the ends of the wire to act as antennæ. The resonator system was enclosed in an ebonite capsule, coated inside and outside with Picein.

In a paper by C. G. Abbot on the energy spectra of the stars (*Astrophys. Journ.*, vol. 69, p. 293, 1929) we are introduced to a new use for the wings of the common housefly. He describes a very sensitive radiometer constructed with vanes of houseflies' wings. The vanes were mounted on a glass fibre and were approximately ·4 mm. wide and 1·0 mm. high. Each vane had three parallel laminæ of flies' wings, of which the anterior one was painted dead black and the two posterior ones left unpainted. The separation of the laminæ was about ·1 mm., so that the conduction of heat from front to back was exceedingly difficult. The mirror, ·9 mm. by 1·0 mm., was mounted about 3 cm. above the vanes. It was made of microscope cover glass, ground and polished to half thickness, and platinised on both sides by sputtering. It weighed less than 1 mg. The system was suspended by an extremely fine quartz fibre. The glass portions were cemented by minute touches of shellac evaporated under heat and reduced pressure; the vanes were attached by bees' wax. The radiometer was mounted in a quartz tube about 4 cm. in diameter. The middle portions of the

tube were worked inside and out to concentric circular curvature. The tube was exhausted and repeatedly washed out with electrolytic hydrogen, and finally filled with this gas to a pressure of ·23 mm. and sealed. Hydrogen was used to reduce damping, and the worked quartz surface meant that the mirror and vanes could take up any orientation, as the whole tube was mounted so that it could easily be turned in any direction. The period of oscillation of the system appeared to change in a peculiar manner due to unknown influences, thought to be of electrostatic origin. The instrument was employed to determine the distribution of energy in stellar spectra formed by a 60° flint glass prism placed at the coudé focus of the Mount Wilson 100-inch telescope.

The Diffraction of Electrons.—In this section we have previously discussed the diffraction of electrons by thin metal films, but our discussions have been confined to photographic methods of investigation. It is therefore of interest to note that E. Rupp (*Ann. der Phys.*, vol. 1, p. 773, 1929) has recently carried out experiments in which electrical methods have been employed. Photographic methods are limited, because, although they give us a permanent record of the position of the diffraction rings, they tell us little or nothing about the number of electrons and their velocity distribution in these rings. The object of Rupp's investigations was to find the spatial and velocity distribution of the diffracted electrons. Two experimental arrangements were used. Firstly, a small Faraday collecting cylinder was placed in different positions to measure the number of electrons diffracted through different angles with respect to the metal film. Secondly, a ring-shaped collector was used to collect all the electrons which were diffracted within a given diffraction ring, the given diffraction ring being made to fall upon the collector by suitable adjustment of the velocity of the incident beam of electrons. In both arrangements a grid could be placed before the collector, and a retarding field applied to investigate the velocity distribution of the electrons captured by the collector. Now, according to G. P. Thomson, the position of the *n*th diffraction ring is given by $\sin \phi/2 = \frac{n}{2\mu d}\sqrt{\frac{150}{V}}$, where ϕ is the angle of diffraction, *i.e.* the angle between a ray incident upon the thin metal film and a ray through the ring, μ the refractive index of the electron waves, d the grating constant of the metal and V the voltage which measures the velocity of the incident electrons. If, then, V is reduced from larger to smaller values, the angle of diffraction for a given ring correspondingly increases, and by suitable adjustment of V, any required ring may be made to cover the ring-

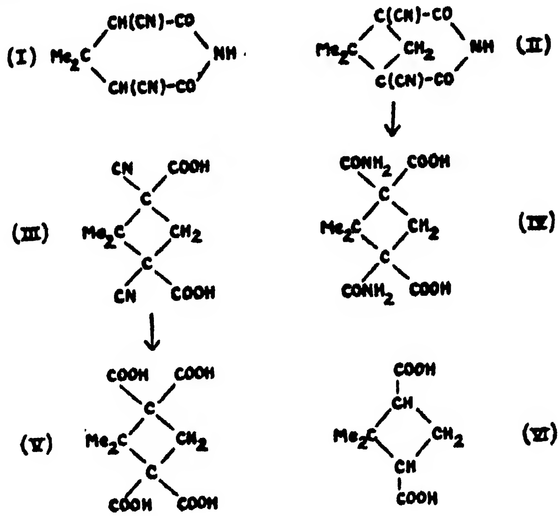
shaped collector. The apparatus was thus of comparatively simple design. The experiments showed that the electrons which were received by the ring-shaped collector had suffered no appreciable loss of velocity, whilst among the electrons received by a centrally placed cylinder were many of low velocity. In other words, electrons which suffer no loss of velocity may be considered as diffracted electrons, whilst those which suffer appreciable loss of velocity must be considered as scattered electrons. It would thus appear that, in general, a diffracted electron suffers just a single encounter in its passage through the film, whilst a scattered electron suffers several encounters, and so, its chance of reaching a diffraction ring is small. Some evidence was obtained that metal films, notably aluminium and silver, showed selective transmission for electrons of certain velocities.

Rupp discusses the question of the refractive index at some length, and he shows that G. P. Thomson's idea of a refraction at the emergent face when the incident beam of electrons is normal appears to be correct. To show this a metal foil was orientated with respect to an incident electron beam of constant velocity, and the diffraction phenomena were examined in cases where refraction might occur at both the incident and emergent faces of the foil, whereupon appreciable displacement of the diffraction pattern might be expected to occur. As a matter of fact no displacement was observed, although the diffraction patterns were spread out with the larger angles of incidence. The refractive indices of silver and nickel were found to be 1·02 and 1·05 respectively, whence the internal grating potentials were found to be 12 and 17 volts respectively. In a succeeding paper in the same issue of the *Annalen* Rupp finds these potentials to be 14 and 16 volts respectively, by an electron reflection method.

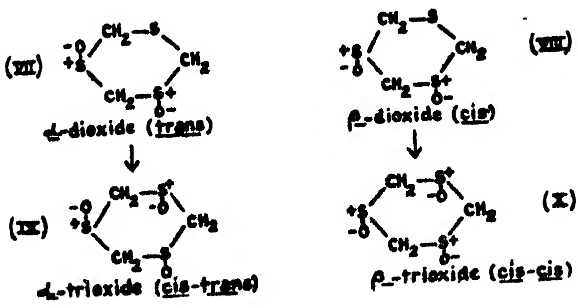
ORGANIC CHEMISTRY. By J. N. E. Day, M.Sc., A.I.C., University College, London.

In view of the difficulty of formation of four-membered carbon rings, reference may be made to a paper by Kerr (*J.A.C.S.*, 1929, **51**, 614), who describes the preparation of norpinic acid (2 : 2-dimethyl*cyclo*butane-1 : 3-dicarboxylic acid [VI]). The starting-point was *αα'*-dicyano-*ββ*-dimethylglutarimide [I]. Methylene iodide reacted with the sodium compound of this substance to give the dicyclic imide [II], the structure of which was shown by the fact that hydrolysis with sulphuric acid gave glutaric acid and phorone. Alkaline hydrolysis gave dicyanonorpinic acid [III] and dicarbamylnorpinic acid [IV]. More concentrated alkali hydrolysed both these compounds to 2 : 2-dimethyl*cyclo*butane-1 : 1 : 3 : 3-tetracarboxylic acid [V],

which, on heating to 200°, gave *trans*-norpinic acid [VI] which had been previously prepared by Perkin and Simonsen from the *cis* acid.

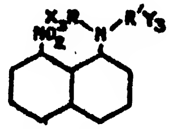


Bell and Bennett (*J.C.S*, 1929, 15) have reinvestigated the oxides of trimethylene trisulphide. They have obtained a monoxide, two dioxides (α and β), and two trioxides (α and β).



That the β-dioxide [VIII] has the *cis*-configuration is shown by the fact that on further oxidation it gives a mixture of the two trioxides, since the oxygen atom may be added on above or below the plane of the ring; the α-dioxide has the *trans*-structure, shown by the fact that it gives only one trioxide, since addition of oxygen above or below the plane of the ring will give the same trioxide. Since the β-trioxide is formed only from the β-dioxide, it must have the *cis-cis*-structure [X], while the α-trioxide [IX], formed from both dioxides, must have the *cis-trans*-configuration.

Reference may be made to an interesting paper by Mills and Elliott (*J.C.S.*, 1928, 1291) on "Molecular Dissymmetry Dependent on Restriction of Rotation about a Single Bond." In this paper an attempt has been made to see if a compound of the type



derived from 1 : 8-nitronaphthylamine would exist in two modifications (*a*) and (*b*), which represent the compounds viewed from above.

(*a*)	(*b*)
NO_2	NO_2
$X_2R—N—R'Y_2$	$Y_2R'—N—RX_2$

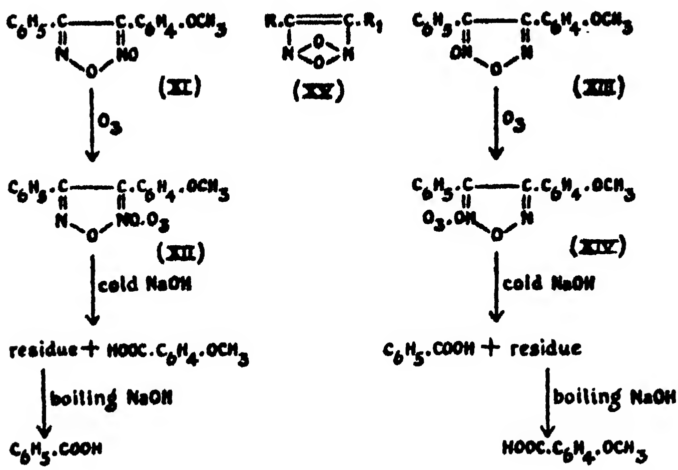
The point of interest was to see if the nitro group could prevent the free rotation of the $X_2R—\overset{|}{N}—R'Y_2$ group round the carbon-nitrogen bond connecting this group to the naphthalene nucleus. The actual compound used was the benzenesulphonyl derivative of 8-nitro-1-naphthyl glycine. Resolution was effected through the brucine salt. Both modifications, however, only possessed a limited stability, and the solutions of both gradually reached equilibrium, the average time of persistence being twenty-five minutes.

The structure of furazan oxides has again been discussed in a paper by Kinney (*J.A.C.S.*, 1929, **51**, 1592) in view of the fact that Meisenheimer, Lange, and Lamparter isolated two substances believed to be isomeric furazan oxides, whereas on the basis of the ethylene linking only one would be expected, see this *Journal*, 1928, xxii, 581.

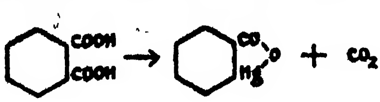
The mixture of the phenyl-*p*-methoxyphenylfurazan oxides, [XI] and [XIII], prepared from *p*-methoxybenzil, through the dioximes, was separated into the α- and β-forms by crystallisation from acetone. Treatment with ozone gave the compounds [XII] and [XIV]. The ozonide from the α-oxide on treatment with cold sodium hydroxide (10 per cent. solution) gave chiefly anisic acid; the residue on boiling with sodium hydroxide gave benzoic acid. Similarly the β-oxide gave with

cold sodium hydroxide mostly benzoic acid, and the residue on boiling with alkali gave chiefly anisic acid.

The fact that two isomeric furazan oxides are now known, shows that the ethylenic structure [XV] is incorrect, and requires the unsymmetrical structures [XI] and [XIII]. As diphenylfurazan did not react with ozone to give acids as the furazan oxides do, the remaining point of attack for the ozone is the NO group ; the aromatic group nearest this is the first to appear as the organic acid after treatment with cold alkali.

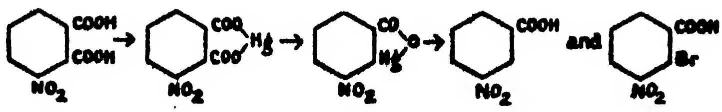


In connection with the entry of mercury into organic compounds mention may be made of three papers by Whitmore and Culhane ; Whitmore and Carnahan ; and Leuck, Perkins, and Whitmore (*J.A.C.S*, 1929, **51**, 602, 856, 1831). Working with phthalic acid, it is known that mercuration and loss of carbon dioxide give an *o*-mercurated benzoic acid :

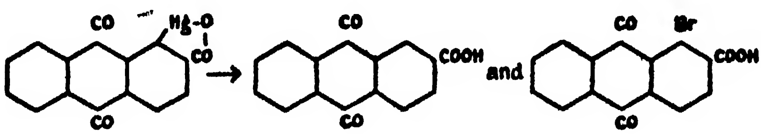


This reaction might take place in two ways : either the mercuric phthalate may lose carbon dioxide, the mercury replacing the carboxyl group, or, under the influence of heat, the mercuric compound may mercurate the benzene ring ortho to a carboxyl group, and this compound, on losing carbon dioxide, would also give the same *o*-mercurated benzoic acid. In order to distinguish between these, the work was carried out with 3-nitrophthalic acid, the resulting mercury compound gave *m*-nitrobenzoic acid with hydrochloric acid, and 2-bromo-3-

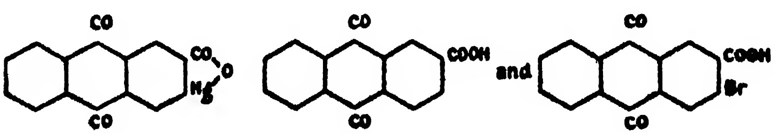
nitrobenzoic acid with bromine, thus showing that there is a replacement of the carboxyl group by mercury.



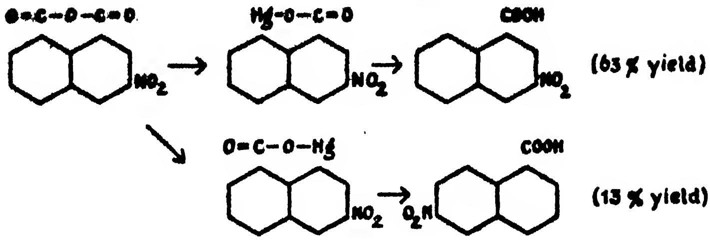
A further study of the replacement of one of two ortho carboxyl groups by mercury was made in the case of 1 : 2-anthraquinonedicarboxylic acid and the corresponding 2 : 3-acid. In the case of the 1 : 2-acid, the mercury salt was prepared and heated until all the carbon dioxide was evolved, and it was shown that the 1-carboxyl group was exclusively replaced, as the mercury compound gave with hydrochloric acid only anthraquinone-2-carboxylic acid, and with bromine only 1-bromoanthraquinone-2-carboxylic acid.

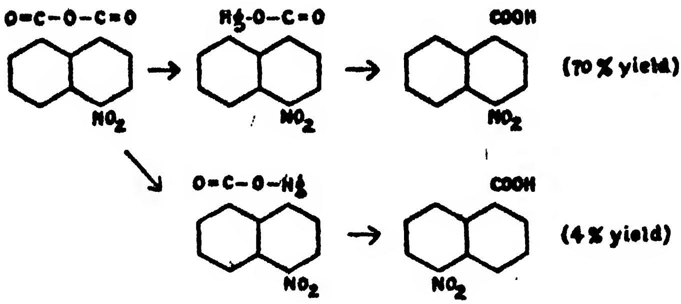


Under similar conditions the 2 : 3-acid gave 3-bromo-anthraquinone-2-carboxylic acid and anthraquinone-2-carboxylic acid.



Similar work was also carried out with naphthalic acid (1 : 8-naphthalenedicarboxylic acid), and 3-nitro- and 4-nitronaphthalic acid. In these cases it was also found that one carboxyl group was replaced by mercury. With the 3-nitro-acid and 4-nitro-acid (actually the anhydrides were mercurated) it was found that either carboxyl group could be replaced, but that the replacement was chiefly in the ring not containing the nitro group (with the nitro group in position 4 less replacement took place in the 1-position than with the nitro group in position 3). The structure of the mercurated products was determined in the usual way by replacing the mercury with hydrogen by boiling with hydrochloric acid. These reactions are stated to be the best for preparing 3-nitro-1-naphthoic acid and 4-nitro-1-naphthoic acid.





McCullough and Cortese (*J.A.C.S.*, 1929, **51**, 225) discuss the use of sulphuric acid in the preparation of alkyl halides and conclude that it should be avoided if the purest halides are required. They give details for the preparation of allyl chloride, bromide and iodide at ordinary temperatures, and state that specimens prepared in this way have been kept for many months without apparent decomposition.

Hurd and Bennett (*J.A.C.S.*, 1929, **51**, 265) give a method for concentrating hydrazine hydrate solutions by distillation with xylene. A 65 per cent. solution was obtained from one of 41·8 per cent. by distilling with an equal volume of xylene; more concentrated solutions, containing up to 95 per cent. of hydrazine hydrate, were obtained by the use of more xylene. The amount of hydrazine which distils over with the mixture of water and xylene is small, and this may be nearly all recovered from the water layer of the distillate after separation from the xylene.

PHYSICAL CHEMISTRY. By R. K. Schofield, M.A., Ph.D. (Cantab). Rothampsted Experimental Station, Harpenden.

Mercury Cathodes.—Because it is a liquid, mercury holds a unique place amongst cathodic materials. This arises partly from the ease with which a fresh, unstrained surface can be obtained and partly because the surface-tension of the electrode can be measured. It is not surprising, therefore, that many of the advances in electrochemistry have originated in the

study of mercury electrodes. The last few years have witnessed a number of investigations in this field which encourage the hope that a still deeper insight into electrolytic phenomena will come from a closer study of mercury electrodes.

Heyrovsky and his co-workers (*Bull. Soc. chim.* 1927, **41**, 1924, and *Handbuch der biologischen Arbeitsmethoden*, 1928, p. 1413) at Prague have recently perfected an instrument, the polarograph, by means of which a steadily increasing E.M.F. is applied to a cell and an automatic record of the resulting current obtained. The cell is a small conical flask. The bottom is covered with mercury which serves the anode, while a fine nozzle from which mercury drops continually break away is used as cathode. On account of the extent of the area of the anode in comparison with that of the cathode, the conditions at the former are not appreciably affected by the application of an external E.M.F., so that the current voltage curves are a reflection of the conditions obtaining at the cathode. When the cathode is not "dropping" the curves are similar to those obtained with solid electrodes. As the applied E.M.F. increases, the current rises in a series of curves, which are only moderately reproducible. With a cropping cathode the curves are much more reproducible, and frequently show maxima and minima. E. Mellanova and Heyrovsky (*Faraday Soc. Trans.*, 1928, **24**, 257) have obtained curves with N/400 nickel chloride, from which oxygen had been carefully excluded, showing practically no current till 1 volt has been applied. From this point the current increases at first exponentially and then linearly till at 1·9 volts it suddenly falls. From 2·2 to 2·8 the current is nearly constant at about half the maximum value, and subsequently increases again due to the evolution of hydrogen.

The sudden fall in the current is a very singular phenomenon. It is inappreciable at 0·0005N and is seven-fold at 0·01N. It is not at all easy to see why an increase in the applied E.M.F. should cause a decrease in the rate of deposition of nickel; yet the existence of the effect has been established beyond question. Curves of the above types have been obtained for the deposition of $Hg_2^{\cdot\cdot}$, $Cu^{\cdot\cdot}$, $Tl^{\cdot\cdot}$, $Pb^{\cdot\cdot}$, $Cd^{\cdot\cdot}$, $Zn^{\cdot\cdot}$, $Ni^{\cdot\cdot}$, $Co^{\cdot\cdot}$, $Fe^{\cdot\cdot}$, $Mn^{\cdot\cdot}$, $Ca^{\cdot\cdot}$, $Sr^{\cdot\cdot}$, $Ba^{\cdot\cdot}$, $Ra^{\cdot\cdot}$, $Na^{\cdot}$, $K^{\cdot}$, $Rb^{\cdot}$, $H^{\cdot}$, and a number of complex and organic ions. The magnitude of the current after the fall depends, for a given cell and drop-rate, only on the concentration of the ions that are being reduced and the temperature. The maximum on the other hand is depressed by the presence of less reducible ions. The effectiveness of these appears to be connected with their volency in a manner that is reminiscent of the Hardy-Schutze law for coagulation. Heyrovsky considers that, at E.M.F.'s

below that at which the maximum current is attained, adsorptive forces are primarily responsible for bringing the reducible ions to the cathode surface, and that the influence of less reducible ions is in proportion to their rate of adsorption. As soon as the rate of reduction exceeds the rate of adsorption the mercury drop as it forms will be surrounded by a layer of solution depleted of reducible ions, and to this factor is attributed the sudden fall in the current.

Whether this is the correct interpretation or not, there can be no doubt that the surface condition undergoes an important change as the E.M.F. is increased over the small range that separates the one condition from the other. The surface-tension, as indicated by the drop-weight, remains nearly constant while the current increases lineally; and then changes rapidly as the maximum is passed. This is a reflection of a similar mode of varieties of the single electrode potential at the cathode. During the rise of the current the increase in the applied E.M.F. is largely balanced by an increase in the potential drop within the cell due to its resistance; when the fall in current occurs the single electrode potential changes rapidly so that the 100 millivolts which separates the maximum and minimum on the ·01N nickel chloride polargram may correspond to a change of more than a volt in the potential difference at the electrode. The lack of coincidence between the applied E.M.F. and the single electrode potential is undoubtedly the main, though probably not the only, reason for the well-known discrepancy between the electrocapillary curves as determined by Kucera's drop-weight method and those obtained by the Lippmann capillary tube method. Heyrovsky and Simunek (*Bull. Acad. Sci. Bohême*, 1927) have shown that oxygen gives rise to maxima in the polargrams similar to those obtained with reducible ions. Sandera finds that the addition of gelatine and colloidal ferric hydroxide eliminates the irregularities, indicating again that absorption is involved.

Using a different experimental method McAuley and Bowden (*Proc. Roy. Soc.*, 1926, **111**, 190) found that on increasing the polarising current across a still mercury cathode, the surface tension and single electrode potential altered only slightly till a certain current had been reached, when a sudden increase in surface tension and change in single electrode potential occurred. On reducing the current the surface tension remained high till another critical value of the current was reached, when it fell back to the original low value. Thus as in the former experiments it is possible to find two values of the single electrode potential (and the surface tension) corresponding to the same current. In a further extension

of this work (Bowden and Rideal, *Proc. Roy. Soc.*, 1928, **120**, 59) measurements have been made of the charge per unit area that must be imparted to a mercury surface in the condition of high surface tension to raise the single electrode potential by 100 millivolts. These experiments were carried out in a cell carefully freed from oxygen. A remarkable feature of the results is that this rate of change of surface is only about one-third that calculated from an application of Lippmann's equation. It is not at all easy to account for this discrepancy, unless it be that the value given by Lippmann's equation corresponds with the condition of low surface tension, and that this condition is in some way (as yet unknown) favoured by the confined conditions within the capillary electrometer. Lippmann's equation has been directly verified only for a mercury electrode whose potential is in the neighbourhood of the reversible potential, *i.e.*, when the current is small and possibly entirely "adsorptive" (Frumkin, *Zeit. Phys. Chem.*, 1922, **103**, 56, and Schofield, *Phil. Mag.*, 1926, **1**, 641). These researches showed incidentally that Gibbs' adsorptive equation, of which Lippmann's equation is a special form, is not invalidated by the presence of electrical faces at the surface as is so often suggested.

The Critical State of Water.—Callender (*Proc. Roy. Soc.*, 1928, **120**, 461) announces a remarkable discovery. On heating carefully purified water in sealed quartz tubes, two layers of different density were observed as far as 380·5 per cent., although the meniscus vanished at 374° C. Curves connecting the densities of saturated steam and water and the temperature were obtained by using tubes containing varying quantities of water, and noting the temperature at which the meniscus reached the bottom or top of the tube. At 374° C. the steam and water occupy 3·79 and 2·28 cc./gm. respectively. Above 374° C. the surface of separation between liquid and vapour is no longer sharp, but when extreme pains are taken to exclude impurities, especially dissolved air, it is sufficiently evident to enable the density lines to be traced towards their meeting point at 380·5° C. Minute quantities of impurities mask the phenomenon by promoting ebullition and nuclear condensation, which doubtless accounts for the phenomenon's not being observed before. These observations have been confirmed by thermal measurements, which show that the latent heat is 72·4 cubs/gr. at 374° C., and only vanished at 380·5° C. The concordance between the two sets of results is such as to leave no doubt as to the reality of the phenomenon.

Whether or not a similar behaviour would be exhibited by other substances in the absence of impurities is a question that only further experiment can decide. There are very

strong grounds for believing that water molecules associate chemically in a way that many substances (*i.e.* simple hydrocarbons) do not. It is impossible, therefore, that this phenomenon is characteristic of an associating liquid. In any case it raises most interesting questions about the conditions necessary for the formation of a meniscus.

Porter (*Phil. Mag.*, 1929, 7, 624) makes some interesting observations in this connection. If the Laplace theory of capillarity be modified by integration between the molecular diameter and infinity, not from zero to infinity, and an inverse seventh power law be assumed, the surface tension can be expressed in the form

$$\sigma = f\left(\frac{\rho_2}{s_2} - \frac{\rho_1}{s_1}\right)$$

where ρ_2 and ρ_1 are the densities and s_2 and s_1 the molecular diameters of the liquid and vapour respectively. If $s_2 = s_1$, the surface tension vanishes when $\rho_2 = \rho_1$, as in the classical treatment. If, however, the molecular diameters in the two states differ, due to a difference in the degree of co-aggregation, the surface tension vanishes when the densities are in the same ratio as the molecular diameter. Thus on this view the molecular diameter of the water molecule is 1·6 times that of the steam molecule. It is not suggested, however, that this constitutes a complete solution of the problem.

Porter further suggests that the total surface energy, u, given by the well-known expression

$$u = \sigma - T\frac{d\sigma}{dT}$$

does not vanish till the latent heat vanishes. In this case $d\sigma/dT$ would have a finite value when the meniscus vanishes, so that σ would become negative before approaching zero asymptotically. Callender's experiments give no positive evidence for this, indeed there is no method available at the present time for measuring negative surface tensions.

The experiments were primarily undertaken in order to obtain data that would enable the steam tables to be extended into the critical regions. The results confirm the validity of Callender's well-known equation for the total heat (measured in work units).

$$H = \frac{13P}{3}(V - b) + bP + B$$

(b and B being constants), and show that the useful relations

that follow thermodynamically from this can be applied with confidence for dry steam up to 270 atmospheres. These results are certainly inconsistent with any equation of the van der Waals type using 374° C. as the critical temperature.

Application of the second law of thermodynamics to (1) shows that the equation of state may be written in the form

$$V - b = \frac{RT}{P} - c,$$

where $\frac{cP}{T}$ is a constant along an adiabatic. Interpreted on the co-aggregation theory of imperfection in gases, this means that the number of units taking part in the thermal bombardment of the walls of the vessel has been reduced by a small fraction, $\frac{cP}{RT}$, owing to the formation of molecular complexes. It is also a thermodynamic necessity from (1) that $P/T^{4\cdot3}$ shall be constant along an adiabatic so that $\frac{cP}{T}$ must be a function of $P/T^{4\cdot3}$. This condition is most simply satisfied if c is proportional to $1/T^{3\cdot3}$. Earlier measurement at lower pressures had shown that $c = 26{\cdot}3(373/T)^{3\cdot3}$ gives a good first approximation; it may be regarded as valid so long as the co-aggregation is mainly confined to the formation of double molecules. On the basis of the extended measurements Callender finds that the above expression for c must be divided by $1 - Z^2$, where $Z = 2{\cdot}23cP/T$, to allow for higher degrees of co-aggregation which become important at higher pressures. The constant b is found to have the surprisingly low value 0·175 /gm.

This result gives great weight to the view that the departure from the Boyle-Charles law is due, in the case of steam, to the formation of complex molecules. Caution should be exercised, however, in applying these results to non-associating substances. Recent research tends to show that the cohesive forces in water are of distinctly different nature from those in a liquid hydrocarbon.

The undirected type of cohesion exhibited by non-associating liquids is no doubt shown by associating liquids as well, but is masked in the latter case by the much more powerful directed attraction of chemical (co-ordinated) combination in which an atom of one molecule is linked with one on another. Chemical combination causes associated liquids to have a much higher critical temperature than non-associated liquids of equal molecular weight, and may possibly profoundly modify the manifestations in the critical regions.

GEOLOGY. By G. W. TYRRELL, A.R.C.Sc., Ph.D., University, Glasgow.

Regional and Stratigraphical Geology.—As usual, a large number of regional and stratigraphical memoirs have come to hand during the year. The more voluminous contributions to this branch of geological science can only be mentioned in this article, but it will be indicated where extended reviews of some of them can be found. J. Jung's important work, " Contribution à la géologie des Vosges hercyniennes d'Alsace " (*Mém. Serv. Carte géol. d'Alsace et de Lorraine*, 1928, 481 pp.), is reviewed in the *Amer. Journ. Sci.*, xvii, 1929, p. 92, and in *Geol. Mag.*, lxv, 1928, p. 377. The late Dr. Walcott's last work on the Early Palæozoic faunas and strata of the Canadian Rocky Mountains (Pre-Devonian Palæozoic Formations of the Cordilleran Provinces of Canada, *Smithson. Miscell. Coll.* 75, 1928, pp. 175–368, Pls. 26–108) is reviewed in *Amer. Journ. Sci.*, xvi, 1928, pp. 547–8, and *Geol. Mag.*, lxvi, 1929, pp. 332–3; and W. H. Twenhofel's comprehensive " Geology of Anticosti Island " (*Geol. Surv. Canada*, Mem. 154, 1928, 481 pp.) is dealt with in an extended review by C. Schuchert (*Amer. Journ. Sci.*, xvii, 1929, pp. 93–6).

Two great mining regions in the western United States are described in recently published *Professional Papers* of the U.S. Geological Survey (" Geology and Ore Deposits of the Leadville Mining District, Colorado," *Prof. Paper* 148, 1927, 368 pp., " Geography, Geology, and Mineral Resources of Part of South-eastern Idaho," *Prof. Paper*, 152, 1927, 453 pp.).

The " Geology of Venezuela and Trinidad," by R. A. Liddle (J. P. MacGowan, Fort Worth, Texas, 1928, 552 pp.), is reviewed in this volume, p. 354. Another part of South America, Peru, has been comprehensively dealt with by Prof. G. Steinmann, the great German authority on Andean geology (" Geologie von Peru," Heidelberg, 1929, 448 pp.). This work is reviewed in *Amer. Journ. Sci.*, xvii, 1929, pp., 478–9. Dr. L. H. Ower has provided a useful summary of the geology of British Honduras (*Journ. Geol.*, xxxvi, 1928, pp. 494–509).

South Africa has been generously treated by geologists of late. In addition to Du Toit's great volume on " The Geology of South Africa " (see SCIENCE PROGRESS, April 1928, pp. 721–2), we now have the second volume of E. Krenkel's " Geologie Afrikas " (Berlin, 1928, pp. 463–1000) and the volume on the Union of South Africa in the *Handbuch der Regionalen Geologie*, written by A. W. Rogers, A. L. Hall, P. A. Wagner, and S. H. Haughton (Heidelberg, 1929, 232 pp.). The last-named work is reviewed in this volume, p. 355. We have, in addition, three memoirs on Rhodesian areas (A. M. Macgregor, " The Geology of the Country around the Lonely Mine, Bubi

District," *Southern Rhodesia, Geol. Surv.*, Bull. No. 11, 1928, 96 pp.; F. E. Keep, "The Geology of the Shabani Mineral Belt, Belingwe District," *ibid.*, Bull. No. 12, 1929, 193 pp.; R. Murray-Hughes, "Geology of Part of North-western Rhodesia" (with Petrological Notes by A. A. Fitch), *Quart. Journ. Geol. Soc.*, lxxxv, 1929, pp. 109–66), and a useful summary of the geology of Tanganyika Territory by Dr. E. O. Teale (*Mining Mag.*, June, July, August, 1928, Reprint, 23 pp.).

Asia is represented by a monumental memoir on "The Geology of Mongolia" (*Amer. Mus. Nat. Hist.*, 1927, 475 pp.), reviewed in *Amer. Journ. Sci.*, xvi, 1928, pp. 175–7; the second volume of Prof. A. W. Grabau's "Stratigraphy of China" (*Geol. Surv., China*, 1928, 774 pp.), and Dr. G. M. Lees' memoir on "The Geology and Tectonics of Oman and of Parts of South-eastern Arabia" (*Quart. Journ. Geol. Soc.*, lxxxiv, 1928, pp. 585–670), in which he states that the geological facts with regard to the Oman folding lend strong support to Kober's view of a continuous orogen surrounding the African continent.

Dr. N. H. Kolderup has added one more to the great stratigraphical memoirs which have emanated from Norway in recent years ("Fjellbygningen i Kyststrøket mellem Nordfjord og Sognefjord," *Bergens Mus. Aarbok*, 1928. Naturvid. Række, No. 1, 222 pp.). As usual, he has provided an English summary of generous length (45 pp.), with two geological maps, and a series of sections. The foundation of the region is a great series of Pre-Cambrian gneisses injected in places with granite, and containing lenses of anorthosite, eclogite, amphibolite, gabbro, and other rocks of the Jotun-norite series of Goldschmidt. This latter series was formerly considered of Caledonian age, but Kolderup now considers that the balance of evidence is in favour of its Pre-Cambrian age. Cambro-Silurian schists occupy a considerable area, and are bent into the curious "arches" that Prof. C. F. Kolderup has made known from the Bergen area. The schists contain, as usual, "greenstone" lavas and intrusions, and are cut by the Caledonian trondhjemite-opdalite plutonics. Old Red Sandstone conglomerates and sandstones rest unconformably on this basement in six isolated areas.

The cutting of the new East Coast Railway of Southern Norrland (Sweden) has provided Dr. H. von Eckermann with unique opportunities for the study of the Early Archæan gneisses and associated granites of that part of Sweden, which are largely masked by glacial drift (*Geol. För. Förh. Stockholm*, 50, 1928, pp. 309–67). The fundamental country-rocks are the leptites and hälleflintgneisses mainly of sedimentary and volcanic origin, which are intruded by several large granite

masses. The petrology of the leptites, gneisses, and granites is illustrated by twenty-six new analyses.

The active Norwegian Spitsbergen Investigation Department, under its Director, Dr. A. Hoel, has now published a comprehensive memoir on "The Geology of Bear Island," by G. Horn and A. K. Orvin (*Norges Svalbard- og Ishavs-Undersøkelser. Skrift. Svalbard og Ishavet*, No. 15, 1928, 152 pp.). Bear Island lies in the Arctic Ocean between Spitsbergen and the North Cape of Norway, and shows the continuation of the Caledonian fold lines between Norway and Spitsbergen. The Hecla Hoek, consisting of dolomites, limestones, slates, and quartzites, is the oldest formation, and from fossil evidence is of Middle Ordovician age. On it there follow unconformably Upper Devonian strata containing what are probably the oldest workable coal-seams in the world. Then come the Kulm (Lower Carboniferous) with a thin coal, Middle and Upper Carboniferous sandstones and limestones, and, finally, a series of Triassic rocks. Tectonic disturbances of three ages, Silurian (Caledonian), Upper Carboniferous (two strong unconformities), and Tertiary (upheaval and tilting), are present.

In his Presidential Address to the Geological Society of South Africa, Dr. A. L. du Toit again dealt with the geological relations of South Africa and South America (*Proc. Geol. Soc. South Africa*, 1928, pp. xix–xxxviii. See SCIENCE PROGRESS, Oct. 1928, p. 217). He points out the amazing geological parallelism between the opposed sides of the two continents. Furthermore, equivalent formations on the opposed shores of the Atlantic are shown to be closely similar in facies, whilst their remoter extensions within the continents tend to exhibit marked variations. This fact is regarded as an extremely strong argument for the displacement hypothesis. Further evidence for the former contiguity of South Africa and South America is found in the similarities between the diamond and manganese ore occurrences in the two continents.

The theme of Prof. J. W. Gregory's Presidential Address to the Geological Society (*Quart. Journ. Geol. Soc.*, lxxxv, 1929, *Proc.*, pp. lxviii–cxxii), "The Geological History of the Atlantic Ocean," naturally also leads to a discussion of the Continental Displacement Hypothesis. This closely-packed memoir does not lend itself easily to summary; but Prof. Gregory's view of the age and origin of the Atlantic is substantially that of E. Suess, namely, that it is due to the enlargement, by successive subsidences, of vast bays which projected north and south from Tethys, the great east-west ocean of Mesozoic times. "The Atlantic is the greatest of the trough-shaped features of the earth; and its sides are

irregular because they intersect many earlier structures and zones of weakness." Prof. Gregory notes the interesting fact that the matching sides of the Atlantic were remarked by Francis Bacon; but he decides strongly against the displacement hypothesis as advocated by Wegener and du Toit, on stratigraphical, zoological, and geodetic grounds, although he recognises the westward lag of the land-masses under some conditions, and ascribes the formation of the Andes to the westward pressure of South America.

A most valuable review of the Late Palæozoic formations and faunas, with special reference to the ice-age of Middle Permian time, has been published by Prof. C. Schuchert (*Bull. Amer. Geol. Soc.*, **39**, 1928, pp. 769–886), in which he defends his well-known view that the glaciation of Gondwanaland took place not earlier than the Middle Permian.

Prof. Schuchert has also given a most useful short account of the geological history of the Antillean region in an address in Section E (Geology) of the American Association for the Advancement of Science (*Science*, lxix, 1929, pp. 139–45), which he promises to expand in a later publication.

In his memoir on "The Geology of Loch Lomond," Prof. J. W. Gregory (*Trans. Geol. Soc. Glasgow*, xviii, pt. 2, 1928, pp. 301–23) describes the Old Red Sandstone, the Highland Border Series (Ordovician), the metamorphic rocks north of the Highland Boundary Fault, the glacial geology, and the structure of the Loch Lomond Basin. The latter is regarded as a tectonic depression due to the Pliocene uplift of Scotland, during which tension clefts fractured the country, and the floor of the loch sank along a main N. to S. fault attended by numerous parallel and secondary faults. A new stratigraphical horizon—the Lennoxian—is erected to include the Luss and Aberfoyle Slates with the associated quartzites and quartz-felspar conglomerates, and the Leny Slates and Grits. The Dalradian Loch Lomond Series to the north is much more metamorphic, containing true schists and albite-gneisses, and has apparently contributed material to the younger Lennoxian series.

Dr. D. A. Allan has mapped and described an area along the Highland Border in Perthshire east of the River Tay (*Trans. Roy. Soc. Edin.*, lvi, pt. 1, 1928, pp. 57–88). The rocks consist of Dalradian Schists, Highland Border Series, a persistent serpentine belt along the Highland Boundary Fault, and the Lower Old Red Sandstone. A new exposure of the Highland Border Series has been found in the River Prosen. The serpentine is certainly pre-Lower Old Red Sandstone, and appears to be a deep-seated sill intrusion. The Lintrathen Porphyry, hitherto regarded as an intrusion,

is now shown to be a lava flow which occupies a constant stratigraphical horizon in the Lower Old Red Sandstone of Perthshire, Forfarshire, and Kincardineshire. The Highland Boundary Fault is a steeply-inclined reversed fault heading N.W. The general structure of the Lower Old Red Sandstone near the fault is that of a monoclinal flexure, slightly overfolded from the N.W., and fractured along the more or less vertical limb of the fold.

Dr. C. A. Matley's study of the Pre-Cambrian complex and the associated rocks of south-western Lleyn (Carnarvonshire) (*Quart. Journ. Geol. Soc.*, lxxxiv, 1928, pp. 440–504) has resulted in the conclusion that the complex is a detached part of the Mona Complex of Anglesey. Not only are these rocks of Pre-Cambrian age, but their special structures and metamorphism were also impressed upon them before Cambrian times. Dr. E. Greenly is inclined to regard the Lleyn Fold as the lower uninverted limb of a recumbent fold belonging to a tectonic horizon higher than those now found in Anglesey. Many basic dykes, mostly of Palæozoic age, cut the complex. Arenig rocks at Aberdaron form a vaulted syncline overridden on the west and north by the Complex, and faulted against it on the east. The whole region has been strongly affected by Palæozoic thrust movements, and the great boundary thrust of the Complex over the Arenig and the Sarn Granite is probably to be measured in miles.

Prof. F. Heritsch describes the Ordovician and Silurian of the Carnic Alps (*Geol. Mag.*, lxvi, 1929, pp. 121–7), and gives a tabular comparison of the divisions with those of Bohemia, and with the graptolite zones established by Elles and Wood.

The geology of the district around Dinas Mawddwy (Merioneth) is described by Prof. W. J. Pugh (*Quart. Journ. Geol. Soc.*, lxxxiv, 1928, pp. 345–81). The rocks consist of mudstones, shales, and slates, with subordinate bands of grit and limestone, belonging to the Bala and Valentian Series. The region is located on the south-eastern flank of the Harlech Dome, and consequently the general strike is S.W. to N.E., but folds along N. to S. axes also occur, which are correlated with structures, such as the Teify Anticline, farther south. There is intense post-folding cleavage, the strike of which is parallel to the general strike.

The geology of the district around Meifod (Montgomeryshire), a region formerly worked over by Sedgwick and Salter, has been described by Mr. W. B. R. King (*Quart. Journ. Geol. Soc.*, lxxxiv, 1928, pp. 671–702). The rocks range from Caradocian to Salopian, and illustrate remarkably well the dependence of the lithological succession on the geographical conditions attending the deposition of the rocks.

According to Dr. G. H. Mitchell the Borrowdale Volcanic Series in Troutbeck, Kentmere, and the western part of Long Sleddale (Westmorland), consists of alternations of andesite and rhyolite lavas with tuffs, in which there are minor intrusions of rhyolite, quartz-porphyry, lamprophyre, andesite, and spilite (*Quart. Journ. Geol. Soc.*, lxxxv, 1929, pp. 9–44). The lavas show a marked thinning to the west or north-west, indicating a south-eastern provenance; but the bedded tuffs thicken to the north-west, indicating an origin for them in that direction. Flow brecciation is a constant feature of the lavas. The structure is determined by two series of folds. The earlier, of Pre-Bala age, trends N.N.E.; the later, which is Devonian, has an E.N.E. direction. Cleavage of Devonian age has strongly affected the rocks.

B. B. Bancroft appears to have demonstrated a Pre-Ashgillian unconformity in the Bala country (*Geol. Mag.*, lxv, 1928, pp. 484–93), and suggests that this unconformity is general, as it can be shown to occur in Pembrokeshire, Glyn Ceiriog, and the Southern Uplands of Scotland.

Prof. P. G. H. Boswell has been able to trace a cleavage-fan in the Silurian rocks of the Denbighshire Moors and the Clwydian range similar to that described by Dr. Howel Williams in the Snowdon region (*Proc. Liverpool Geol. Soc.*, xv, pt. 1, 1928, pp. 69–77). In the northern part of the area the dip of the cleavage planes is towards the S., and increases to verticality in that direction. In the southern area the dip increases to the N. Between the two areas there intervenes a belt two to four miles in width in which the cleavage planes are vertical, or dip alternately N. and S. at high angles. The origin of this structure is tentatively attributed to lateral pressure acting more or less perpendicularly to the bedding on the opposed margins of the Caledonian geosyncline.

In his important paper on "The Siluro-Devonian Junction in England," Dr. T. Robertson (*Geol. Mag.*, lxv, 1928, pp. 385–400) states that his recent work has led him to adopt the conclusion of Barrois, Stamp, and others, that the base of the Ludlow Bone Bed affords by far the best horizon to take as the Siluro-Devonian junction. It is approximately Murchison's upper limit for the Silurian, and it can be located with ease and accuracy. It indicates a strongly-marked change in fauna without stratigraphical discontinuity, and its selection eliminates the necessity for indefinite and perplexing "passage beds."

The Central Mendips, consisting mainly of Lower Carboniferous rocks, shows complex structures which have been described and elucidated by Dr. F. B. A. Welch (*Quart. Journ. Geol. Soc.*, lxxxv, 1929, pp. 45–76). In the first place, an

anticlinorium of Palæozoic rocks appears to have been formed, consisting of three main periclines with an American trend, and aligned *en échelon*. A large amount of faulting and overthrust testifies to the tectonic weakness of the region. There are two stable periclines (North Hill and Beacon Hill) with an unstable pericline (Pen Hill) between them, and the thrust-faulting has apparently been concentrated within the latter.

The Cattybrook cutting near Bristol, described by Dr. Stanley Smith and Prof. S. H. Reynolds (*Quart. Journ. Geol. Soc.* lxxxv, 1929, pp. 1–9), shows only the Dibunophyllum zone (D_2 and probably D_1) of the Carboniferous Limestone, the rest being cut out by the great Cattybrook Fault which brings in the Coal Measures. The Lower Carboniferous rocks, which are clearly of shallow-water origin, are extremely variable, and consist of coarse ferruginous oolites with pseudo-breccias, grits, and shales. The extraordinary structures seen in this exposure are well shown in the detailed sections which accompany the paper.

The Lower Carboniferous rocks of the Menaian region of Carnarvonshire are fully described by Dr. E. Greenly (*Quart. Journ. Geol. Soc.*, lxxxiv, 1928, pp. 382–439). The lithology has already been commented upon (Science Progress, April, 1929, p. 591). Nearly the whole series belongs to the Dibunophyllum zone, of which three sub-zones are present. Cherts come above the limestones, and the combined thickness is about 1,150 feet. The structure is that of a synclinal infold, truncated on the S.E. by a great boundary fault. The syncline is asymmetrical, the south-eastern limb being much steeper than the south-western. The series rests unconformably upon all older rocks, and there is rapid overlap in westerly and north-westerly directions.

An important paper on the stratigraphy and tectonics of the Alston Block has been published by F. E. Trotter and S. E. Hollingworth (*Geol. Mag.*, lxv, 1928, pp. 433–48). The region dealt with embraces the northern end of the Pennines, together with the low ground of the Tyne-Irthing gap to the N. It is bounded on the west by the Vale of Eden, and is structurally separated from adjacent geological units on the N. and W. by the Stublick and Pennine Faults respectively. The Stainmore depression bounds the block on the south, but the eastern boundary is concealed beneath a Mesozoic cover. The paper deals with the post-Carboniferous earth movements of the Alston Block and the adjoining area to the W. and N., and with the relations of these to the Whin Sill. There is evidence that the Alston Block existed in Devonian times, and it is shown that it acted as a unit during the deposition of the Carboniferous rocks, with a

relatively unstable geosynclinal trough to the N. During the Hercynian compression it was depressed relatively to a geanticline which formed on the site of the trough to the N. The strata outside the north-western margin of the block were compressed into a group of crescentic folds. The Whin Sill was then intruded, and later, the compression found relief in the formation of a series of tear faults. Important conclusions are arrived at as to the form and distribution of the Whin Sill, which seem to depend on the varying incidence of the pressure.

F. W. Anderson describes the Lower Carboniferous of the Skyreholme Anticline, that part of the Craven Lowlands south of the North Craven Fault (*Geol. Mag.*, lxv, 1928, pp. 518–27). The limestones, both in lithology and fauna, are more related to the Northern facies than either to the Culm or the Reef facies of the Craven Lowlands. Structurally the region consists of a series of anticlines *en échelon*, with the dominant N.W. to S.E. trend of the region, and much broken by faulting. The beds are overlain unconformably by the Millstone Grit, which oversteps the flanks of the anticline.

Miss E. Dix and Dr. A. E. Trueman describe the marine horizons in the Coal Measures of South Wales (*Geol. Mag.*, lxv, 1928, pp. 356–63); and Mr. S. G. Clift and Dr. Trueman deal with the non-marine lamellibranchs in the Coal Measures of Nottinghamshire and Derbyshire (*Quart. Journ. Geol. Soc.*, lxxxv, 1929, pp. 77–108).

New excavations in the northern part of the Lickey Hills, south of Birmingham, which have yielded much new information on the geology of this complex region, have been described by Prof. W. S. Boulton (*Geol. Mag.*, lxv, 1928, pp. 225–66). Fresh facts have been ascertained with regard to the Cambrian, Quartzite, Llandovery Sandstone, Coal Measures, Bunter Sandstone, the Birmingham Fault, and the overfoldings and thrusts of the district.

Similarly, through new borings and exposures in shafts and trenches, Prof. Boulton has been enabled to record much new information regarding the Red Rocks between the Carboniferous and Trias, and the Spirorbis Limestones of the Keele Beds, in the Birmingham District (*Geol. Mag.*, lxv, 1928, pp. 313–23).

The country around Kenilworth (Warwickshire), the geology of which is exhaustively described by Mr. F. W. Shotton (*Quart. Journ. Geol. Soc.*, lxxxv, 1929, pp. 167–222), contains the southern termination of the barren Upper Coal Measures (Corley or Enville Beds,—conglomerates, breccias, sandstones, and marls), which rest on the Productive Middle Coal Measures to the north, and pitch deeper continuously until they are

overlaid by the Keuper Sandstone. The glacial deposits of the district are also fully treated.

PLANT PHYSIOLOGY. By PROF. WALTER STILES, Sc.D., F.R.S. The University, Reading.

Respiration.—The process of respiration is a characteristic of all living plant cells, but it is, all the same, in some degree mysterious. That an attack on the problems presented by this function of plants is being made under the direction of Dr. F. F. Blackman in the Cambridge Botany School is a matter for special notice and the publication of some of the results of his investigations is greatly to be welcomed. These results are presented in the form of the first three papers of a series of "Analytic Studies in Plant Respiration" published in the *Proceedings of the Royal Society* for last October (Series B, vol. 103, pp. 412–523, 1928).

Much work has already been done in Cambridge on the respiration of evergreen leaves which remain in an approximately constant condition of maturity for a long time. It was thought that an investigation of the respiration of ripening fruit, which is in a condition of senescence, should be of importance, as it could then be discovered whether the same fundamental principles are evident in the two types of organ. Such an investigation was rendered possible by experimental work on the cold storage of apples at the Cambridge Low Temperature Station, and the contributions to be discussed here deal with the results obtained with specimens of Bramley's Seedling apples kept in storage at about 2·5° C. until removed for experimentation.

The method of investigation adopted was to remove from the cold store, at intervals over a period of eight months, single apples and determine the respiratory activity at 22° C. and the course followed by the respiratory activity in air and in other gas mixtures. In all 21 apples were so examined and determinations on each apple were carried on regularly over various periods of time up to sixteen days.

The first paper of the series, by F. F. Blackman and P. Parija ("The Respiration of a Population of Senescent Ripening Apples"), deals mainly with the course of the respiration of the apples in air. In most cases part of this course had to be found by interpolation as the apples were generally removed from air into other gas mixtures during part of the experiment. Various interesting conclusions could be drawn from the numerical results obtained. Firstly, it appeared that in what seemed to be a homogeneous apple population there were present certainly two, and possibly three, physiological classes of apple, which ripened at different rates. When this was

recognised it became possible to formulate, as the investigators express it, "the characteristic phases of the metabolic drift." The senescent period appears to be particularly characterised by what is termed a lowering of "the organisation-resistance" of the tissues. The respiratory activity of a tissue is determined not merely by the amount of reserve food material present, but also by the rate at which such reserve material can be converted into the actual substance used in the respiratory process, "the effective substrate of respiration." Some of the organisation of the protoplasm can be regarded as a resistance to the reaction producing the effective substrate from the reserves; such resistance might be due to the presence of protoplasmic membranes or to adsorption or combination of particular substances with protoplasmic constituents. With a lowering of this organisation-resistance the rate of production of effective substrate will be increased. Presumably in the case of the ripening apple the lowering of the organisation-resistance involves a lowering of the resistance to hydrolysis of polysaccharides, disaccharides, and glucosides, thus resulting in an increased rate of production of glucose and other monosaccharides, the effective substrate of respiration.

The experimental results of Blackman and Parija lead them to the conclusion that the observed respiration of apples is the result of two independent and opposed processes. One of these is the lowering of organisation-resistance, particularly as it affects hydrolysis, which leads to increased effective substrate and so increased respiratory activity. The other is starvation, which must set in where isolated organs are maintained without a supply of food, and which tends continually to lower the respiration rate.

The second paper of the series, by P. Parija ("The Respiration of Apples in Nitrogen and its Relation to Respiration in Air"), deals with the respiration of the apples in nitrogen. It was found that the respiration in this medium depended on the physiological class of apple. It has been mentioned that the investigation of the respiration of the apples in air brought out the fact that they belonged to two or three different classes which ripened at different rates. In the case of the slow-ripening apples transference to nitrogen results in a rise in the respiration rate at once, but after a few hours the respiration falls rapidly to a level of the respiration in air, continues at this level for a shorter or longer time, and then quite suddenly declines rapidly and regularly with time. With apples of the more rapidly maturing class the course of respiration in nitrogen is quite different. On transference to nitrogen the respiration of this class of apple rises immediately and very considerably, and then declines slowly and with fluctuations,

but maintains a respiration rate well above that given in air. With both classes of apple retransference to air after quite long exposures to nitrogen brings about a return to the normal rate of respiration in air, although this may take as long as two days, during which the rate of respiration may exhibit a certain amount of fluctuation, and may be exceptionally low.

From a comparison of the general course of respiration in air and nitrogen it is concluded that the difference between the two is brought about by something more than the difference between oxidation of sugar in the one case and the splitting of sugar in absence of oxygen in the other. It is further concluded that to understand the difference between the two sets of respiration phenomena it is necessary to inquire into the carbohydrate metabolism preceding respiration and which supplies the directly respirable material.

Such an inquiry forms the subject of the third paper of the series. This is by F. F. Blackman, and is entitled "Formulation of a Catalytic System for the Respiration of Apples and its Relation to Oxygen." A scheme is formulated which takes the form of a series of reactions, the products of one action being the reactants of the next action in the chain.

The first reactants in the scheme are the reserve carbohydrates and the first reaction is the hydrolysis of these reserves with formation of hexose sugars. These then constitute the reactants of the second action in the chain. This action is called activation and appears to consist of an isomeric change in the hexose molecule so that heterohexoses are produced with internal ring structure which is less stable. These then form the reactants for the third reaction. This is an enzyme action in which the enzyme complex known as zymase functions. Neuberg and his many collaborators have shown that in this action a number of intermediate products are formed, such as methyl glyoxal, lactic acid, pyruvic acid, and acetaldehyde. These various substances are regarded as the product of the third reaction of Blackman's scheme, spoken of as glycolysis, and constitute the reactants of the last stage of the process and the substrate for the process of respiration in the restricted sense. This last stage will vary and the products differ, according as oxygen is present or not. The whole scheme thus comprises four linked reactions: hydrolysis of carbohydrate reserves into hexoses, activation of the hexoses, glycolysis, and respiration in the narrow sense.

Having formulated this scheme, Blackman considers its working in relation to starvation and variations of oxygen supply, and shows that the actual results obtained with apples in air and nitrogen are plausibly explained by the system postulated. It is finally pointed out that the most funda-

mental departure in the considerations of respiration in this work is emphasis on the rate of glycolysis. This latter is regarded as the common measure of respiration under all conditions while the production of carbon dioxide gives an index of the magnitude of this glycolysis. Another important point is the view that not normal hexoses but some specialised derivative of them constitutes the actual substrate for glycolysis.

From his analysis Blackman is led to the view that during respiration in air not only are carbon dioxide and water produced but that also large amounts of some other substance are formed. What this substance is, and what its fate, are not clear. But it does not appear to accumulate, and it is thought probable that it is " built back into the stream of catabolites."

There can be no doubt that these analytic studies on respiration constitute one of the most thoughtful and important contributions to this subject that have been published for many years, and plant physiologists will look forward to further studies on the subject from the same source.

ZOOLOGY. By F. W. Rogers Brambell, B.A., Ph.D., D.Sc., King's College, London.

An unusually large number of papers dealing with various aspects of Zoology have appeared during the last few months. The task of selecting those for mention here is even more difficult, and the result more arbitrary, than usual. It seems best, under the circumstances, to limit ourselves to one or two fields in which a number of important advances have been made. One of the most obvious is Cytology, to which several valuable contributions have been made.

In view of the large amount of work carried out in recent years on the cytoplasmic inclusions of animal cells, it is remarkable and regrettable that so little has been accomplished in elucidating these structures, especially the Golgi apparatus and mitochondria, in plant cells. Our knowledge of cell organs in plants is almost entirely limited to the pioneer work of Guilliermond, of Bowen, and of a very few other cytologists. A recent paper by Patten, Scott, and Gatenby (*Q.J.M.S.*, vol. 72, pt. 3, 1928) on " The Cytoplasmic Inclusions of Certain Plant Cells " is therefore of considerable interest. These authors employed chrome-osmic and corrosive osmic methods on the roots and shoots of luguminous plants. They identify three categories of cytoplasmic structures: mitochondria, plastids, and osmiophil platelets. They agree with the previous workers on the identification of the mitochondria and are inclined to support the view of Guilliermond that the plastids, so characteristic of plant cells, are really modified mitochondria.

The osmiophil platelets, first described by Bowen, are remarkably similar to the Golgi bodies of many invertebrate animals. Gatenby and his collaborators and Bowen are agreed upon the identification of these platelets as the representatives of the Golgi bodies in plant cells. They find no evidence that the vacuoles found in the plant cell are in any way connected with the Golgi bodies, and deny that the vacuolar system of the plant cell, as maintained by Guilliermond, is homologous with the Golgi apparatus of some animal cells.

A number of cytologists in recent years have described a vacuole, or system of vacuoles, in a number of different kinds of animal cells. This vacuolar system characteristically stains *intra vitam* with neutral red and appears to be a more or less constant constituent of the animal cell. It is usually associated with, and is probably produced by, the Golgi apparatus. Parat, doubtless impressed by this close association, formulated the theory that the neutral red vacuoles of the living cell represented the Golgi apparatus of the specially fixed cell, and gave to them the name of the " vacuôme." This view, which gained some measure of acceptance at first, has been gradually losing ground in the light of exact investigation. Recently Hirschler, Monné, Voinov, Gatenby, and others have shown that this system of vacuoles may separate from the Golgi apparatus during spermatogenesis in a number of forms. This important observation appears to establish beyond doubt the separate identity of the Golgi apparatus and neutral red vacuoles of animal cells and is a grave objection to Parat's " vacuôme " theory. The relations of the Golgi bodies and neutral red vacuoles during spermatogenesis in Lepidoptera, Mollusca, and Mammalia are described and discussed in a recent paper by Gatenby (*Roy. Soc. Proc.*, B., vol. 104, 1929). The neutral red vacuoles may be situated inside or outside the archoplasm, which is surrounded by the Golgi bodies. Among the Mollusca the vacuoles are usually situated in the archoplasm. When this is the case the vacuoles usually form a scattered group near each aster during mitosis.

The neutral red vacuoles, in the Lepidoptera and Mammalia examined, lie in a group outside the archoplasm but close to it. During mitosis they form a spindle-shaped group near the chromosomes in the equatorial plate, but outside the area of the amphiaster. This group divides into two smaller groups, as the division progresses, which move to the neighbourhood of the centrosomes at telophase. Consequently each daughter cell receives a group of neutral red vacuoles, which lie near the re-formed Golgi apparatus. During the subsequent formation of the acrosome by the Golgi apparatus of the spermatid the group of neutral red vacuoles keeps near the

Golgi apparatus, but does not appear to participate in any way in the formation of the acrosome.

In another paper Gatenby and Wigoder (*Roy. Soc. Proc.*, B., vol. 104, 1929) have drawn attention to a new cell structure which takes a prominent part in the spermatogenesis of a large number of animals: molluscs, annelids, anthropods, and mammals, with flagellate spermatogenesis. This structure is in the form of one or more granules, the post-nuclear granules, in the spermatocytes and early spermatids. These granules are densely impregnated by Da Fano's cobalt-silver-nitrate method, and may appear in association with the Golgi apparatus. They soon separate from the Golgi apparatus and remain in a group in the cytoplasm until spermateleosis begins. They then become arranged close to the nuclear membrane, around the proximal centrosome, at the posterior end of the nucleus, and form a structure which serves to support the nucleus and to unite it firmly to the centrosome and flagellum. The centrosome in molluscs and insects passes through the middle of this post-nuclear plate into the hinder part of the nucleus. The post-nuclear body serves as a sort of supporting disc fitting the centrosome and flagellum into the nucleus. In some animals, notably the amphibians, the post-nuclear body is not in the form of a plate, but is either spherical or elongate. It forms in mammals a wine-glass-shaped structure into which the nucleus fits and with which the centrosome, with the flagellum attached, is in contact. It might be compared to the cup of an acorn, the acorn representing the nucleus and the stem the tail of the sperm.

The same authors (*Roy. Soc. Proc.*, B., vol. 104, 1929) have also carried out an investigation of the cytological effects of X-rays on the testes of guinea-pigs. They found that suitable doses of X-rays inhibited mitosis in spermatogonia and primary and secondary spermatocytes. Inability to undergo either the first or the second maturation division, owing to irradiation, does not necessarily result in the immediate death of the cell. Such spermatocytes, either primary or secondary, although unable to divide, may proceed directly with acrosome formation and the other preliminary phases of spermateleosis which are normally peculiar to the spermatids.

A recent paper by Harvey (*Trans. Roy. Soc. of Edinburgh*, vol. 56, pt. 1, No. 8, 1929) describes the formation of yolk in the oocytes of Carcinus. He finds a syncytium in the early stages of oogenesis from which the follicle cells and oocytes gradually separate. The mitochondria cannot be distinguished in this syncytium, but can be seen in the oocytes after they have separated from it. They are not plentiful, and are in the form

of a crowd of granules, at first near the nucleus, which moves towards the periphery of the oocyte and breaks up into two or three parts. Finally they become distributed throughout the cytoplasm but are more plentiful around the nucleus. They are more or less completely obscured in the larger oocytes by the yolk. The Golgi apparatus can be seen in the syncytial stage. One Golgi body is included in each oocyte as it is cut off from the syncytium. The Golgi bodies multiply within the oocytes and form a peripheral zone in each. The yolk begins to form in this zone and gradually drives the Golgi elements inwards to form a ring around the nucleus. Finally the Golgi bodies, like the mitochondria, are obscured by the abundant yolk. It is thought that plasmosomal material is extruded from the nucleus and is added to the droplets of yolk. The proteid yolk is said to arise in relation to the Golgi bodies, while the fatty yolk is said to arise in the cytoplasm entirely independently of the cytoplasmic constituents. This interpretation of Harvey's is contrary to the views of most cytologists, who describe the fatty yolk as arising, directly or indirectly, from either the mitochondria or the Golgi bodies.

Turning to another aspect of cytology, Goldschmidt and Katsuki (*Biol. Zentralblatt*, Bd. 48, Heft 11, 1928) have obtained some remarkable results regarding the origin of gynandromorphs in the silkworm moth (*Bombyx mori* L.). They found that true gynandromorphs arose comparatively frequently in their strains of silkworms. Genetical analyses of these strains showed that these gynandromorphs could only be understood on the assumption that they arose from eggs with two nuclei. The manner in which these binucleate eggs arose cytologically is the subject of the paper. They worked on one thousand eggs of a race known to produce gynandromorphs, and succeeded in elucidating this problem. The normal mature egg contains four nuclei which are arranged in a row perpendicular to the surface. The inner of these is the female pronucleus, next to it is the second polar nucleus, and the outer two are the daughter nuclei of the first polar nucleus which has undergone the second maturation division. These polar nuclei will be called, for convenience, 1, 2, and 3, counting from without inwards. During normal fertilisation the female pronucleus sinks into the deeper layers of the ooplasm towards the male pronuclei, of which one, two, or three may be present in the egg. Meanwhile Nos. 2 and 3 of the polar nuclei or else all three fuse together to form the " polar fusion nucleus " frequently found in insect eggs. Normally this fusion nucleus may remain in the polar cytoplasm of the egg for a long time, while the blastoderm is forming. Finally it fragments, sometimes undergoing first several mitoses.

In the eggs of the gynandromorphic race the female pronucleus and the second polar nucleus (No. 3) are equal in size, but the other two polar nuclei are smaller and are unequal in size. Polar nucleus No. 1 reaches the resting stage more slowly and is smaller than No. 2. No. 2 gradually becomes as large as No. 3, which it approaches and fuses with to form the fusion nucleus. The female pronucleus at the same time sinks deeper and approaches the nearest sperm nucleus, with which it fuses. The polar fusion-nuclei in four cases were observed to leave the polar cytoplasm and to behave in a similar manner to the zygote nuclei arising from the fusion of the male and female pronuclei. The polar fusion nucleus in each of these cases sank into the deeper layers of the ooplasm and became surrounded by an aster. They took up positions symmetrical with the zygote nuclei, and showed every sign of participating in the ensuing cleavage. The bi-nucleate eggs, therefore, provided all the requirements necessary for developing into gynandromorphs, since the zygote nucleus would give rise to one-half of the body and the polar fusion-nucleus to the other half in each case. The development of a gynandromorph from such an egg would depend on one of these nuclei, either zygote or polar fusion-nucleus containing *xy* sex-chromosomes and the other two *x*-chromosomes. The chromosomal constitution of these nuclei will depend, since the female is the heterogametic or *xy* sex, on whether the first or the second maturation division is reductional for the sex-chromosomes. It can be shown that if the first division is reductional half or all such eggs will be gynandromorphic, while if the second is reductional the proportion will rise to two out of three such eggs.

Horning (*Australian Jour. Exp. Biol. and Med. Sci.*, vol. 6, 1929) gives an interesting review of recent work on mitochondria. It is illustrated with some very fine photographs of mitochondria in various kinds of cells. The frequent accumulation of mitochondria in the cytoplasm around the nuclear membrane and close beneath the cell membrane is discussed. It is suggested that this may be a surface tension phenomenon. There is evidence that mitochondria are largely composed of phosphatides which contain fatty acid radicals. Such substances have the capacity of reducing surface tension and would consequently, according to the Gibbs-Thomson law, accumulate at the phase boundary of the cytoplasm. This explanation would account for the frequent aggregation of mitochondria around vacuoles in the cytoplasm as well as beneath the cell-membrane, and in the vicinity of the nucleo-cytoplasmic interface.

The tissue culture method furnishes a most valuable means

of attacking many fundamental problems of cytology, since it allows of the cultivation of living cells *in vitro* in a thin film suitable for examination under the microscope. One of the most fundamental problems upon which much light has been thrown in this way is that of cell growth. It is now a matter of general knowledge that the growth of a given tissue cultivated *in vitro* depends almost entirely on the composition of the medium in which it is cultivated. Thus the addition of extracts of neoplasms or of embryonic tissue greatly accelerates growth. This has been shown to be due to the presence in these extracts of certain growth-promoting substances which have not yet been isolated or definitely identified. Horning and Byrne (*Australian Jour. Exp. Biol. and Med. Sci.*, vol. 6, 1929) have studied the relative growth rate and cytological characters of cultures of a round cell sarcoma grown in chick plasma to which one of the following extracts was added: (*a*) testicular extract from a tumour-bearing mouse, (*b*) testicular extract from a normal healthy mouse, (*c*) tumour extract, (*d*) chicken embryonic extract. They found that the cultures with testicular extract from a tumour-bearing mouse grew more rapidly than those with a similar extract from a normal healthy mouse. Similarly cultures with embryonic extract displayed much less rapid growth than those in the tumour extract. These experiments appear to show that the growth of sarcoma cells cultivated *in vitro* is stimulated to a much greater extent by tumour extracts than by extracts from the tissues of healthy individuals.

AGRICULTURAL PHYSIOLOGY.

By John Hammond, M.A., School of Agriculture, Cambridge.

Milk Production. (*a*) *Mammary Development.*—While the first phase of the growth of the mammary gland has been shown quite definitely by several investigators to be under the control of the corpus luteum, the origin of the stimulus for the second (pregnancy) phase of growth has been attributed by different workers to the prolongation of the corpus luteum, the myometrial gland, or the fœtus and placenta. Recently, however, Parkes (*Proc. Roy. Soc.*, B. 104, 1929, p. 189) has been able to produce the full development of the mammary glands, in the absence of the fœtus, by injections of luteal stimulating extracts of anterior pituitary into the pseudo-pregnant rabbit; no fœtal factor is therefore required for complete development of the mammary glands—only prolonged luteal action. Corner and Allen (*Amer. Journ. of Physiol.*, 88, 1929, pp. 326, 340) have prepared an extract of the corpus luteum which is physiologically active, and not only induces the pseudo-pregnant changes, but also maintains the normal development of pregnancy

in spayed rabbits. Active extracts of the corpus luteum have also been isolated by Wiesner and Patel (*Nature*, March 23, 1929). Studies on the growth and development of the udder in the cow have been made by Turner (*Proc. Amer. Soc. Animal Prod.*, 1928) and Hammond (*Reproduction in the Cow*, Cambridge, 1927). The size and number of the alveoli of the cow's udder have been investigated by Tsingonatov (*Ann. State Inst. Exp. Agron.*, Leningrad, **5**, 1927, p. 119). Ivanova (*Zeit. f. Tierzücht u. Züchtungsbiol.*, **12**, 1928, p. 119) has determined the variation in the number of nipples in the cow, and finds that supernumerary teats are an inherited dominant character, while Nachtsheim (*Deut. Landw. Tierzücht*, No. 21, 1925) has also shown in pigs that males with large or small numbers of nipples raise or lower respectively the nipple number in the offspring of sows with which they are mated.

(*b*) *Age Changes.*—In the Report of the World's Dairy Congress, London, 1928, the problem of the correction of milk yield and butter fat yield for age and other factors was discussed in papers by Gowen, Sanders, Zwagerman, White and Drakeley, Tuff, and Roberts; these taken together form a summary of recent work in the practical application of this branch of statistical physiology. Statistical methods have enabled studies to be made on the changes which occur in the function of the gland not only during the course of one lactation (the lactation curve), but also in those which occur in the shape of these curves during the course of the animal's life. Sanders, in a comprehensive investigation into the factors which affect milk yield (*Jour. Agr. Sci.*, **17**, 1927, and **18**, 1928), finds that as age increases the level at which the lactation commences rises, but the increase in the total yield is not proportional to this rise, for with advancing age the curve becomes steeper (*i.e.* the persistency of lactation diminishes). He suggests that the former may be caused by increase in the area of the gland with age, and the latter by inadequate mammary nutrition with advancing age or alternatively, that, just as the cells of the mammary gland are rejuvenated at each pregnancy and become senile during the course of the lactation curve, so the increasing senility of the animal as a whole with age increases the rate at which this proceeds during the lactation curve. Brody (*Missouri Agr. Exp. Sta., Res. Bul.*, **105**, 1927) has also independently taken this latter view and concludes that the decline in milk yield with the advance in the period of lactation represents a species of senescence as truly as the decline with advancing years; he compares these with a wide range of biological curves for growth and senescence and concludes that their equations are essentially those of the monomolecular change of the physical chemist.

Gaines (*Jour. Gen. Phys.*, **10**, 1926, p. 27), however, questions this last conclusion and thinks that the rate of decrease of milk secretion with advance in lactation is dependent on factors of a nutritional nature. Turner (*Jour. Dairy Sci.*, **10**, 1927, p. 95) has also described the decline in the persistency of secretion during the lactation period as the cow reaches maturity. Methods for measuring persistency have been reviewed by Gaines (*Jour. Agr. Res.*, **34**, 1927, p. 373), and he takes the view (*Illinois Agr. Exp. Sta.*, Bull. 288, 1927) that persistency of yield is not so much related to age as it is to the initial yield. Roberts (*Jour. Agri. Sci.*, **16**, 1926) finds that, in comparing two breeds, the Shorthorn yield, which is higher for the first few weeks after calving, falls off more rapidly than the Welsh, which starts at a lower level. Kay and M'Candlish (*Jour. Agr. Sci.*, **19**, 1929, p. 342) conclude that the increase in production with maturity is associated more closely with high initial production than with persistency of production, and attribute this in part to the growth of the secretory tissue of the udder with age and to body growth in general, although part may also be due to an improvement in functional activity through use. Turner (*Jour. Dairy Sci.*, **12**, 1929, p. 60), from an investigation of Guernsey records, has shown that for an increase of 100 lbs. in live weight accompanying age there is an increase of 77 lbs. of butter-fat per year: however, when age is held constant there is only an increase of 20 lbs. of fat for an increase of 100 lbs. in live weight, so he concludes that about 25 per cent. of the total increase of fat secretion with age is due to the live weight of the animals concerned, whereas the other 75 per cent. would be attributed to the development of the udder by recurring pregnancies.

Sanders (*Jour. Agr. Sci.*, **18**, 1928) has shown that the effect of a short dry period before calving in reducing yield in the subsequent lactation is much more marked in the case of second calvers than in subsequent lactations: this he attributes to the growth factor operating more powerfully with young cows, although whether it is bodily or mammary growth he could obtain no evidence. M'Candlish (*Rpt. World's Dairy Congress*, London, 1928) finds that the best age at which to calve Ayrshire heifers for the first time is about 2½ years, although he suggests that there may be variability according to their growth and development.

(*c*) *Intervals between Calvings.*—Gaines (*Jour. Dairy Sci.*, **10**, 1927, p. 117) is of the opinion that a service period (days between calving and service) of 174 days, such as is found in the American Guernsey advanced Registry tests, is far too long for the most economical life-time production, but he could find no evidence that the high rate of milk secretion

interferes with the recurrence of conception in these records. The optimum calving interval has been calculated by Sanders (*Yorkshire Milk Rec. Soc., Handbook*, 1929) as 13 months, for the cow's average weekly production over her whole life will be lowered by longer intervals when she spends too much time on the tail end of a lactation curve, and reduction will also occur at shorter intervals due to the very short rests between milking periods which reduce the yield in the subsequent lactation. Tuff and Landmark (*Nordisk Jordbrugsforskning*, 1926, p. 89), in an investigation of the factors affecting yield, find that more milk is obtained after a dry period of over than after one under 40 days.

(*d*) *Milk Secretion.*—For some time it has been a debatable point as to whether at milking-time all the milk obtained is already present in the udder or whether the larger part is secreted during the milking process. Tgetgel (*Schweiz. Arch. f. Tierheilk.*, **68**, 1926), who measured the pressure in the udder, found that, after a gradual rise during the interval between milkings, there was a sudden increase of pressure when the udder was manipulated just previous to milking: Ziegler (*Schweiz. Arch. f. Tierheilk.*, 1927, p. 121) attributes this to the erectile tissue (Rubeli) or muscular action (Zietzschmann) in the udder. Gaines and Sanman (*Amer. Jour. Phys.*, **80**, 1927, p. 691), after milking a cow at 12-hour intervals and determining the weight and lactose content of the milk, finally killed the cow (unmilked) at the regular milking time and analysed the amputated udder for lactose. Rather more lactose was obtained in this way than had been obtained at previous milkings; they infer, therefore, that there is present in the udder at milking time an amount of milk in excess of the amount secured upon milking.

Gowen and Tobey (*Jour. Gen. Phys.*, **10**, 1927, p. 949; and **12**, 1928, p. 123) also arrived at much the same conclusions on a somewhat similar experimental basis, cows producing up to 30 lbs. at one milking having the lactose equivalent of all the milk in the udder at the time when milking begins; they also found that the udders of dry cows contained no lactose.

Johansson (*Ultuna Lantbruks. årsredogörelse*, 1927) could find within the breed no relation between body weight or measurements and butter-fat production in Swedish cattle, but found positive correlation when all the breeds were compared with one another. Kronacher, Böttger and Patow, (*Zeit. f. Tierzücht u. Züchtungsbiol.*, **12**, 1928) could also, within a breed, find no relation between milk production and the angle of the ribs, a difference in which had been found to exist between beef and milk breeds by Duerst (*Züchtungskunde*, **2**, 1927).

Investigations have also been made recently concerning the secretion of milk in pigs. Schmidt and Lauprecht (*Züchtungskunde*, 1, 1926, p. 50) find that the larger the number of young suckled the greater is the amount of milk produced, more especially after the time of the maximum yield which occurs in small litters at the third and in large litters at the fourth week; that on a ration low in protein very much less milk was given than on a high protein ration; and that, as with the cow, the total and initial yield of milk rose with age to the sixth lactation; they also investigated the composition of the milk. Ohligmacher, Rodewald, and Hempel (*Arb. Deut. Gesel. f. Zücht.*, H. 37, 1928) have confirmed and extended these results; while the latter also finds that the piglings suckling the thoracic glands usually grow faster than the others.

(*e*) *Composition.*—Porcher, in a book (*Le Lait au point de vue Colloïdal*, Lyon, 1929) dealing with research on the mechanism of the action of rennet, classifies milks into two types: (1) casein milks in which the casein is 75 per cent. or over of the total protein (cow, goat, sheep), and which can be used for cheese-making; and (2) albuminous milks in which the chief proteins are globulin and albumin, the casein being under 65 per cent. of the total proteins (woman, mare, ass, bitch), so that no curd suitable for cheese-making is obtained. His main interest is in the constitution of milk as distinguished from its composition, and he regards it as a collection of different systems dispersed in water; the albumin and particularly the globulin, by virtue of adsorption, act as protectors of the caseinate, and if in sufficient quantity prevent its coagulation by rennet. He also considers that globulin to which phosphoric acid is attached closely resembles casein. These conclusions are of interest in connection with the recent findings (see SCIENCE PROGRESS, No. 82, Oct. 1926, p. 244) that during the initiation of secretion in the mammary glands of the cow during pregnancy the globulin stage of secretion is passed through in the stage of early alveolar development, and they suggest a possible new line of work in the biochemistry of the evolution of milk. Muffet (*Diss. l'École Vét.*, Lyon, 1928), however, regards colostrum as a retention product of milk and not an evolutionary form.

In a paper dealing with methods of testing pasteurised milk Orla-Jensen (*Rpt. World's Dairy Congress*, London, 1928) shows that the cream-forming substance in milk, an agglutinin, follows the globulin fraction and that it is possible to improve the cream rising in heated milk by the addition of only 1 per cent. of colostrum, for on heating to 68° C. for five minutes the cream line is totally destroyed. As the agglutinin has a

low optimum temperature it explains the fact that the optimum temperature for cream separation is as low as 0° C. White and Drakeley (*Jour. Agr. Sci.*, 1927, pp. 118, 420), in an investigation of the influences of various factors on the yield and quality of milk, find that whereas the fat percentage increases slightly with age and then decreases the percentage of solids-not-fat decreases continuously; that the minimum fat percentage, and with smaller variations the solids-not-fat percentage, is reached about the thirtieth day of the lactation period, after which there is a steady increase; and that the shorter the interval between milkings the lower is the solids-not-fat percentage and the higher the fat percentage. Bartlett (*Jour. Agr. Sci.*, **19**, 1929, p. 36), who studied the yields and variations of milk and butter-fat produced at morning and evening milkings, finds that heavy-milking cows during the first two months of lactation yield a greater weight of fat at the evening than at the morning milkings, and in this respect may be compared with cows in their first lactation; he suggests that cows with a high udder pressure yield a lower weight of fat at the morning than at the evening milking. He also finds that the evening yields (after the short interval) attain higher maximum and minimum values than the morning yields, and suggests that this is caused by the morning milking showing less response to the milk secretion stimulus which occurs during May and June. Since in yield of fat similar and even more pronounced changes are found, he concludes that if cows are stimulated to secrete more milk the stimulation should be accompanied by a shortening of the long interval between milkings in order to obtain the full benefit and to avoid a depression in the fat percentage of the milk.

Hansen (*Rpt. World's Dairy Congress*, London, 1928) classifies the concentrated feeding-stuffs into four groups differing in their effect on the yield and composition of milk; the first, while increasing yield, tend to decrease the fat percentage correspondingly, so that the amount of fat remains the same, while the second (particularly palm cake containing a high fat percentage) with very little effect on the milk yield increase the percentage and total yield of fat. Nils Hansson (*Rpt. World's Dairy Congress*, 1928) has shown that the production food required by cows rises from 0·30 food units per kg. milk when the fat content of the latter amounts to 2·75 per cent. to 0·42 food-units when the fat content is 5·0 per cent. Møllgaard and Lund (131 *Ber., Forsøglab. Kgl. Vet. og Landbohojsk.*, Copenhagen, 1929), in an investigation on the nutrition of milk production, find that a negative energy balance can occur for a considerable time without any effect on the amount of, or fat percentage in, the milk, but that a

negative nitrogen balance very soon causes a considerable fall of milk production and fat percentage in the milk.

Ruminant Digestion.—The increase in motor traffic has caused the veterinary physiologist to pay more attention to the cow and the problems of rumination have lately been re-investigated. Wester (*Bijdrage tot de Verg. Phys. van het Digesticapparaat*, Diss. Utrecht, 1923), by means of a rumen fistular, has been able to feel with his hand, and record with instruments, the method of the passage of the food and the contractions occurring during rumination. While in the calf the œsophageal groove, by means of a reflex, forms a tube carrying the milk direct to the fourth stomach, with the growth and development of the first two stomachs this reflex is lost, and it can no longer close completely, so that fluid, and the food after rumination, enter the first and second stomachs just as it does after the first deglutition. Similar experiments have also been made on twenty cattle of different ages by Schalk and Amadon (*North Dakota Agr. Exp. Sta.*, Bull. **216**, 1928), who, among other interesting conclusions, find that light buoyant forage boli pass into the depths of the rumen, while the heavier grain boli are chiefly retained in the reticulum; that the majority of uncrushed kernels are not regurgitated, and pass through the alimentary tract in an intact state; that the œsophageal groove takes no part in the formation or regurgitation of the bolus; that an adequate supply of water is an absolute requisite for physiologic rumination; and that the act of rumination can be artificially initiated in most animals by drawing the finger-tips or forages over the mucosa in the field of sensitivity in the anterior dorsal rumen region. From registrations of pressure Bergman and Dukes (*Jour. Amer. Vet. Med. Ass.*, **69**, 1926, August) conclude that entrance of the food into the œsophagus on regurgitation is effected by an aspiratory act of the thorax without definite bolus formation. Studies on rumination by X-ray methods have been made by Czepa and Stigler (*Fortschr. d. Naturwiss. Forsch.*, *Neue Folge*, H. 6, 1929), and they also find that, while fluids in the young animal go directly to the fourth stomach, in the adult the liquids pass to the first two stomachs, and that the finely divided contents of the rumen and reticulum can pass into the omasum and abomasum direct without rumination. Gas was normally found in various parts of the alimentary canal. Woodman (*Jour. Agr. Sci.*, **17**, 1927, p. 333) has suggested that digestible cellulose is broken down and absorbed as sugars. In a bulletin prepared for farmers Crampton (*Macdonald Coll., Canada*, Bull. No. 3, 1928) has referred to many of the physiological properties of the common feeding-stuffs, and has pointed out the necessity for different methods

of preparation to suit the physiological differences in digestion of the various species of farm animals. In a summary of recent work on the physiology of ruminant digestion, Hammond (*Vet. Record*, **9**, 1929, p. 343) has pointed out the importance of, and the necessity for, speeding up the rate of digestion when high production is required.

METEOROLOGY. By E. V. Newnham, B.Sc., Meteorological Office, London.

Relationship between Auroral Zones and Variations of the Earth's Magnetic Field.—A paper on this subject by A. Rostad appears in *Geofys. Publ.* 5, No. 5, Oslo, 1928. It opens with a summary of the known facts about the occurrence of the aurora, and of auroral zones, and of theories relating to these phenomena. Cathode rays from the sun suffer deviation when passing into the earth's magnetic field. There is reason for supposing that in the case of a strongly magnetised sphere such rays would arrive in zones somewhat resembling those of maximum auroral frequency on the earth. Störmer has developed this possible explanation of the terrestrial zones, and has attempted to show that if the earth's magnetic field were identical with that due to a magnet placed at the centre of the earth, electrical particles will only reach the earth along narrow circular bands around the magnetic poles. He has recourse to the idea that variations in the strength of upper winds blowing from the Equator, together with variations in the electrical emission from the sun, account for the observed irregularities in the region of occurrence of the aurora. Support for this idea is obtained from a study of magnetic data on days when aurora has been observed in low latitudes, which has shown such days to be days of unusual magnetic disturbance.

The main part of the paper under review is occupied with an analysis of disturbances of the earth's magnetic field at Potsdam from the assumed undisturbed field in relation to the recorded appearances of the aurora at Drontheim and Oslo. Curves for the disturbance on days of aurora show a sharp maximum at 21 h., which corresponds very closely with the mean of the times of maximum auroral frequency at Oxford and Prague found by Angot. It was found further that the disturbance must exceed a certain minimum value before aurora becomes visible in low latitudes, this minimum increasing rapidly with the distance of the place from the zone of maximum frequency of aurora. There is evidence that different minimum magnetic disturbances correspond with different types of aurora. Difficulty in determining the true time of commencement of an auroral display in summer—and to some extent

even in spring and autumn—owing to daylight, is held to account for certain anomalies in the observed relationship between the magnetic disturbance and the time of first appearance of the aurora, but in not 1 out of 107 cases of aurora at Drontheim was there any disturbance of the earth's magnetic field.

Trades and Counter Trades at Samoa.—A welcome contribution to the study of the winds of the trade-wind belt has recently been published by the Department of Scientific and Industrial Research, New Zealand. The author is Andrew Thomson, Director of Apia Observatory, Western Samoa. The results of 380 pilot-balloon ascents made at Apia between May 1923 and April 1928 are summarised. The value of the work is greatly enhanced, firstly, by the fact that so little is known of the upper winds of the immense tract of ocean between Australia and South America—a region where the circulation of the atmosphere is so little interfered with by land, and, secondly, by the high average altitude obtained in these ascents, in nearly half of which the air currents were explored up to a height of at least 10 kilometres. The results obtained necessarily apply only to times of clear weather, and are therefore not quite representative of average conditions, though evidently more nearly so than would be a similar set of ascents in a more cloudy part of the world; moreover, at the observatory at Apia the surface winds are deflected to some extent, and reduced in speed, by mountains to the southward.

There is a long analysis in tabular form of the results obtained. The chief items of interest that emerge from these tables are:

(1) The trade winds have their maximum mean velocity of about six metres per second at a height of 250 metres, and decrease fairly steadily above that height.

(2) The counter-trades, a layer of westerly winds, have a maximum mean velocity of nearly 11 metres per second at a height of 11·5 kilometres, that is to say several kilometres below the probable mean height of the stratosphere thereabouts.

(3) The counter-trade not only has a higher average velocity, but shows a much wider range than does the trade-wind stratum below 5 kilometres, where less than 5 per cent. of the velocities exceeded 12 metres per second.

(4) There is a region of maximum variability of wind direction at 4·5 kilometres, and regions of marked steadiness from the surface to 3 kilometres, and again above 6 kilometres, especially between 10 and 12 kilometres.

(5) Considering the net transport of air in two-monthly

periods for layers 1 kilometre thick, this is found to be greatest in the bottom kilometre, where the westward motion has a component towards the Equator, a result that would of course not have been expected from the foregoing figures, and which is to be accounted for mainly by the relatively small density of the air in the more rapidly moving counter-trade strata.

(6) In the layers below 14 kilometres, 5·3 times as much air is transported towards the Equator during the year than away from it. The density of the air higher up is too small for any compensating return current to be likely, and this is presumably a region of local inflow towards the Equator.

(7) There is some evidence for approximate equality in the masses of air moving east and west above this region.

Long Range Rainfall Forecasting in Australia.—There is a paper on this subject by E. T. Quayle in the *Proceedings of the Royal Society of Victoria*, xli, pt. 2. It is interesting more from the possibility of economically important forecasts that is indicated than from any light that may be thrown upon the physical processes that cause rainfall in Australia. The forecasts are based upon a correlation found between two-monthly means of pressure at a single tropical station—Darwin—and the rainfall of the succeeding two months in Northern Victoria as indicated by ten representative stations. It is only for one season, namely spring, that the connection becomes notably high. The June to July Darwin pressure is correlated negatively with the rainfall in North Victoria during the following two months, with a coefficient of $-0{\cdot}79 \mp {\cdot}04$, and when pairs of values of these two quantities are plotted graphically it is seen that the relationship is a fairly linear one. The falling off of the coefficient a month before and after these periods is rapid, to $-0{\cdot}65$ and $-0{\cdot}52$ respectively, and for March–April Darwin pressure the correlation is barely significant. Fortunately the period of high-correlation is such that good two-month forecasts appear possible of that part of the annual rainfall which has almost a critical importance in the growth of cereals and of spring and summer pastures. A test was made of the frequency with which the sign of the departure of the rainfall for the two months from the mean could be predicted, and it was found to amount to 82 per cent. at the most favourable time. It appears highly significant that simultaneous values of pressure and rainfall do not show anything approaching the same degree of closeness of relationship.

It is to be hoped that the results obtained will be followed up so as to yield further advances. Correlation coefficients between meteorological elements so far separated in space and time rarely approach 0·8 in magnitude except when the number of observations upon which they are based are too

small, and in this case there are as many as forty-five pairs of observations.

The Distribution of Terrestrial Radiation.—Dr. G. C. Simpson gives, in a recently published memoir of the *Royal Meteorological Society* (No. 23, vol. iii), very interesting world maps showing the distribution of mean cloud amount, mean surface temperature, mean intensity of outgoing long-wave radiation, and mean intensity of the difference between the incoming solar radiation at the earth's surface and the above-mentioned outward radiation, for "10° squares," and for different seasons. The calculation of these means of outward and net inward radiation was made possible by employing a number of bold simplifying assumptions. Among these may be mentioned the assumption that the mean temperature of the stratosphere is a function of the latitude simply, and is constant throughout the year, and that the mean temperature of the upper surface of the clouds is everywhere, and at all times, 260°*a*. It is not stated whether the amount of error that might arise from such assumptions has been investigated, but the author is evidently satisfied that the general character of the results obtained would be the same were it possible to employ a more rigorous treatment.

For the outward radiation the most remarkable feature is the small range of variation over the earth in the course of the year, and the small differences between the values in different localities at a given season. The values found nearly all lie between 0·26 and 0·30 calórie per square centimetre per minute. The central equatorial region, where the highest surface temperatures and consequently the greatest outward radiation might have been expected, radiates only slightly more than do the polar regions. This fact is due to the greater cloudiness of the region near the Equator, which transfers the region of effective outward radiation to the stratosphere, and the stratosphere is coldest near the Equator. In regions like the Sahara, where the mean surface temperature is practically the same as it is near the Equator, but where the sky is generally clear, the earth's surface is more often the region of effective radiation, and it is in such regions that the radiation reaches its maximum value—about 0·36 calorie per square centimetre per minute, in July. A quantity at least equal in importance to the outward long wave radiation is the "net radiation," *i.e.* the difference between that part of the inward solar radiation that is not reflected from the earth and its atmosphere and the outward radiation. If due regard be given to the sign of the two quantities required when calculating it, this net radiation gives the rate of accumulation of heat due to radiation. In order to find the proportion of inward radiation that is not reflected it was necessary to assume a relationship between albedo and

cloud amount, and this was done by averaging the slightly different values found by Aldrich and Angström. The maps showing the distribution of the "net radiation" suggest that—

(1) In the summer of the northern hemisphere more heat is being received than is being lost, from the Equator up to latitude 60°, and similarly for the summer of the southern hemisphere.

(2) In the winter, on the other hand, increasing net loss is shown on passing from the tropics to latitude 70 in both hemispheres.

The writer then passes on to a consideration of the mean outgoing radiation and of the "net radiation" during individual months, and over the whole year, in each 10° zone of latitude. The outgoing radiation is found to show two slight maxima at about 20° or 30° on either side of the Equator, in each month. The net radiation, owing to the approximate constancy of the outward radiation for each zone, is necessarily governed mainly by the effective inward solar radiation (the portion of the solar radiation that is not reflected from the earth and its atmosphere, to which reference has already been made). In December the net radiation is positive, *i.e.* the inward radiation exceeds the outward, from the South Pole up to latitude 14° N., while in June it is positive from the North Pole down to latitude 11° S., except possibly in a narrow zone around 80° N. It is only in September and March that the net radiation is positive in equatorial and negative in both polar regions. There is a remarkable degree of similarity between the figures for the corresponding zones in each hemisphere, in spite of the great differences in the distribution of land and sea.

On coming to compare the total outward and inward radiation from the earth in different months, a very good agreement is found, and on the whole year the difference is only 2 per cent. As we may reasonably assume that the inward and outward radiations for the whole year are equal, the smallness of the difference suggests that the simplifying assumptions upon which the whole investigation rests are justified. The author is inclined to attribute the 2 per cent. difference to errors in calculating the albedo from the cloud amount, and gives revised values for the differences of inward and outward radiations in individual months by reducing the albedo by 2 per cent. throughout. The revised figures then show that there is a very close balance throughout the year, the radiation received being very slightly in excess of that emitted from October to March, and conversely from April to September.

OLOGY. By L. J. P. Gaskin, Librarian to the Royal Anthropological Institute, London.

Transactions of the Royal Society of South Africa, vol. xvii, part 4, 1929.—Mr. E. J. Wayland contributes a well-illustrated article on "A Pebble Industry in the Transvaal." The artefacts described in this article are correlated by the author with his previous discoveries in Ceylon, and with similar cultures in Kenya and Uganda, and bear a striking resemblance to some of the pre-palæolithic implements recently discovered in Suffolk.

Two stone age sites were found at Belfast, South Africa; the first yielded a large series of "pebble tools" which the author correlates with a culture known in Uganda as the *Kafuan*. The second yielded implements of typically Le Moustier form, called *Sangoan* in Uganda.

The Uganda sequence of stone age cultures embodies the *Kafuan*, a pebble-culture, the *Sangoan*, a Le Moustier culture, and the *Magosian*, a microlithic culture.

Mr. Wayland groups the pebble artefacts, which have all been made from a fairly close-grained quartzite, and are all similarly weathered and patinated, under six general headings: Hammer-stones, Cores, Flakes, Cutters, Scrapers, and Points, and describes each series in detail.

Mr. Wayland considers the Pebble Industry to be older than that of Le Moustier.

Antiquity, June 1929.—Mr. M. C. Burkitt contributes an article on the Rock-carvings in the Italian Alps.

The author considers that the carvings fall into three classes: (1) carvings of animals, (2) weapons and tools, (3) signs, patterns, screens, etc. He points out that the animals most commonly represented are almost entirely oxen, and that the weapon most commonly figured is the triangular dagger.

Mr. Burkitt thinks that the figures have a ritual origin; and suggests some kind of Agricultural Cult which would be in keeping with the wild natural surroundings and the inaccessibility of the district. In proof of this he asserts that there is no evidence which has survived of any extensive habitation of the district before the period of the carvings; that culturally all the carvings can be assigned to the Bronze Age culture of the Italian Lakes; that there is no necessity to correlate them in time with the industries of the Italian Lake dwellings, and that all the carvings are not of the same age; that a date of A.D. 300 (*i.e.* before Christianity penetrated the region) might be assigned for their production, and that they were drawn in the first instance during seasonal pilgrimages to Monte Bego by the inhabitants of the neighbouring valleys,

who were hoping to obtain from the occult powers of the mountain good weather for their herds and crops, and later blessing for their arms and weapons.

For a more intensive study of this interesting subject C. Bicknell's *A Guide to the Prehistoric Rock Engravings in the Italian Maritime Alps* should be consulted.

Anthropologie, Prague, vol. 7, 1929.—Dr. A. Stocky writes on the "Prehistoric Population of Bohemia." The culture of the Cord-ceramic ware had its geographical distribution in the river basin of the Saale, in Anhalt, Bohemia, and Moravia.

The two primary types of pottery are the amphora and the beaker. The amphora is a vessel of globular body which narrows towards the bottom; where the diameter of the body is longest, two handles or eye-holes are attached one on each side of the vessel. The beaker has a globular body with a high cylindrical neck and conical rim. Associated cultural objects are stone hammers and battle-axes, and necklaces, bracelets, and anklets of stringed dogs' teeth, wolves' teeth, and eye-teeth of stags.

Interment was by means of pit graves; there were no stone enclosures.

Dr. Stocky considers that the Cord-ceramic culture preceded and eventually merged with the Aunjetitz culture, the original Bronze Age civilisation of Central Europe.

Annals of Archæology and Anthropology, Liverpool, vol. 16, contains a preliminary report on the excavations at Armant by Mr. R. Mond and Mr. W. B. Emory.

Two Saitic burials of mothers of the Buchis bulls were found in brick-vaulted pit-tombs at the end of an underground Roman passage. East of this passage a gigantic quartzite sarcophagus was discovered. The north end of the sarcophagus had been broken open; the contents were blue faïence cylinder beads, amulets, large quantities of gold foil, bones of the animals and six earthenware pots, one of which bore two hieratic inscriptions from which the burial was identified. The article is well illustrated. Further excavation in the vicinity by the Egypt Exploration Society has led to the discovery of the lost Bucheum, the burial-place of the Buchis Bulls.

Revue anthropologique, April–June, 1929.—The Abbé Breuil writes on two Aurignacian engraved pebbles found in the L'Abri Labatut at Sergeac (Dordogne). On one is carved the figure of a horse turned to the right; an excellent representation, though the mane has short hair. On the back of the pebble is a second horse turned to the left and better drawn.

The second pebble has been broken and the drawings are not quite complete. On one side is the head of a mammoth, and on the back is an animal of bovine character.

M. Breuil concludes his article with a description of some Aurignacian flints found at the station.

Proceedings of the Somersetshire Archæological and Natural History Society, 1928, contains a long and well-illustrated account of the excavation of Gough's Cave, Cheddar, by Mr. R. F. Parry. This report was foreshadowed in SCIENCE PROGRESS, April 1929.

The article has been divided into several parts. Mr. Parry describes the history of the site; the cave has been explored in fairly recent times by Messrs. C. G. Seligman, F. G. Parsons, and H. N. Davies. It will be remembered that nothing was found which could be definitely assigned to the Neolithic and Bronze Ages; though there was plentiful evidence that the cave had been occupied in Palæolithic, Early Iron Age, and Romano-British times.

Miss Bate has identified the animal remains as belonging to the Pleistocene era.

Mr. J. A. Davies describes the flint implements; the industry is Aurignacian.

Mr. H. St. George Gray describes the antiquities other than flint. These include the *bâton de commandement*, to which Mr. Gray assigns a Magdalenian origin, the rod of ivory, of either Magdalenian or Aurignacian date, necklace ornaments and objects of bone. The pottery is of Romano-British and Iron Age date, and comparable with that of the Somerset Lake Villages and Wookey Hole. The Human Remains comprise the skulls of a young man and a child. Sir Arthur Keith gives it as his opinion that they are of late Palæolithic date.

Bulletin de la société préhistorique française, May 1929, contains an important article by M. Ed. Vignard on an Aurignacian site at Nag-Hamadi (Upper Egypt).

The author makes a detailed survey of the implements which he discovered on the site. These number some two thousand pieces, eight hundred of which were burins. M. Vignard claims that they are representative in every way of the European Aurignacian burin. The article is well illustrated.

Præhistorische Zeitschrift, vol. 19, pts. 3, 4, 1928.—Fräulein E. Baumgartel contributes an article on the Neolithic implements of North Africa, Egypt, and Palestine.

The author finds a close connection between the form of these implements and those of the later Palæolithic era.

Of particular interest are the *coups-de-poing* which Fräulein Baumgartel compares with those of the Campignian culture in Europe. There are excellent illustrations.

Eurasia Septentrionalis Antiqua, vol. 4, 1929.—V. J. Tolmatchov writes on the Manchurian Palæolithic era. The

author points out that research into the Palæolithic civilisation of Manchuria is still in its infancy. He explains the lack of animal remains as being due to the natives, who, when they find fossil bones of mammoth or rhinoceros, derive a good profit by selling them to the local pharmacists as "dragon bones." Nevertheless, in 1918–19, fossil bones of mammals of the Quaternary era were discovered at Dchalainor. Amongst these were the remains of a mammoth. Thus encouraged, trial excavations were started at Dchalainor, and further remains of mammoth, worked rhinoceros, and stag-horn bones were found, proving that man was contemporaneous with the early mammals.

In 1926 M. Guerasimov discovered a new Palæolithic site near the railway station of Malta east of Irkoutsk. Amongst the objects excavated was that of an effigy of a woman carved in relief on a mammoth tusk.

Herr V. Antoniewicz contributes an article on a Bronze Age horde found at Stublø near Mizocz in Poland.

The weapons found include two socketed celts; the ornaments, bronze armlets, armbands, and earrings. The author considers that the celts and the earrings are of early Bronze Age, South-Carpathian type, though he correlates the horde as a whole with the Aunjetitz culture, the original Bronze Age civilisation of Europe.

Journal of the Royal Society of Antiquaries of Ireland, vol. lviii, pt. W.2, 1928.—Mr. A. M. D'Evelyn describes a find of Bronze Age urns in County Antrim.

The urns were discovered in a sandpit at Ballymacilroy, near Ballymena, on the farm of Mr. John Wilson. They are of reddish clay, well burnt, and similar in size; the diameter at mouth 6 inches, height 5 inches, and circumference at rim 20 inches.

Each urn is of bowl form and profusely ornamented with parallel lines and curved indentations, and has a raised middle band, which is divided into four panels by four projections or knobs.

There is an illustration of the urns.

The Illustrated London News for July 6th contains the first number of a new series of articles on the treasures discovered in the annexe of Tutankhamen's tomb.

The articles described include the king's hat-box, a plain wooden case with simple incrusted ornaments of peculiar domestic interest; a magnificently carved ivory head-rest, a pair of small ebony and cedar travelling-boxes, and an alabaster boat steered by an achondroplasic dwarf (27 inches by 23 inches), probably used at banquets as a centre-piece for the royal table.

The illustrations are excellent.

Exhibitions: Ur.—An exhibition of the objects discovered at Ur during the last season was opened to the public at the British Museum on July 6th. The most interesting exhibit is undoubtedly the lyre decorated with the head of a bull in gold. This has been reconstructed so as to show the instrument as it originally was; only the silver on the cross-bar has suffered from burial underground for more than 5,000 years.

Other objects of interest include two silver lyres found with the gold lyre, one decorated with a cow's head and the other with the statue of a stag in the round; two statues of a "ram caught in the thicket"; one is exhibited as excavated, the other has been reconstructed. There is a whole case of gold ornaments from the great death-pit, and some fragments of pottery, flint, and obsidian implements representing an older civilisation contemporary with the Flood.

University College, British School of Archæology in Egypt.—An exhibition of the results of last season's work was on view from July 8th–28th.

A cemetery of the Hyksos dynasty was unearthed at Beth-Phelet, Tell Fara (Palestine).

Of particular interest among the many objects on view was a fine bronze dagger with ivory handle and curious chain attachment, and a bronze strainer dating from about 700 B.C. showing marked Oriental influence.

ARTICLES

EFFECT OF WATER AS A PROMOTER OF CHEMICAL REACTIONS

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The effects of water on chemical reactions were observed as far back as 1794 by Mrs. Fulhame in her *Essay on Combustion*. She noted that salts of silver and gold supported on a wet fabric were rapidly attacked by sulphuretted hydrogen, but when the fabric was dry the reaction was comparatively slow. She also recorded an observation of Bergmann's that phosphorus is oxidised very slowly if the air be dry. On these results she based her theory of combustion, that combination with oxygen always takes place by means of water, the oxidisable substance combining with the oxygen of the water and liberating hydrogen, which combined with the oxygen of the air. On her theory she predicted that sulphur, carbon, the metals, nitrogen, and hydrogen would not combine with oxygen if the air were perfectly dry. Her predictions in nearly every case have been remarkably verified.

Wanklyn [1] in 1869 found that sodium, magnesium, zinc and Dutch metal were untarnished in dry air over a period of months. Introduction of a trace of moisture caused immediate reaction.

Between 1880 and 1900 the subject was advanced rapidly on the experimental side—notably by Baker—and the following list summarises many of the reactions, including some more recent work, which are found to be retarded by intensive drying.

Na and other metals with chlorine.
K and Na with oxygen.
C, P, S and Te with oxygen.
$CaO + SO_2$. $CaO + SO_3$. $CaO + CO_2$.
$CaO + NH_4Cl$.
H_2S and metallic salts or oxides.
HCl and $CaCO_3$.

HCl and Na or Al.
NH_3 and HCl, and dissociation of NH_4Cl.
NH_3 and CO_2.
2 $CO + O_2$. [3] $CO + N_2O$. [3] $CO + O_2$.
$H_2 + Cl_2$.
$2H_2 + O_2$ [4]. $N_2O + H_2$ [5].
$2NO + O_2$ [6] (a doubtful case).
Dissociation of PCl_5 [7]. Hg_2Cl_2 [7] and N_2O_3 [8].
$C_2H_4 + Cl_2$ [9].
$C_2H_4 + Br_2$.

On the other hand, the following examples have been carefully studied and have been found not to be retarded by intensive drying :

B, As, Sb, Se and oxygen.
Oxidation of CS_2 and of C_2N_2 [10].
Oxidation of hydrocarbons [10].
Conversion of oxygen into ozone.
Dissociation of N_2O_5 [11].
Dissociation of hydriodic acid [12].

The result of these investigations was that it came to be considered by many chemists that water, or at all events some catalyst, is necessary before any reaction can take place, and if drying were sufficiently intense and the reactants sufficiently pure, all reactions might be stopped. Prof. Armstrong is prominent among supporters of this view and his theory is considered further on in this paper. The evidence is, however, convincing that water is entirely without effect on certain reactions, and that others can take place to some extent in its absence. Apart from the reactions in which Baker [2] and Bone [10] failed to bring about any retardation by intensive drying, we have very convincing data in the cases of

(i) $2\,HI \rightleftarrows I_2 + H_2$
(ii) Decomposition of N_2O_5.

In the first case, if we assume that before any two molecules can react on collision they must have a combined energy greater than a critical value, E, then E can be determined in two ways.

A. The total number of collisions is calculated from the data of molecular diameters and the mean velocity of the molecules ; and the number of effective collisions is determined from the rate of reaction. Assuming a Maxwellian distribution of velocities, the proportion with energy greater than a given value can be obtained, and equating this to the proportion of effective collisions, E can be calculated.

B. E can be determined directly from the temperature coefficient by means of the relation

$$\frac{d \log k}{dT} = \frac{E}{RT^2},$$

where k is the velocity coefficient and T the absolute temperature. The reader will find a full account of the theory and calculation in Hinshelwood's *Reactions in Gaseous Systems*.

In four well-established homogeneous, bimolecular reactions, viz:

(*a*) $2\ HI = I_2 + H_2$ [13].
(*b*) $2\ N_2O = 2\ N_2 + O_2$ [13].
(*c*) $2\ Cl_2O = 2\ Cl_2 + O_2$ [13].
(*d*) $2\ NO_2 = 2\ NO + O_2$ [14].

the agreement obtained by the two methods is satisfactory, and in none of these cases has water been shown to be a catalyst in the homogeneous reaction.

Further, in case (*a*) Kistiakowsky [11] has shown that the reaction is unaffected by very intense drying, and is bimolecular and homogeneous, over a great range of pressures, and further his results are entirely in agreement with those of Bodenstein. Now the value of E determined from the temperature coefficient is just accounted for if we assume that all collisions between HI molecules with combined energy greater than E are effective. But if water or any other catalyst were necessary for reaction the number of effective collisions would be enormously reduced. It therefore appears that, under the conditions of Bodenstein's and Kistiakowsky's work, the decomposition of hydriodic acid is a direct homogeneous reaction and does not depend on the presence of any catalyst.

The results with nitrogen pentoxide are equally convincing, and have the advantage that they do not depend in any way on the truth of a possibly controversial theory. Nitrogen pentoxide is strongly self-drying, probably as much so as phosphorus pentoxide. If water (or any other catalyst) were necessary, it would be unlikely that different investigators should obtain under varying conditions the same velocity constant. However, if we compare the values obtained by Daniells and Johnston with those of three other sets of investigators, viz. Hirst [16], White and Tolman [17], and Rice and Getz [11], we find very good agreement.

The experiments of Rice and Getz are very convincing, as they varied their conditions considerably to see if there was any catalytic effect. The results were:

	k
Ordinary runs	0·286
Filtered through asbestos	0·284
Gas passed through electric precipitator	0·278
P_2O_5 in reaction vessel	0·278
Prepared from $AgNO_3$ and Cl_2 instead of from HNO_3 and P_2O_5	0·291

Further, they showed that HNO_3 was not a catalyst, which makes it extremely unlikely that water can be even in the most minute traces. Again, the rate of reaction is the highest explainable by any hypothesis, and if water were necessary the number of effective collisions must be reduced many millions of times.

The majority of reactions in which water has been proved to have an effect are heterogeneous, and many are reactions between solids and gases. Water has, however, an accelerating effect in certain homogeneous reactions, namely :

(*a*) Certain explosions, *e.g.* $2\ CO + O_2 = 2\ CO_2$.
(*b*) $H_2 + Cl_2 = 2\ HCl$.

The data as regards the reaction $2\ NO + O_2 = 2\ NO_2$ are conflicting. In the reaction between ammonia and hydrochloric acid heterogeneity has not been established, and in the reaction between ammonia and carbon dioxide data as to homogeneity do not appear to be available. The large number of heterogeneous reactions retarded by intensive drying suggests that water, which we know to be very strongly adsorbed by glass, plays its rôle in an adsorbed film on the surface of the vessel, or of the reactant if solid. This would explain the extreme intensity with which drying has in most cases to be carried out, as it is known that glass holds a unimolecular layer of water extremely tenaciously. It will therefore be useful to discuss the positive evidence for this theory, and then to consider the ones that have been advanced to explain the homogeneous reactions mentioned above.

In the case of many of the solid-gas reactions the explanation is simple and not likely to be disputed, namely, that the layer of water on the surface causes solution and ionisation in the ordinary way. For example, in Mrs. Fulhame's experiments with sulphuretted hydrogen and metallic salts, we have presumably a simple ionic reaction. The same explanation is also probably correct in the case of HCl and the metals, and might be so in reactions between SO_2 and CuO or CaO. In the cases where hydrochloric acid is a reactant only the minutest trace of moisture should be necessary to cause reaction, as the gas is ionised by the minutest traces of moisture.

As regards heterogeneous gas reactions the most valuable work is probably that of Dr. R. G. W. Norrish [9] carried out in this laboratory and published in three papers, two in the *Journal of the Chemical Society* and one, a theoretical discussion, in the *Transactions of the Faraday Society*, 1926. The dark reactions between ethylene and chlorine or bromine take place entirely on the surface of the vessel. Norrish found that with ethylene and bromine the velocity was greatly affected by the nature of the surface, the following velocity constants being obtained:

Surface.	k.
Glass	0·0506
Stearic acid	0·0864
Cetyl alcohol	0·0266
Paraffin wax	0·0030

These results indicate that a polar surface catalyses the reaction, and it seemed probable that the effect of water was of this nature. The adsorbed molecules of ethylene and bromine would be deformed, actually polarised, by this polar surface and so activated and enabled to react.

The results recorded in the second paper were greatly in support of this view. Ethylene and chlorine were used—the latter being easier to use and usable at higher pressures than bromine. It was found that the reaction could be almost entirely suspended in a paraffined vessel. The velocity coefficients in the paraffined vessel were variable and there was a period of induction corresponding probably to the diffusion of the reactants through cracks in the surface. When all rubber connections and stoppers were eliminated the lowest velocity obtained was about 1/1200 of that for a glass surface of the same dimensions. Stearic acid had again a higher activity than glass, and ordinary wet glass was about 30 per cent. more active than when glass and reactants had been dried. In these experiments drying was nothing like as intensive as that described by Baker or Bone in their experiments. With a paraffined surface drying had no effect on the reaction velocity. The following is a summary of the results:

Reaction.	Glass (moderately dry).	Glass (wet).	Stearic acid.	Paraffin and rubber.	Paraffin optimum.
$C_2H_4 + Br_2$	1·0	—	1·7	0·06	—
$C_2H_4 + Cl_2$	1·0	1·3	1·06	0·076	0·0008

Addition of an adsorbable alcohol, viz. propyl alcohol, greatly augmented the activity of a waxed surface. In later (unpublished) experiments Norrish has tried the effect of intensive drying and finds that the velocity with intensely dry glass is only about 10 per cent. of that on an ordinary glass surface.

We now come to the homogeneous reactions which are accelerated by water. The reaction

$$2\,CO + O_2 = 2\,CO_2$$

has been very thoroughly investigated [18]. It may be either heterogeneous (low temperature) or homogeneous (explosion by the electric spark). The homogeneous reaction was studied by Dixon (1880), who dried carbon monoxide and oxygen thoroughly over phosphorus pentoxide and found it did not explode with a spark of moderate intensity. His theory was that the reaction proceeded in two stages as follows :

$$\text{(i)}\ CO + H_2O = CO_2 + H_2.$$
$$\text{(ii)}\ 2\,H_2 + O_2 = 2\,H_2O.$$

The second reaction was regarded as taking place rapidly as the hydrogen was nascent. It is interesting to note that this is a restatement of Mrs. Fulhame's theory of combustion.

Traube (1882) [19] and Mendelieff (1891) [20] suggested that the direct reaction did not take place on account of the rarity of termolecular collisions. Their theory was that the direct termolecular reaction was replaced by the bimolecular reactions.

$$CO + H_2O = CO_2 + H_2.$$
$$H_2 + O_2 = H_2O_2.$$
$$H_2O_2 + CO = CO_2 + H_2O.$$

Dixon [3], in opposition to this, showed that dry mixtures of carbon monoxide and nitrous oxide or ozone were as difficult to explode as dry carbon monoxide and oxygen. This cannot, however, be taken as evidence against the theory, as nitrous oxide and ozone will be directly decomposed at the temperature of the spark, and the direct bimolecular reactions will not be possible.

$$N_2O + CO = CO_2 + N_2.$$
$$O_3 + CO = CO_2 + O_2.$$

Bone and his co-workers have, in the last few years, examined the carbon monoxide-oxygen reaction very thoroughly [18]. They investigated the relation between the water content of the mixture and the intensity of spark required for ignition, and find that the intensity required increases regularly over the range 2 per cent. – 0·03 per cent. of water by volume. The intensity required at 0·03 per cent. (dried over calcium chloride) is about 30 times that at 2 per cent. (saturated at the ordinary temperature). Further, they have shown that the most intensely dry gases—dried for six months with periodical baking out of

the reaction vessel—still explode with a sufficiently powerful spark. They have shown that the percentage combustion produced by a single spark decreases with the water content and in intensely dry mixtures is greater the more powerful the spark. Also they have shown that at high pressures water becomes less and less important, until at 25 atm. with an intensely dry mixture we get easy explosion and practically complete combustion. Experiments on the rate of propagation of the flame showed that, at ordinary pressures, it was only about 1/10 as fast in intensely dry mixtures as under ordinary conditions.

It appears, therefore, that carbon monoxide may either combine directly with oxygen, or indirectly by means of the reaction

$$H_2O + CO = CO_2 + H_2$$

followed by the combustion of the hydrogen. At high pressures, at which termolecular collisions become common, direct action can readily proceed and water becomes unimportant. At ordinary pressures, on the other hand, the indirect oxidation greatly predominates. Bone considers that steam, besides acting chemically, as it undoubtedly does when present in sufficient concentration, may promote ionisation, even when present in much smaller quantities, by virtue of its high specific inductive capacity; or, possibly, also, by condensing on free electrons may hinder their combination with positive ions and so maintain the reactivity of the system.

Another view has recently been suggested by Garner and Johnson [21], who found that small quantities of water vapour depressed the infra-red radiation from the explosion. They suggest that the reduction in the intensity of the infra-red emission is due to collision of water molecules with freshly formed CO_2 molecules. In the absence of water, the CO_2 molecules give up their excess of internal energy as infra-red radiation, whilst in the presence of water they lose part or the whole of it by collision. In the latter case they suppose that the greater amount of energy thereby retained in the system causes an increase in the rate of reaction.

The surface reaction taking place at lower temperatures has also been studied by Bone and his co-workers. They observed the relation between speed of reaction and time of drying over P_2O_5 on various surfaces, including gold, silver, and porcelain. They found that the first effect is slightly to increase reactivity, presumably by removing a film of moisture, but that the ultimate effect is to diminish greatly or practically to stop the catalytic combustion. With gold at 240° and silver at 360° the ultimate effect was practically to stop combustion, and

on introduction of moisture the original reactivity was in time completely restored. With porcelain, however, at 500°, the introduction of moisture did not restore the reactivity.

The reaction between hydrogen and oxygen is one of peculiar interest. H. B. Baker [4] before 1902 had made a large number of experiments to find whether intensive drying retarded this reaction, without success, until a new method of preparing very pure electrolytic gas was introduced, and hard glass tubes were used. He recorded the following observations:

(i) When dry and wet tubes were heated in pairs in the Bunsen flame only the wet one exploded. In two tubes dried only for two days, water was actually seen to condense, slow reaction occurring without explosion.

(ii) A wire of pure silver could be fused (960°) in the gas without explosion taking place, and no contraction was observed when the tube was subsequently opened under mercury.

(iii) If sparks were passed through the gas it exploded, wet or dry. In one case only, with very small sparks, and moderately dried gases, explosion did not take place, but no experiments were recorded to show that the ordinary moist gas would explode under the same conditions. With intensely dry gases, the smallest spark that could be passed exploded the mixture.

In this paper Baker quoted Armstrong's theory to explain the facts, namely, that an electrolyte is necessary for reaction to take place, and that the water formed by the slow reaction in the dried tubes was too pure to conduct electricity and hence cause explosion. He interpreted this to mean that the explosion was due to ionisation and that the pure water formed in the reaction did not cause ionisation. Apart from theoretical objections, it may be noted that the gases were extremely pure before drying, and it is difficult to conceive what the electrolytic impurity was that was removed by drying with phosphorus pentoxide.

Dixon [3] and Edgar repeated the work, using the same materials and method for preparing electrolytic gas, and silver drawn from the same ingot. They confirmed Baker's results except in one particular, namely, that by the time the silver wire melted, all or almost all the hydrogen and oxygen had united by non-explosive combustion. If the silver wire was heated very rapidly, explosion took place before melting. Further, Dixon showed that sparks exploded the mixture, wet or dry, and that the explosion wave was equally well propagated in the dry mixture. We see immediately that the effect of water in this reaction is in no way comparable with its effect in the explosion of the mixture $2\ CO + O_2$. In that reaction a very powerful spark is required to explode the dry mixture,

and then the propagation of the explosion wave is much slower and combustion incomplete.

Hinshelwood [22] has done some valuable work on this reaction. He showed that in silica and porcelain vessels at about 520° the reaction is more rapid in a packed bulb, *i.e.* it was accelerated by the surface, but that at a somewhat higher temperature the reaction was quicker in an unpacked bulb. When the pressure was lower the surface-accelerating effect predominated up to higher temperatures. Further, the initial presence of steam catalysed the reaction, but so also did excess oxygen or an inert gas.

His theory was that the reaction begins on the surface, and as the temperature is raised a chain reaction becomes possible. The retarding effect of packing at higher temperatures was due to the breaking of reaction chains by the surface, the catalytic effect of steam and inert gases being due to the lengthening of these chains on account of the increased number of collisions. If the chains start on the surface, as seems probable, then above a temperature at which branching of chains becomes considerable, increasing the surface beyond a certain limit will on this theory cause a diminution in velocity. The theory entirely fits the fact that at low pressures the surface-accelerating effect predominated up to higher temperatures.

Reviewing this experimental work the following seems a possible explanation.[1] Under ordinary conditions the reaction starts on the surface, above a certain temperature reaction chains can be propagated, and finally the loss of heat by radiation, conduction, etc., is slower than the development of heat chemically, and the temperature rises until explosion occurs. Baking out and intense drying of certain normally catalytic surfaces (*e.g.* hard glass) in the absence of impurities cause a change, in which their catalytic activity is destroyed, and the activity is not at once regenerated when the first traces of water are formed by the slow reaction. We have a parallel case in the work of Bone on the surface combustion of carbon monoxide. In the case where porcelain was the catalyst, the original activity was not regained on the introduction of water. The purely homogeneous reaction initiated by the electric spark is not affected by the drying. Experiments like those of Bone [18] on the gradual reduction of catalytic activity of the surface during drying in the oxidation of carbon monoxide should give interesting results in this reaction over the range of temperatures employed in Hinshelwood's experiments. Bone and Andrew [23] have examined the rate of reaction at 525° in Jena borosilicate glass tubes and find that after four weeks' drying there is

[1] Rice suggested a surface theory for this reaction in Taylor's *Physical Chemistry*, p. 921 (Macmillan, 1924).

only 0–3 per cent. combustion in ten minutes as against 40–50 per cent. in the undried mixture.

The oxidation of nitric oxide has been the subject of many researches [6], but the results are very conflicting. Bodenstein [24] and his co-workers have shown that neither the direct nor the reverse reaction is affected by surfaces, and further Norrish [14] has shown that the reverse reaction agrees with the data required for a direct bimolecular reaction on the activation theory, as mentioned above in this paper. Thus it is extremely improbable that the reaction is affected by water at about 575°, the temperature for which Norrish made his calculations. On the other hand, Baker (1894) found that if he prepared nitric oxide from copper and nitric acid no effect could be obtained on drying, but when prepared from the compound of ferrous sulphate and nitric oxide apparently reaction could be prevented. No brown fumes were obtained on mixing the dried gases at the ordinary temperature, and no contraction on opening under mercury. Hasche found that whereas surfaces in general did not affect the reaction, covering the surface of the vessel with paraffin wax cuts down the reaction velocity by about 25 per cent., and that intensive drying causes a decrease in velocity of some 50 per cent. only. Briner pointed out that the reaction, which is exceptional in having a negative temperature coefficient, proceeds some sixty times faster at −180° than at ordinary temperatures. At this temperature the vapour pressure of water is vanishingly small, and it seems impossible therefore that at such temperatures it could catalyse a homogeneous reaction. Preparing his gases from sodium nitrite and sulphuric acid and purifying by distillation at a low temperature, he found that even after drying for periods of three months to a year, a time much longer than Baker had used, brown fumes were always obtained on mixing. On the other hand, J. W. Smith [6] claims that intensive drying not only stops reaction but actually shifts the equilibrium.

As regards the reaction between ammonia and hydrochloric acid, it is very difficult to prove whether or not the reaction is homogeneous, as it proceeds normally with immeasurable rapidity. Hinshelwood [25] suggested that it was probably a surface reaction and that water played its part in an adsorbed film. In Baker's original experiments pure ammonia and hydrochloric acid, dried for a week over phosphorus pentoxide, formed no cloud on mixing, and no contraction in volume on opening under dry mercury. Great difficulty was often experienced in repeating this experiment, most minute traces of moisture being sufficient to cause instantaneous reaction. The reaction is apparently either completely withheld or proceeds extremely rapidly, and no intermediate rate can be ob-

tained by partial drying. Hinshelwood [26] has found a similar case in the catalytic effect of traces of NO_2 on the combination of hydrogen and oxygen. A trace of NO_2 lowers the temperature of explosion some 200°, and if sufficient is present to cause any observable reaction explosion occurs. Examination of such cases of trace-catalysis will probably throw light on this subject.

Hinshelwood's theory is that the trace-catalyst forms a centre from which reaction chains are initiated sufficiently violently to cause immediate inflammation. In the NH_3, HCl reaction it seems probable that the reaction centres produced by water are ions, since the HCl molecule is un-ionised in the pure state, but in the presence of water is almost completely ionised, and further, a minute trace of moisture will produce ionisation in the gaseous phase. The ionisation theory of the action of water was originally due to Sir J. J. Thomson [27] and has been supported by Baker [2, 4], Bone [28], and Dhar [29].

We now come to the hydrogen-chlorine reaction. Baker [2] found that with gases dried for four days over phosphorus pentoxide, more than a quarter of the mixture remained uncombined after exposure to two days' diffused and two days' bright sunlight. Bodenstein and Dux [30] showed that the velocity of reaction was independent of the concentration of water within wide limits. This was confirmed by Coehn and Jung [31], who attempted to determine the relation between concentration of water vapour and the rate of reaction, and found that the velocity rose from practically zero at 10^{-7} mm. to its full value of about 10^{-5} mm., above which value the velocity was independent of the pressure of water vapour. Their method was to dry out the vessel and then let in various quantities of water vapour, assuming that the water vapour would be in the homogeneous phase. Their theory was that water actually took part in the reaction chain, thus:

$$\text{Primary process } Cl_2 + h\nu \rightarrow Cl_2^1 \rightarrow 2\,Cl$$
$$\begin{cases} Cl + H_2O = HCl + OH \\ OH + H_2 = H_2O + H \end{cases}$$
$$H + Cl_2 = HCl + Cl \text{ and so on.}$$

This mechanism is, however, in conflict with their own and with Bodenstein's results, for if water is to take a part in the chain in this way it would certainly have an effect on the reaction up to much higher concentrations. Consequently it seemed more likely to Weigert [32] that water takes part in a complex which is required to initiate the primary process before the chain sets in, and that the presence of water in such a complex enables a smaller quantum to start the reaction than would otherwise be the case. Dahr [29] suggests that the

reaction is due to the formation of ions, and advances the view that ions generated in the primary reaction are absorbed by the inactive molecules, which are thus activated even in the absence of light, and hence accounts for the high quantum yield.

Cathala [33], on the other hand, suggested that the function of the water molecule was to induce greater chemical reactivity within its sphere of influence, which was assumed far greater than the normal radius of the molecule. When the sum of the volumes of the spheres of influence exceeds that of the vessel, he considered that water should have no further accelerating effect.

Norrish [34] pointed out that if Coehn and Jung's vessel had been completely dry before the introduction of water-vapour, under the conditions of their experiment the water introduced would be largely adsorbed on the walls of the vessel and showed that at their supposed value of 10^{-5} mm., at which water showed its full effect, the surface would be just covered with a unimolecular film and that there would be practically no water-vapour in the homogeneous phase. It was, therefore, indicated that water played its part on the surface. Norrish also pointed out a fallacy in Cathala's argument, and showed that his theory could not be reconciled with the experimental results.

Norrish's theory is, however, not in agreement with the work of Weigert and Kellermann [35]. They produced two sparks at a very small interval of time, the first initiating reaction and the second used for photographing it. In this way it was shown that combination takes place along the beam of light, and not from the surface. The reaction cannot, therefore, be entirely initiated at the surface.

G. E. Rollefson [1] in a very recent paper suggests that the chlorine molecule decomposes on absorption of light into a normal chlorine atom and an excited atom in the $^2P_1$ state, and that the chain is initiated by reaction between this and a water molecule. He points out that such an excited atom will be metastable and possessed of sufficient length of life to explain Coehn and Jung's observation that the rate of reaction does not fall off appreciably until the water vapour concentration is very low, and also the fact that the velocity coefficient does not fall off at low pressures of the reacting gases.

The catalytic effect of water has been observed in certain other photochemical reactions, such as the formation of sulphuryl chloride and carbonyl chloride. The reader will find an account of the subject in Berthoud's *Photochemie* (pp. 185–7) and in a paper by Tramm [36].

The work of Baker [37] and of Smits [38] on the change of

[1] *J.A.C.S.*, 1929, 770.

physical properties of solids and liquids on drying has of course an importance in connection with the subject we are considering. Smits [39] has shown that the change in boiling point, vapour density, and surface tension of certain liquids after prolonged drying observed by Baker does not prove that any shift of the inner equilibrium has taken place during drying, but merely that water catalyses the change between two forms of the substance. He has shown that the measurement of vapour pressure at the temperature at which drying takes place is the only true test, and from his experiments he has concluded that the equilibrium between two forms of sulphur trioxide is shifted on drying.

It may be pointed out here that a shift in the equilibrium would be impossible unless the work done by the chemical reaction in such a shift were less than the work done in removing the traces of water present. Thus we should not expect to find a measurable shift in the equilibrium of a chemical reaction, except in those which are attended only by a very small free energy change over a considerable range. The allotropic change of sulphur trioxide is apparently a reaction of this nature.

The possibility, in certain reactions retarded by drying, that one of the reacting species has been changed from a normal active form to an allotropic inactive modification must therefore be considered. Clearly such an explanation will be of very limited applicability, but might conceivably apply to some reactions of sulphur trioxide and other substances in which such a shift of inner equilibrium may take place. It has been suggested, as an explanation of the effect of intensive drying on the dissociation of ammonium chloride, which was supposed to be converted from the normal dissociable form to an undissociable form.

We have seen that there is very strong evidence in certain reactions for Norrish's theory of the surface action of water, but that it cannot be a general theory applicable to all reactions catalysed by water. Further experiments along the lines of Norrish's on ethylene and chlorine would show how far the theory is applicable.

Baker's [1] theory that the effect of water is due to its ionising power has been supported by Bone and Dahr and is probably correct in many cases, such as the combination of ammonia and hydrochloric acid, and almost certain in the case of many solid-gas reactions involving ionisable reactants. Sir J. J. Thomson (1892) showed that intensive drying increased the difficulty of passing a discharge through air and hence presumably reduced ionisation. This was confirmed by Baker (1894). Baker's theory is that water condenses on ions and electrons and prevents their recombination, and further causes ionisation of

[1] Originally suggested by Sir J. J. Thomson.

the reactants on account of its high specific inductive capacity; these ionic nuclei then act as reaction centres. Another way of considering the question is that the reaction centres are clusters consisting, for example, in the ammonia-hydrochloric acid reaction of the H_3^+O (hydrated hydrogen) ion and the $\overset{-}{Cl}$ ion, round which a layer of ammonia molecules would be held [29]. Very strong evidence for the existence of such ion clusters has been obtained by Lind in his work on the chemical effects of α-particles, and this would account for the extremely small trace of water required in some cases to cause rapid reaction.

As evidence for his view that ions are the reaction centres, Baker, in his presidential address to the Chemical Society in 1928, described some experiments in which mixtures of hydrogen and nitrous oxide, dried to contain only a few mgs of water in a million litres, were heated in a resistance furnace. He put in the reaction vessels powdered glass, lime, thoria, and radium bromide which produce ionisation increasing in this order. He found that with lime the rate of reaction was five times the rate with powdered glass, and that with thoria twenty times, and with $RaBr_2$ there was an explosion and the tube burst. These results indicate that ionisation increases the reaction velocity, though not necessarily that the accelerating effect of water is due to the same cause.

Thus the ionisation theory and the surface theory are not necessarily antagonistic, and further research will probably show that in many reactions we have adsorbtion and deformation at the surface of the vessel, while in others the effect of water is probably to form or stabilise ionic reaction centres. In certain cases both effects may exist, and in others we probably have special mechanisms, such as definite chemical participation, as in the oxidation of carbon monoxide.

My thanks are due to Dr. E. K. Rideal for reading this paper and recommending it for publication.

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THE BIOCHEMISTRY OF PECTIN

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I. Introduction

In the field of plant biochemistry, it is remarkable that certain regions have been so little explored, whilst in other directions an enormous amount of work has been carried out. Cellulose, starch, the sugars, and, perhaps to a lesser degree, lignin, have repeatedly and continually attracted the investigator, both from the commercial and the purely academic points of view. There are, then, still many gaps in our knowledge of the chemistry of plant materials ; and the nature and formation of several substances which are almost equally widely distributed as those just mentioned, are as yet unknown. For example, very little precise information exists as to the composition or genesis of cutin, suberin, gums, hemicelluloses, and mucilages. Until comparatively recently, the same might have been said of the pectic substances. These are constituents of the cell-wall of plants, being particularly plentiful in young tissues, and in many fruits and fleshy roots. Of recent years, however, some very considerable advances have been made in this field, and these it is proposed briefly to review here.

II. The Constitution of Pectin

Early Work.—Much of the earlier work on the nature and occurrence of the pectic substances was carried out by botanists, and, as a consequence, the botanical aspect was rather stressed,

while the purely chemical side of the subject was neglected until more recent years.

Jelly-like cell-wall substances were mentioned by these early botanists, but the first record of the preparation of a pectic substance is to be found in a paper by Payen (1824). In 1825, Braconnot recognised that the gelatinous substance isolated by him from vegetable tissues, and in particular from fruits, was acidic in character, and named it "pectic acid." Guibourt (1825) prepared the same substance from currant juice, and considered it to be related to the gums. Mulder (1838) described the presence in various fruit juices of a soluble substance related to pectic acid, to which he later gave the name "pectin." He stated that this pectin differed from the pectic acid of Braconnot only by its content of inorganic substances, and regarded it as being calcium pectate. He also suggested that pectin arises by degradation of some unknown substance incrusting the cell-wall.

Analytical methods were then applied by a number of workers, all of whom showed that pectin differs from the true carbohydrates in the O/H ratio.

Payen (1846) expressed the view that pectic acid exists ready formed in the cell-walls of the plant. Later (1856) he amplified this, and stated that the pectic substances exist in the cell-wall as calcium pectate, and this is found chiefly in the region of the middle lamella.

A considerable advance from the chemical point of view was made by the work of Frémy (1848, 1859.1.2) which extended over many years. He described a whole series of pectic substances and their degradation products. The insoluble parent substance occurring in the cell-wall, intimately associated with cellulose, he designated as "pectose." This insoluble pectose he stated to be converted to soluble pectin by boiling with acids, or, progressively, by the action of the cell-sap during the ripening of fruits. He suggested that as ripening proceeds, pectin itself is decomposed into a series of pectic acids by the action of enzymes, giving finally a stable product, "metapectic acid." This acid he also obtained by prolonged treatment of the fruit with dilute organic acids. As calcium also appears to be liberated by this treatment, he stated that "pectose" is a calcium compound of pectin. This was denied by Weisner (1861), who was of the opinion that pectose is a definite compound of pectin with cellulose.

Scheibler (1868) was the first seriously to attack the problem of the constitution of the pectin molecule. He observed that Frémy's "metapectic acid" on prolonged acid treatment gave eventually an acid and a reducing sugar, the latter at first being termed "pectinose."

Later (1873) he identified it as arabinose, then considered to be a hexose. Herzfeld (1891) stated that pectin contained both hexose and pentose units, and he successfully demonstrated the presence of galactose and arabinose in the hydrolysis mixture.

Mangin (1891, 1892, 1893) attacked the problem both from the botanical and the chemical points of view. By employing differential staining methods for the first time, he concluded that pectin is in combination with cellulose in the cell-wall, and that the middle lamella, as Payen had stated, was composed of calcium pectate. He stated that pectose, pectin, pectic acid, and the so-called "metapectic acid," the final product of mild acid hydrolysis, are the only definite pectic substances. The other pectic acids described by Frémy he regarded as being intermediate mixtures.

Tromp de Haas and Tollens (1895), after many ultimate analyses of pectin from various sources, came to the conclusion that pectin is related to the carbohydrates, though not a true carbohydrate, as the percentage of oxygen was higher than anticipated. Tollens (1898) later accounted for the difference by supposing one or more carboxyl groups to be present.

For a considerable period the subject appears to have been neglected, since practically no work was published until interest revived again, some twelve years ago.

Recent Researches.—As it will be necessary in later portions to deal with other aspects of the recent work, only those conclusions leading up to the determination of the constitution of pectin will be given here.

In 1916, Schryver and Haynes published analyses of samples of pectin from many sources, and concluded that a single substance acidic in character was obtained from all. This they named "pectinogen," which substance on treatment with alkali gave what they termed "pectin," uniform in composition. (This substance is that now known as pectic acid.) Combustions gave results indicating the presence of seventeen carbon atoms. They gave as the empirical formula of this substance — $C_{17}H_{24}O_{16}$.

Further light was thrown on the actual composition of the pectin molecule by Saurez (1917) who announced the isolation of an isomer of glycuronic acid—galacturonic acid—in the hydrolysis mixture from lemon pectin.

The researches of Ehrlich (1917), mark a very definite advance in the study of the constitution of pectin. His nomenclature is not very clear, so that his evidence is a little difficult to follow in places, but his conclusions are of the utmost importance. Crude pectin, he states, consists of two substances, a lævo-rotatory araban, giving on hydrolysis

l-arabinose (dextro-rotatory), and a strongly dextro-rotatory substance, the calcium salt of pectic acid. ıis last substance is a weak acid, yielding on mild acid hydrolysis, a monobasic acid, galactose-galacturonic acid, $C_{12}H_{20}O_{12}$. It has not been possible to repeat this, since no conditions are quoted, apart from saying that 1 per cent. oxalic acid is employed. Complete acid hydrolysis, by boiling for two or three hours with 1 per cent. hydrochloric acid, goes further, yielding galactose, and *d*. galacturonic acid, as mentioned by Saurez. Strong alkaline hydrolysis in the cold is stated to give an open chain tetra-galacturonic acid, its basicity being deduced from alkali titrations. The existence of this substance is doubtful.

Ehrlich's final conclusion is that pectin is a mixed calcium-magnesium salt of an acid containing galactose, arabinose, tetra-galacturonic acid, and methoxyl groups.

Von Fellenberg (1918), by analysis of pectin extracted by treatment of the tissue with superheated steam, confirmed the presence of arabinose, galactose, galacturonic acid, and methyl alcohol, but stated that in addition methyl pentose is always present. He gave figures for furfural estimations corresponding to arabinose 35–46 per cent., and methyl pentose 6–10 per cent. He did not, however, recognise that the galacturonic acid yields furfural also. He put forward as the formula of pectin $C_{78}H_{120}O_{68}$, in which by elimination of water are linked up, one unit of methyl pentose, two units of arabinose, one unit of galactose, and eight of galacturonic acid in the form of its methyl ester

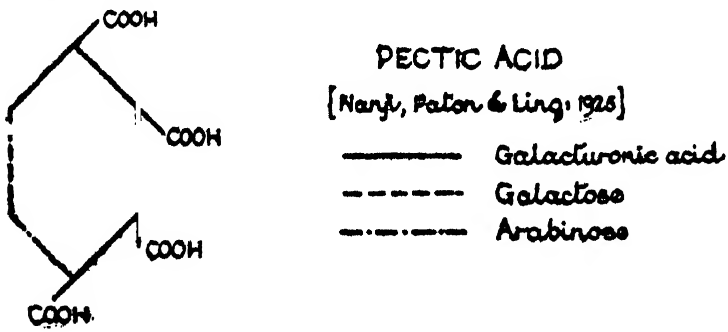
$$[(COOMe \cdot C_5H_7O_4)_8 \cdot (C_6H_{10}O_5) \cdot (C_6H_{10}O_5) \cdot (C_5H_8O_4)_2 + 2H_2O]$$

He also stated that by mild alkaline treatment, or less readily by acid treatment, the eight methoxyl groups might progressively be removed, giving several intermediate acids which he termed "pectinic acids." The various intermediate acids described by Frémy are members of this group, and are thus merely partly de-methoxylated octa-methoxy-pectic acid.

Clayson, Norris, and Schryver (1921) prepared samples of pectin from many sources. They state that methyl pentose is absent from the molecule. Smoleński, Komornicka, and Stypiński (1923) identify galacturonic acid in the hydrolysis liquid obtained by treatment of pectin with 1 per cent. sulphuric acid under pressure, and point out that this acid may form the link in the conversion of hexose to pentose. They confirm Ehrlich in the existence of a lævo-rotatory araban, soluble in 75 per cent. alcohol. They do not consider arabinose to be present in the pectin molecule, ascribing its presence in hydrolysis mixtures to the degradation of galacturonic acid. For the basal unit of pectin, they suggest that galacturonic

acid is linked through its carboxyl groups with the methyl ester of galacto-galacturonic acid. Another formula, somewhat similar to this, was given by Ahmann and Hooker (1925), who propose as a nucleus for pectic acid a dibasic galacturonic-galactonic acid, and suggest that six of these units are linked together to give the stable molecule, since from evidence of titration they believe at least eleven carboxyl groups to be present.

Nanji, Paton, and Ling (1925) were able to overcome the difficulties met by other workers, by making use of a modification of the method of Lefevre and Tollens (1907) for the estimation of the uronic acids (glycuronic and galacturonic acids). In this way they were able to determine the actual amount of galacturonic acid in pectin, which is 70·56 per cent. calculated as the anhydride. The furfural yield they found to be a little under 20 per cent. It was known that galacturonic acid anhydride yields 16·66 per cent. of its weight of furfural, so that it was easy to see that the 70·56 per cent. present in pectin would itself yield 11·73 per cent. furfural, the difference from 20 per cent. being due to arabinose. This corresponds to anhydroarabinose 14·25 per cent., the remainder of the molecule being anhydrogalactose. They confirmed the fact shown by Clayson, Norris, and Schryver (1921), that methyl pentose is absent, rightly ascribing the small quantity of phloroglucide soluble in alcohol, usually obtained in furfural estimations, to ω-hydroxymethyl furfural obtained in small quantities from the galactan units. From these results they were able to deduce as the formula for pectin a six-membered ring, four units of which consist of galacturonic acid, one arabinose, and one galactose.



In 1925 also, Norris and Schryver produced much experimental evidence which supported the Nanji, Paton, and Ling formula, which these workers accepted.

In the following year there appeared two papers by Ehrlich, the first of which, with von Sommerfeld, dealt with the nature

of pectin from sugar beet. They describe it as containing four units of galacturonic acid, one of arabinose, and one of galactose, but state that in addition it contains two methoxyl and three acetyl groups. The greater portion of the second paper by Ehrlich and Schubert (1926) deals with the nature of pectin from the flax plant. Their final conclusion is that it consists of four molecules of galacturonic acid, one each of galactose, arabinose, and xylose, and two molecules of methyl alcohol, and two of acetic acid, linked by loss of ten molecules of water. These workers are alone amongst those of recent years in suggesting the presence of xylose, but the presence of acetyl groups has also been mentioned by Smoleński (1924). These two papers by Ehrlich contain the first suggestion that pectin from different sources may be different in composition, in other words that pectin as a term may only be applied to a type of substance of particular properties, and not to one specific substance of definite composition. It would seem that the homogeneity of pectin preparations from many sources was established by the work of Clayson, Norris, and Schryver (1921), and Norris and Schryver (1925). Contrary to the view of Ehrlich, Henderson (1928) has recently claimed that pectin from flax contains only galactose and galacturonic acid, and rejects the hexa-ring formula of Nanji, Paton, and Ling. He proposes in place of it a straight chain formula, galactose-galacturonic acid. Though he obtains arabinose from the acid hydrolysis of pectin, he ascribes its presence to the partial de-carboxylation of the galacturonic residues, or to adsorption on the original pectin preparation. The furfural yield of pectin calculated from the Nanji, Paton, and Ling formula is a little under 20 per cent., while on that proposed by Henderson it would be 14 per cent. This worker did not, however, state the furfural yield of his preparation. In view of the fact that Norris and Schryver (1925) obtained furfural yields of from 18–20 per cent. from pectin preparations from many plant materials, it seemed unlikely that flax should differ so markedly from that from other sources. However, in view of this conflict of opinions, and the obvious importance of the question, this form of pectin was carefully re-investigated by Norris (1929), who finds no reason for departing from the original hexa-ring formula put forward by Nanji, Paton, and Ling.

This formula was put forward on mathematical grounds by calculation from the yields of carbon dioxide and furfural, coupled with a knowledge of the ultimate products of hydrolysis. Although there is little doubt that the proportions of the various constituents, as given by them, are correct, no evidence has ever been given as to the arrangement in the ring

and the method of linkage. With regard to the latter point, they say that the linkages are "other than 1 : 6," thus leaving the carboxyl groups free. The phrase quoted is a little ambiguous, because though it is certain that group 1 does participate in the linkage, as pectin is a non-reducing body, it is equally certain that group 6 does not, since pectin is an acid body when de-esterified. In view of the amylene oxide ring theory for sugars, it is possible that the linkage may be 1 : 4, though, of course, this theory has not as yet been proved to hold good in the case of all polysaccharides. Hydrolysis might be expected to throw some light on this question, but in point of fact, such work is extremely difficult, for very complex mixtures of pectin with its degradation compounds are obtained, that prove almost impossible of separation. It remains for some new method of attack to be evolved, to clear up this question.

III. The Nature of the Pectic Substances in the Plant

It is recognised that the pectic substances are a universal constituent of young plant tissues, and as such they must be of considerable importance structurally. It would seem that even the youngest and thinnest cambium walls, and the primary cell-walls laid down in mitosis, are pectic in nature. In many plants on maturity the middle lamella of the cells changes to something more stable, which is not pectin. In mature timber the middle lamella is practically entirely lignified, while in the functional parts it still consists of pectin. Several workers have hinted at a subtle relationship between the appearance of lignin and the disappearance of pectin. Although this is an attractive theory, there is not as yet any direct evidence in support of it.

It is generally agreed that the pectic substances in the plant fall roughly into two groups, the "cell-wall substance" and the "middle lamella substance." The "cell-wall substances" are two, protopectin and free pectin (or pectinogen, as it has been termed by several workers).

Protopectin is a form insoluble in water, and found in close association with the other cell-wall constituents, notably cellulose. Tutin (1923) questioned the existence of this water-insoluble form, stating that the incomplete extraction obtained by the use of water as a solvent is due only to the mechanical difficulty of getting proper penetration to the tissues. Carré (1925) produced evidence conclusive for the presence of some pectic substance insoluble in water. This substance could be removed, she showed, by boiling with hydrochloric acid as dilute as N/75. For this substance she revives the old name

"pectose" in place of the term "protopectin."[1] Many of the earlier workers claimed that protopectin is merely pectin in combination with cellulose. Sucharipa (1925) states that he has shown the existence of a whole range of substances consisting of varying amounts of cellulose and pectin, but the evidence is a little open to criticism. Carré (1925) also supports the view that protopectin is pectin in combination with cellulose. However, it is impossible to see how any normal type of cellulosic linkage can be ruptured by acid treatment as mild as N/75 HCl, or 0·5 per cent. oxalic acid, by treatment with 0·5 per cent. ammonium oxalate, or even slowly by prolonged boiling with water, yet this insoluble substance protopectin can be removed by these means.

Both Kopaczewski (1925) and Nanji, Paton, and Ling (1925) noted that free pectin gives with a solution of an iron salt a gelatinous precipitate which is insoluble in water. It is but slowly dissolved on boiling, though easily soluble in dilute oxalic and hydrochloric acids. These observations caused the latter workers to suggest that pectin exists in a condition insoluble in water in the plant in loose combination with metallic ions, such as iron. Nanji and Norman (1928) suggest that this insoluble form is a polymer consisting of several molecules of the fully esterified tetra-methyl-pectic acid, associated with one or two partially de-esterified molecules, the free carboxyl groups of which are linked to the metallic ion. Ehrlich (1917) regarded it as being a mixed calcium-magnesium salt, but there is evidence from the ash of pectin preparations that it is a mixed calcium-iron salt.

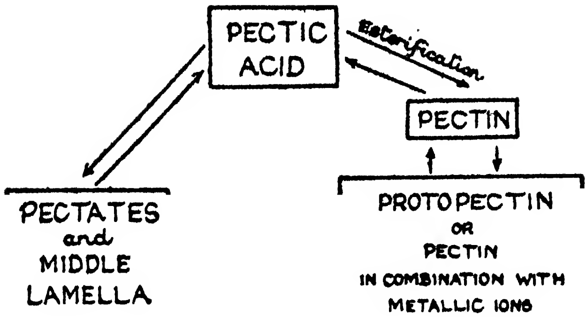
This insoluble modification, protopectin, gives rise to the soluble form pectin (or pectinogen), though it is not possible to be definite as to the type of change which takes place. It seems not unlikely that this may be merely depolymerisation. This change, the production of pectin from protopectin, occurs progressively during the ripening of fruits, as shown clearly by Carré, but it must not be supposed that free pectin is present only in ripe fruits. It is present to an appreciable extent whenever pectic substances are present, so possibly there is some subtle balance between the amounts of soluble and insoluble material present dependent upon the conditions obtaining in the cell. Further, it is clear that the soluble pectin does form a part of the cell-wall not only in young plants, but in more mature tissue, since it is usually but a small portion of the soluble form present that is actually in solution in the cell-sap. It is not easy to obtain precise information

[1] Protopectin is, however, the more correct term, implying as it does the "forerunner" of pectin. Further, the use of the term "pectose" is to be criticised in that it employs a suffix reserved for the simple sugars.

as to the condition of this substance *in situ*. Examination after extraction is apt to give results which are misleading, owing to the changes which may take place. Von Fellenberg (1918) assigned to pectin a formula containing eight carboxyl groups, and each of these he supposed to be methylated; in fact, he even suggested that pectin might be estimated in plants by the methyl alcohol yield on de-esterification with dilute alkali. Nanji, Paton, and Ling (1925) suppose four carboxyl groups in the molecule, each of which is methylated. Norris and Schryver (1925), on the other hand, suggest that but three of the four groups are esterified, whilst one is still free to react with a metallic ion in normal salt formation. This conclusion is based on the fact that these workers never found more than 9·2 per cent. methoxyl in any of the many samples of pectin prepared by them; the usual figure obtained was much below this. The theoretical percentage for tetra-methyl pectic acid is 11·76 per cent. and for the tri-methyl ester, 8·94 per cent. Norris (1926) studied the soluble pectin in the juice of the orange and stated that his product corresponded almost exactly with a tri-methoxylated derivative. The method of preparation used, however, possibly involved the loss of a portion of the methyl alcohol. Norman (1928) examined a preparation of free pectin from the juice of the lemon, great precautions being taken to avoid any de-esterification. An apparent methoxyl content was obtained approximating to a tri-methoxyl derivative. If, however, a correction for the calcium pectate yield was applied, this being the only absolute criterion of purity, then the corrected figure approached the theoretical for a fully esterified molecule. In view of this it is claimed that no evidence exists for stating, as some workers have done, that the soluble pectic substance is uniformly in the tri-methoxylated condition, but rather it must be regarded as being almost completely methylated.

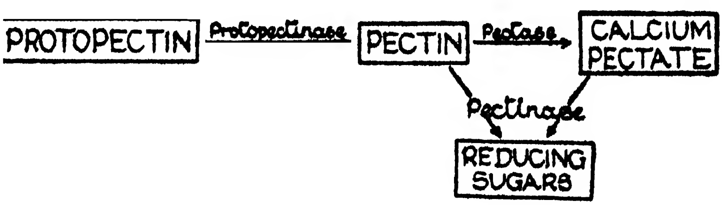
The pectic substance of the middle lamella is insoluble, and fairly generally agreed to be composed of combined pectic acid, probably the calcium salt, though certain workers have suggested that it also is in combination with cellulose. Its solubilities again render this unlikely. From a consideration of its quantitative distribution, it seems likely that combined pectic acid, *i.e.* pectates, occurs to a much greater extent in some tissues than can be accounted for by supposing that it forms the middle lamella only. This is particularly the case in the rind of the citrus fruits. It may be mentioned here that traces of free pectic acid have been found in some tissues.

The relationship of these substances may be made clearer in the following scheme.



The only work which has been carried out on the seasonal changes of these various constituents is that of Carré (1925) on the ripening and senescence of apples, in which the problem was studied in great detail and correlated by Carré and Horne (1927) with valuable histological work.

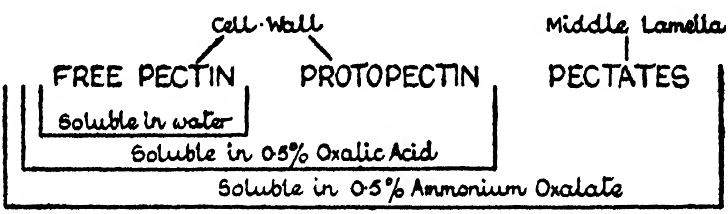
The mechanism of the pectic changes in the plant is very obscure. On lignification there is usually a complete disappearance of these substances, while in the ripening of fruits, such as the apple, the insoluble form protopectin is converted to free pectin, and later, if the fruit becomes overripe, there is a disappearance also of the pectate of the middle lamella. There is no doubt that enzymes are concerned in both these processes, yet information about them is very scanty. Three enzymes are believed to exist, and to be specific. Protopectin is converted to the soluble form by an enzyme "protopectinase" (or "propectinase"), which Carré (1925) states to have an optimum temperature of 45° C. Bertrand and Mallèvre (1894) described an enzyme, which they termed "pectase," giving a gelatinous precipitate in neutral pectin



solutions. They showed that this enzyme was extremely susceptible to acid conditions, and that the presence of a salt of one of the alkali metals was necessary for its activity. The jelly formed by it they proved to be the alkali salt of pectic acid. Bourquelot and Hérissey (1898–9) confirmed these results, and announced the existence in malt of another enzyme, which, unlike pectase, is capable of converting pectic acid

or pectin to reducing sugars. This they termed "pectinase." Practically no work has been carried out in recent years on these pectic enzymes, and a re-investigation in this direction might yield results which would throw much light on the metabolism of this substance, and also perhaps be of importance in connection with the ripening of stored fruits.

On the basis of the various solubilities of the different pectic substances, Nanji and Norman (1928) have proposed a scheme of estimation by differential extraction, which is conveniently summarised in the diagram below.



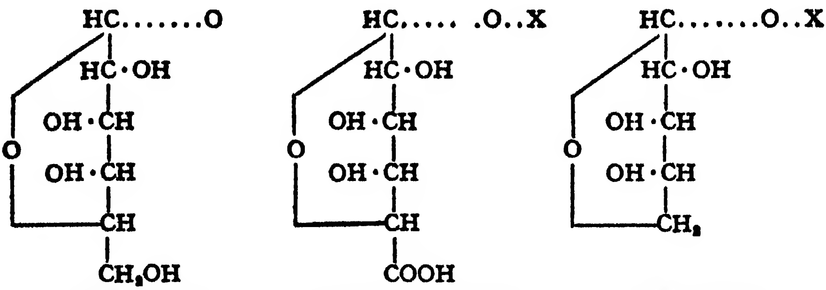
It will be seen that by difference each of the constituents can be separately obtained. The method of estimation employed by them is a modification of that suggested by Carré and Haynes (1922) and very similar to that given by Emmett and Carré (1926). Briefly, it consists of the precipitation of the pectin in the extract by acidified alcohol, re-solution in water and very dilute ammonia, and de-esterification by treatment with N/10 soda overnight. After acidification with acetic acid, calcium chloride is added and the pectate deposited as the insoluble calcium salt. The method is susceptible of considerable accuracy.

There is as yet no information as to the genesis of the pectic substances. It is very significant, however, that each of the units in pectin is similar in steric arrangement, and is directly derivable from galactose, if, as is usual in the plant, the carboxyl groups are shielded by linkage.

Protracted mild oxidation of the terminal carbinol group of this sugar gives rise to *d*. galacturonic acid, which in turn, on de-carboxylation (which may also be a process of mild oxidation), yields arabinose. It is not necessary, therefore, to postulate the formation of pentosans prior to the formation of pectin, for the only pentose unit in pectin, arabinose, is easily derivable in this way from galactose.

The formation of pectin may thus be due to the oxidation of linked galactan units. It is curious that galactose appears to occupy this important place in the transition to pectin, and perhaps also to hemicelluloses, for a continuation of the process would give substances of that type, whilst it is glucose which

is the most widespread unit of hexosans, *i.e.* starch and cellulose. The change from glucose to galactose, involving only the interchange of the hydroxyl and hydrogen groups on the γ-carbon atom, is one which must take place readily in plants, and, for that matter, in animals also, as evidenced by the production of lactose, on a galactose free diet. The explanation of this prominence of galactose is not obvious, unless it be that hexose in the form of galactose is less readily respired than as glucose.



d. Galactan unit. *d.* Galacturonic acid unit. *l.* Arabinose unit.

It is difficult to see how such a theory as this can be experimentally tested, and it is likely that its only support will remain in the indirect evidence just given.

IV. The Biological Decomposition of Pectin

Since the pectic substances form a universal constituent of the cell-walls of unlignified tissue, their biological decomposition is a subject of great interest to the plant pathologist. Our knowledge of these processes is, however, fragmentary. Misconceptions as to the nature and properties of pectin have hindered progress, but the recent advances made in this direction should pave the way for a more successful study of the biological aspect.

Apart from the plant pathological viewpoint, an understanding of the microbiological degradation of these substances is of commercial importance in connection with the senescence and rotting of stored fruit, potatoes, etc., the retting of fibre plants, such as flax and hemp, and the rotting of vegetable material in the manure-heap, and in the soil.

When a plant is attacked, whether by bacteria or fungi, the invasion of its tissue may take place in two ways, either along the middle lamella between the cells, or else by direct penetration of the cell-walls. The solution of the middle lamella is probably the most widespread action in plant diseases, although penetration by this route is sometimes aided by splitting

of the tissue owing to mass action caused by rapid growth of a bacterial colony, or ramification of a fungal mycelium, in a closed space.

It seems practically certain that in every case a specific enzyme is responsible for the solution of the middle lamella, though the evidence on this point is contradictory and confusing in the extreme. For example, Cooley (1914) maintains that the hyphæ of *Sclerotinea cinerea* in plum tissue are mainly intracellular, though he was unable to show the presence of any enzyme capable of dissolving the middle lamella, neither could he find any trace of softening in advance of the hyphæ. Valleau (1915) came to opposite conclusions in observing the same fungus. He described an enzyme, pectinase, which was responsible for the destruction of the middle lamella, though the softening in advance of the hyphæ, which he observed, he ascribed to the action of oxalic acid excreted from the hyphæ.

This same suggestion, that oxalic acid may be excreted and be responsible for the solution of the middle lamella, has been made also in the case of bacterial infections.

The work of Willamen (1920, 1922, 1925) on the growth of *Sclerotinea* on certain tissues is by far the most complete investigation of this question. He unhesitatingly concludes that a specific enzyme is responsible for dissolving out the middle lamella, which, however, he supposes to consist of protopectin. He advances the interesting suggestion that this dissolved pectin is re-precipitated in a different physical form as calcium pectate, which being a hydrophyllic gel is supposed to maintain the firmness of the fruit after rotting, a characteristic of fruits rotted by *Sclerotinea*. The gel is also presumed to be of use to the fungus in maintaining a supply of water.

Several workers have isolated enzymes from the expressed juices of roots, such as carrots, which have been attacked by very active soft-rot bacteria. They have shown that the dissolving power for calcium pectate which this juice possesses is destroyed on boiling, and in one or two cases tested the bacteria concerned in an artificial pectin medium, and found that it is slowly dissolved.

This question would repay further investigation, though it is likely that confirmation of the view that a specific enzyme is concerned would be obtained.

Turning now to industrial processes involving the microbiological decomposition of pectin, the most important case is the retting of fibre plants, such as flax and hemp, for the isolation of the fibres. By the solution of the middle lamella of the phloem and cortical tissue, the bundles become separated from the woody matter, which is removed later, after drying,

in the process of scutching. Eyre and Nodder (1924) have made a most detailed study of this operation of retting. The first stage in the process is a purely physical one, in which swelling of the straw takes place, and soluble materials come into solution. The biological stage which follows they divide into three phases. First, active fermentation of these soluble materials takes place aerobically, paving the way for anaerobic conditions in the tanks. In the second part, which is not entered upon till all the readily fermentable substances have been removed, the water-soluble pectin is fermented away. During the third, and principal part of the biological stage, the main fermentation of the pectic substances takes place, and rapid loosening of the fibre-bundles occurs. Finally if the ret is prolonged, the bundles are attacked, and their texture and tensile strength adversely affected. Many bacteria play a part in the retting process, the chief one being stated to be *Plectridium pectinovorum*, an anaerobic organism.

The decomposition of the pectic substances in the soil, and the probable production from them of simple carbohydrate material, is of some importance in relation to the nitrogen cycle, and possibly also to the formation of humus. There is some evidence that the galacturonic acid portion of the molecule is not easily destroyed by micro-organisms, but remains in the soil organic matter. In this connection it is significant that a uronic acid has recently been extracted from the soil humus by Jacks (1928). Further evidence has been obtained of a similar nature in the rotting of straw by a mixed soil micro-flora. It seems that during the normal period and conditions of rotting, pectin does not disappear, but rather tends to accumulate. This phenomenon appears to be connected with the reaction of the material, for if it becomes acid—an abnormal condition—the pectin is apparently easily fermented by organisms tolerant of such conditions.

As will be seen, the information in this direction is distinctly scanty, and there is considerable scope for further investigation, which might yield results of considerable practical importance.

V. Some Commercial Aspects

Soluble pectin possesses the property of giving a viscous solution when boiled with sugar and dilute acid under suitable conditions, which solution sets to a clear firm jelly on cooling. It is, in fact, the pectin present in the fruit which makes it possible to prepare jams, preserves, and jellies. The ordinary process of jam-making involves boiling to concentrate the pectin and fruit acid present, so that, with the added sugar, a satisfactory jelly will be obtained. Certain fruits have long

been recognised by housewives as giving a poor jam that does not set well, and this fault is due generally to a deficiency of pectin. To such fruits it has been customary to add apple pulp with the sole intention of increasing the pectin content, so that a good jam is obtained, setting firm and rapidly. A knowledge of the jellying properties of pectin, and the optimum conditions thereof, is of the greatest importance in the jam-making industry.

It has been shown many times that prolonged boiling or over-boiling of jams destroys the power of the pectin to give a jelly, with the result that the jam does not set. Experiments have shown that it is not possible to obtain a jelly with pectic acid and sugar, so the conclusion drawn is that the degree of esterification of the pectin is intimately connected with jelly formation. Sucharipa (1923) stated that a jelly was not given if fully methoxylated pectin was employed with sugar alone, but that on boiling it with acid a jelly could be obtained. Nanji and Norman (1926) showed that both fully esterified pectin (tetra-methyl pectic acid) and the tri-methyl acid gave good jellies with sugar and acid; if anything, that from the tri-methyl acid was slightly better. The jellying power, however, progressively dropped after the removal of more than one methoxyl group, so that the dimethyl ester gave a poor jelly, while the mono-methyl, like pectic acid, failed to give one at all. It would seem, therefore, that the jellying power of a sample of pectin is a function of its methoxyl content; the more complete the esterification the greater the jellying power, all other factors being constant. Tarr (1923) has shown that the hydrogen-ion concentration of the mixture is an important factor, the optimum being $p_H = 3{\cdot}46$.

It may be gathered that the addition of a small quantity of pectin would frequently much simplify the process of obtaining a good jam, since long boiling and the loss of flavour and discoloration which accompany such a process would be avoided. In America a considerable industry has arisen for the manufacture of pectin for this purpose. Even the housewives understand the process, for there is a large sale of pectin preparations for domestic use. There is no doubt that the addition of pectin to jams by jam-makers is carried out in this country also, but not to the same extent. There is a considerable prejudice against such a practice here, but it is difficult to see how the addition of pure pectin—a natural fruit product—is any more objectionable than the addition of sugar, while it is certainly less so than the custom of using low-grade fruit pulps, or vegetable pulps, as is done not infrequently.

The commercial pectin preparations fall into two groups, though there are a score or so of patents covering their produc-

tion. In the one case the aim is to produce a pectin "concentrate," that is a solution of pure pectin of strength from 3–4 per cent. usually, while the other is to obtain a pure dry powdered substance. The latter is perhaps preferable, since it is very difficult entirely to free the "concentrates" from the flavouring and colouring of the original material, even though a preliminary extraction with water be carried out. In general extraction is carried out with a hot dilute acid, after treatment with cold water. Some flocculant, such as casein, is employed to clear the extract. Starch may be removed by diastatic action, after which the extract is concentrated. In an ingenious method devised by Wilson (1925) resulting in an unusually pure product, the extracted pectin is precipitated by aluminium hydroxide, the precipitation being a colloidal phenomenon in which an electro -ve particle is precipitated by one of opposite charge.

The chief source for the production of pectin at present is apple pulp, or the pomace from cider factories, but a promising industry is being built up in the utilisation of surplus citrus fruits in this way.

In this country the largest potential source lies in the beet pulp from which the sugar has been extracted. Large quantities of this are available, and are at the moment being disposed of as cattle food. The dried pulp usually contains upwards of 10 per cent. of pectin. It seems that the removal of the pectin makes little difference to the value of the pulp as cattle food, so that the residue would still find a ready market. It appears not unlikely, therefore, that a pectin industry might arise in this country, if there was a very considerable extension of the demand for this material to assist jam and jelly making.

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RIVER TERRACES AND RAISED BEACHES: THE WORK OF THE COMMISSION ON PLIOCENE AND PLEISTOCENE TERRACES AT THE INTERNATIONAL GEOGRAPHICAL CONGRESS, 1928.

By B. R. M. SANER, B.A.

Part I

The first report of the "Commission on Pliocene and Pleistocene Terraces," edited by K. S. Sandford, was presented at the International Geographical Congress at Cambridge in 1928. The Commission was appointed after the meeting of the Congress in Cairo, in 1925, to investigate the following problem: "The Study of the Coastal Terraces and River Terraces in order to determine the existant of constant levels and to fix their succession especially on the Western European coasts and in the Mediterranean Basin."

The first work of such a Commission was bound to be simply the collection and correlation of facts; only when a considerable body of detailed evidence has been accumulated can any conclusions be reached as to the world-wide distribution of constant levels.

In order to obtain a clear idea of the scope of the work undertaken by this Commission it may not be out of place to examine more critically what is really implied by the study of River Terraces and Raised Beaches. To many geographers they are but elements in the present topography which merit description since they form part of the landscape—part of the environment of Human Activity. But the study of the landscape itself—Geomorphology—is the common meeting ground of geologists and geographers. The work of Prof. W. M. Davis of Harvard has provided a sound basis for the scientific approach to this study. He introduced the theory of the "Geographical Cycle,"[1] as a working hypothesis whereby a landscape may be analysed and the land forms classified according to their origin and stage of

[1] "The Geographical Cycle," W. M. Davis. *Geographical Journal*, vol. xiv., p. 481.

development. Each unit in the landscape is thus the result of the interaction of three factors, structure, process, and time. An ideal Geographical Cycle or Cycle of Erosion begins with the uplift of a " new land " whose structure is already determined by preceding events. The process of erosion begins even before the uplift is completed, and the resulting landforms vary according to the agents of denudation concerned in the process ; for instance, the coasts are at once subjected to marine erosion and the features produced are determined by the structure of the land, but these features will not be the same at any stage in the process. The whole tendency of denudation is to restore the equilibrium upset by the relative uplift of the " new land "—that is, to reduce the land to a plain at sea-level. Sea-level is generally the base-level of erosion, but there may be local base-levels, as for instance a lake in a mountain valley; these are generally features of the early stages of the Cycle.

The Geographical Cycle rarely reaches even the penultimate stage, when a " peniplain " is produced almost at sea-level, but it is interrupted, usually by a change of base-level due to a renewed relative uplift of the land.

There are two main schools of thought concerning the cause of the movement of land relative to sea-level, the one postulating the actual tectonic movement of the land, the other favouring changes in the sea-level owing to an increase or decrease in the volume of water—Eustatic movements.

River Terraces and Raised Beaches are land forms usually associated with the interruption of the erosian cycle by changes of base-level ; but if the terraces are to be used as evidence either in favour of the theory of tectonic or eustatic cause of the movement, they must be carefully studied and described or the evidence may be very misleading. In the first report of the Commission, the authors rarely attempt to describe the form of the terrace, and it is the form of the terrace which is the key to nature of the movement which caused the terrace. W. M. Davis recognised five main forms of terrace.[1]

(1) The Seashore Terrace.
(2) The Lakeshore Terrace.
(3) Structural Rock Benches in the valleys of dissected plateaux, e.g. the Colorado Valley.
(4) Cut and Built River Terraces.
(5) Drift River Terraces.

Seashore and Lakeshore terraces are raised beaches left by a relative retreat of the water. They may be notches cut

[1] *Geographical Essays*, W. M. Davis, p. 514. "The River Terraces of New England."

in the solid rock—wave cut plateforms—backed by cliffs, or drift deposits accumulated along the former shore-line, or even off shore banks or reefs now above the water-line. But all such terraces have one important characteristic which differentiates them from the River terraces, each terrace is originally at one level, though they may be subsequently deformed.

Structural Rock beaches are not related to the base-level of erosion. They may be caused by outcrops of harder rock, or by step-faulting, as described by Ph. Négris in the report on the "Marine Terraces of Greece."

River terraces are remnants of the flood-plain or bed of the river, and thus form a fossil "thalweg" of the river at one stage in its history. For this reason river terraces were graded to a local or regional base-level and are usually related to the present gradient of the streams.

There is an important distinction between the two main types of river terraces. The "cut and built" terraces (Fig. 1)

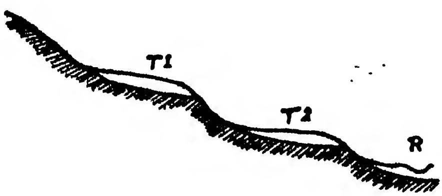

FIG. 1.

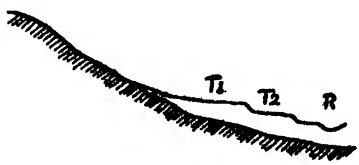

FIG. 2.

T^1 and T^2 = Terraces
R = River
Solid Rock = Shaded
Drift = Blank

are formed when phases of down cutting (due to change of base-level) alternate with phases of accumulation, when the river is cutting laterally and depositing its load on a flood-plain. Renewed uplift causes the river to incise itself in its former flood-plain and a step of solid rock is left when a new flood-plain is formed at a lower level during the following period of quiescence. The form of the steps between the terraces depends on the nature of the uplift. If the movement is slow a long drift-covered slope may be formed rather than a rock step. All the higher terraces of the Thames belong to this "cut and built" type.

Drift terraces are formed when a river is degrading a drift-filled valley (Fig. 2). These terraces may appear similar in form to the "cut and built" terrace if they are formed by a series of uplifts, but W. M. Davis distinguishes another im-

portant type of drift terrace[1] caused by a meandering stream slowly and continuously degrading a formerly aggraded valley, without any change in the volume of the stream. Terraces of this type rarely occur at the same height on both sides of the valley, their pattern is very irregular, and the separate terraces are very impersistent. The drift terraces of New England described by W. M. Davis are considered to be an accident in the normal cycle of erosion due to the infilling of mature valleys by glacial drift. But valleys may be aggraded and then again degraded owing to tectonic movements.

The occurrence of river terraces in a valley is not conclusive evidence that there has been any change in the volume of the stream unless the terraces form identical "flights" on each side of the valley. W. M. Davis also suggests that a graded river may be caused to degrade its flood-plain as well by diminishing its load as by increasing its slope if the volume of the stream is constant.

The former existence of local base-levels in the upper courses of rivers may cause the evidence of the terraces to be misinterpreted. De Martonne[2] suggests that only those terraces which occur near the mouths of the mature rivers should be used as evidence in reconstructing regional base-levels of erosion.

The study of the Pliocene and Pleistocene Terraces is particularly important because these river terraces and raised beaches form a basis for the chronology of recent geological time. It is in the terrace and beach deposits that the remains of the Pliocene and Pleistocene fauna and flora and the artifacts of the primitive men are preserved. They are also the key to the reconstruction of the topography and climatic conditions during the phases of the Quaternary Glaciation.

Detailed studies of the terraces have already been made in certain areas in Europe, especially in France, by de Lamothe and Chaput, where the same terraces can be traced and correlated in all the main river valleys opening to the Atlantic. Depéret has connected these terraces with the raised beaches on the Atlantic coast and has suggested a chronology for Quaternary time.[3] He postulates a continual relative lowering of sea-level from the Pliocene to the present.

The theory of the Eustatic origin of the changes of base-level is widely supported in France, but elsewhere, especially in America, the Tectonic theory is preferred. The evidence

[1] *Geographical Essays*, W. M. Davis, p. 514, "The River Terraces of New England."

[2] *Traité de Géographie Physique*, Emm. de Martonne.

[3] "Essai de Coordination Chronologique Général des Temps Quaternaires," Charles Depéret. *Compt. Rend. Acad. Sci.*, Paris, 1918–20.

collected by the Commission should throw some light on these problems.

In the Report of the Commission on Pliocene and Pleistocene Terraces, the communications are collected under the headings of five regions: The Mediterranean Basin, the Seaboard and Rivers of Spain, the Atlantic Seaboard and Rivers of France, the British Isles, Regions remote from the Mediterranean Basin and the Atlantic Coasts of Europe.

The Mediterranean Basin.—The paper by W. F. Hume and O. H. Little on "The Raised Beaches and Terraces of Egypt," really deals with the south-eastern corner of the Mediterranean Basin and the Red Sea.

Along the coast from Southern Palestine to the west of Alexandria there is no conclusive evidence of raised beaches, but there is definite evidence of movements during the last 2,000 years, when depression has been more marked than elevation.

In the Erythrean Depression (the Gulf of Suez and the Red Sea) terraces and raised beaches are well preserved. The best developed and most persistent feature is a raised beach at 15–20 metres above present sea-level. This is a Pleistocene raised beach containing many fossils belonging to existing species, and is considered to be analogous to a 20-metre beach more or less proven on the west coast of India and in East Africa. Other beaches and terraces occur at higher levels in the Red Sea area, but are impersistent; for example, well-developed features are found at 30, 60, 213 metres on the west coast of Sinai.

In the Nile Valley itself important and interesting results have been obtained by K. S. Sandford and W. J. Arkell. Only a short summary is given in the report of the Commission, but the results have been published.[1]

The Egyptian Plateaux were elevated in Miocene and possibly Oligocene times and a river system was initiated. During the Pliocene the Nile Valley formed an arm of the Sea, the Nile and Wadi Qena being flooded to a height 169 metres (550 ft.) above present sea-level. The Pliocene deposits consist of marginal breccias with limestones and clays towards the middle of the gulf. Uplift occurred at the end of the Pliocene or beginning of the Pleistocene and the river system was renewed. At this time an enormous amount of detritus was brought down by the big Wadis, such as the Qena and Shait, from the Red Sea Hills.

There is an ordered series of Pleistocene river terraces in

[1] Paper read before the Geological Society, July 1929, "The Pliocene and Pleistocene Deposits of Wadi Qena and of the Nile Valley between Luxor and Assiut," by K. S. Sandford.

the Nile Valley and in the major Wadis. The series of local and non-local stages is complicated owing to the meandering of the Nile, but the chief stages and associated Palæolithic cultures are as follows:

150 ft. (47 metres) terrace	Barren.
100 (29 ,,)	Chellian.
50 ,, (15 ,,)	Acheulian.
25–30 (8 ,,)	Early Mousterian.
10 ,, (3 ,,)	Mousterian.

After the last stage desert conditions were established and the Wadis became dry, the Nile alone surviving. At about this time the Nile cut a very deep channel and re-excavated the deep parts of the Plio-Miocene gorge. This channel was afterwards filled up completely and aggradation is still going on. The terraces are also traceable near Faiyum. It is suggested [1] that the Faiyum depression is essentially an erosion feature. There was a Faiyum lake in the middle Palæolithic which was drained before the Neolithic; this draining of the lake and the excavation of the depression below sea-level belong to the post-Mousterian (Sebilian) time and is connected with the cutting of the buried channel of the Nile. The depression has been preserved and not filled in with detritus owing to the oncoming of desert conditions. The work of E. J. Wayland in Uganda [2] indicates that the features of the Nile Valley may not be unconnected with Tectonic movements in Central Africa.

K. S. Sandford hesitates to correlate the Nile terraces with similar stages in Europe, or the African Pluvial periods with the European Glacial periods.

In his paper on the "Marine Terraces of Greece" Ph. Négris distinguishes three types of terraces on the coast of the Gulf of Corinth and Patras.

(1) Ancient surfaces of Marine erosion (Pontic).
(2) Post-Pliocene Tectonic Terraces.
(3) Recent Quaternary Marine Terraces.

The older terraces form plateaux at a height of more than 1,000 metres in the north, descending towards the west and south. They truncate rocks of Cretaceous and Eocene age, and in some places Pliocene deposits rest on the surface or in eroded hollows. These Pliocene beds are thought to be of Pontic age.

The Tectonic terraces are due to large scale step-faulting; in Pliocene time, they are easily recognised by their slope towards the south.

[1] "The Relations of the Nile and Faiyum in Pliocene and Pleistocene Times," K. S. Sandford and W. J. Arkell. *Nature*, vol. 121, p. 670.
[2] Paper read before the Geologists' Association, 1929, E. J. Wayland.

The recent terraces occur at various levels from 900 metres to sea-level. They give the impression, confirmed by a study of the borings of molluscs, of a continued relative uplift of the land.

A detailed list of the levels of these terraces is given, but they only apply to small areas and apparently most of the terraces are impersistent. In the Valley of Messina the terraces are only found at the lower levels, 25 to 300 metres. These terraces are often separated by very small vertical intervals. Their generally lower level is attributed to a tilting movement. Two limestone cliffs, several metres in height, with caves, occur at 140 and 180 metres above sea-level. These cliffs are horizontal, and it is suggested that they are conclusive evidence that there have been no tilting movements since the level of 180 metres. In the region near Pylos the limestones are perforated by molluscs at heights accordant with the terraces in the Valley of Messina. The limestones of the islands of the Archipelago also show these perforations up to a height of 690 metres on the island of Siphnos. There thus appears to be evidence of a general relative uplift of the land in Greece during the Pleistocene, accompanied by a tilting towards the south. Ph. Négris suggests that the fall in the sea-level of the Mediterranean is due to the foundering of the "Atlantide," "Tyrrhenide" and other continents within the Basin.

Michele Gortani in his paper on the "Marine and Fluviatile terraces of Italy," finds evidence of Miocene and Pliocene cycles of erosion above 1,000 to 1,500 metres in the Piedmont and Lombardy Alps, and above 500–1,000 metres in the Venetian Alps, while the marine Pliocene can be traced on the north-eastern slopes of the Apennines up to 800 and 1,100 metres, and is found at 800–900 metres in Tuscany. There is also a Pliocene peniplain in the granitic massifs of Calabria, between 850 and 1,200 metres. The marine Pliocene is also preserved on the north of Sicily at 700–900 metres, but is absent in the south. The plateaux of Gargano and Murgie stood above the Pliocene Sea, and in the central and southern Apennines there are dry valleys and levelled surfaces which belong to the Pliocene cycle.

Gortani adopts Stella's classification of the Pleistocene Terraces, and recognises two systems of terraces corresponding to two glaciations (Glaciations of the Diluvium Superior and Diluvium Inferior) with an interglacial uplift. The difference in level between these two sets of terraces decreases from 600–1,000 metres in the west to 200–700 metres in the east. There are apparently no terraces due to post-Glacial uplifts. The Diluvial deposits are preserved throughout the basin of the Po, though they have been subsequently terraced and

eroded by streams at the foot of the Alps and the Apennines; towards the mouth of the Po they are buried under recent alluvium. On the north-east flank of the Apennines Diluvial Terraces are found 200–250 metres above the Adriatic Sea; there are also terraces at 100–150 and 30–50 metres. Marine Terraces are found below the plateaux of Gargano and Murgie, at 100–200 metres (Calabrian) and lower levels. In Calabria the slopes towards the Tyrrhenian Sea show four groups of terraces due to marine erosion during the late Pliocene and Pleistocene. The "Sicilian" Beach occurs at 800 metres on the north of Etna, but elsewhere between 80 and 400 metres. The Tyrrhenian stage is also traceable on Etna between 30 and 250 metres, while in Sardinia it is between 1 and 10 metres.

At the beginning of the Quaternary folding and Volcanic activity produced great lacustrine basins in the Southern Apennines and in Tuscany, which were filled in with deposits and subsequently eroded to great depths.

In Italy, as in Greece, recent Tectonic activity makes interpretation of the evidence very difficult.

The paper by D. E. Hernández-Pacheco on the River Terraces of Spain is particularly important, as the bulk of the work was undertaken especially for Report of the Commission, and was necessarily very general, but it is hoped that it will serve as a basis for more detailed studies. The author points out that the five great rivers of Spain belong to different topographic regions.

The Douro and the Tagus are similar, and their courses may be subdivided into four parts:

(1) A torrent mountain course.

(2) A meandering course across the horizontal Tertiaries of the Mesita, where terraces are formed.

(3) A course in Western Spain, where down-cutting is again dominant.

(4) An Estuary course on the low plains of Portugal.

The Douro has a torrent course in the Iberian Cordillera, but before it enters the Tertiary plain of Old Castile the river makes a sharp bend at Numance at 1,000 metres above sea-level and flows to the Atlantic instead of the Mediterranean. This bend is considered to be evidence of a capture which took place after the highest (70-metre) terrace. There are three terraces in this region—

70 metres above the river.	(The deposits on this terrace are boulders—it can be traced towards the Mediterranean.)
44 " " " "	
10 " " " "	

On the plain there is a well-developed set of terraces in the Valley of the Pisuerga (a tributary of the Douro) near Vallodolid, where the present river bed is at 680 metres above sea-level.

100	metres	above	the	river.
70	"	"	"	"
30	"	"	"	"
10	"	"	"	"

There is also a striking "alluvial plateform" at about 900 metres, where there is a pebble bed about 1 metre thick resting on sediments which have yielded Pontian remains. It is suggested that this pebble bed may be Pliocene or Sicilian. The plateform can also be traced in the eastern part of the basin from Burgos to Lerma at 950 metres above sea-level, where red clays are found below pebbly alluvium. The author suggests that the clays belong to a dry Pliocene phase and that the alluvium is quaternary.

The Terraces of the Tagus are similar to those of the Douro, and may be summarised as follows:

Where the river leaves the mountains and enters the plain of New Castile, terraces are found at 55–60, 30, 15 metres above the present river bed.

To the east of the basin in Guadalajara, in a tributary valley, there are terraces at 22–30 and 7–10 metres above the river. In this area there is also a wide plateform 100 metres above the rivers, covered with rolled stones. At Aranjuez, in the middle course of the Tagus, the present river flows at 481 metres above sea-level and terraces are found at 100, 50, and 10 metres above the river.

At Toledo the Tagus flows at 428 metres above sea-level, with terraces 86, 52, and 17 metres above the river.

At Talavera de la Reina the terraces are confused, but those at 30 and 7 metres are the most constant.

Near Madrid two tributaries of the Tagus show some interesting features. The Jarama flowing from the central Cordillera has three terraces at 50–60, 27–30, and 12–15 metres above its present level. These terraces are cut in a wide belt of torrential deposits at the foot of the mountains, which is thought to belong to the early quaternary.

The Manzanares crosses an alluvial plateform 100–140 metres above its present level and shows well-developed terraces.

40 metres above the river	*Elephas antiquus* and other mammalian remains and
25–30 " " "	Chelo-Acheulian implements have been found at the base of these deposits.
6–12 " " "	

There is also a wide plateform on the slopes of the Toledo mountains, 200 metres above the rivers, with a deposit of rolled quartzite pebbles which sometimes attains the thickness of 90 metres.

The course of the Guadiana is different from those of the Douro and Tagus. It has no torrent course, but rises on the plain south-east of the " meseta " of New Castile, and crosses horizontal porous limestones of the Trias. In this region the course forms a series of long lakes separated by falls or rapids, and with no traces of terracing. Crossing the Miocene sands and marls of the plain of " La Mancha," the river winds over the plain, which is covered by a very thin bed of rolled pebbles at about 700 metres above sea-level. There are no terraces and the river has no valley below the level of the plain. Near Ciudad Real there is a terrace about 12 metres above the river where the deposits have yielded (*Elephas meridionalis*, *Hippopotamus major*, *Equus* and *Cervus*) a fauna associated with an older quaternary inter-Glacial period.

The Guadiana then flows through an area of Silurian schists and quartzites in a narrow valley, but towards the end of this reach three terraces are recognisable at 80, 17 and 7 metres above the river. Outside the valley there is a pebble-covered plateform 170 metres above the river.

The lower course of the Guadiana before it enters Portugal is over a wide plain covered by fine alluvium which suggests a lake bed, but there are no terraces.

The Ebro shows a well-marked terrace even in the plain of Reinosa, 850 metres above sea-level, at 16 metres above the river. On the plain of Miranda del Ebro there is another terrace 10–18 metres above the river. These two plains are separated by a torrent course. Three terraces are recognised in the plain of Aragon, at 90, 30, and 68 metres above the river.

The tributaries entering the Ebro from the Pyrenees have terraces associated with the terminal moraines of the Quaternary glaciers. There is also a plateform at the foot of the Pyrenees 100 metres above the rivers which the author, with Depéret and Chevalier, considers to be Sicilian.

When the Ebro cuts its way through the coastal mountains to the Mediterranean there are well-developed terraces at 100, 35–32, and 20 metres.

The Guadalquiver has no terraces in its mountain course, but they are developed on the Andalusian plain 230 metres above sea-level, at 78, 50, 35, and 15 metres above the river; there are terraces at similar levels on the Guadalimar near its confluence with Guadalquiver.

Near Cordoba there is a plateform at 150–230 metres, as well as terraces corresponding to those already listed. Just above

Seville the Guadalquiver enters what was an estuary filled even in historic time. There are plateforms on the Sierra Morena covered with deposits of rolled pebbles, now trenched by the streams.

All the Spanish rivers except the Guadiana have three or four well-developed terraces at comparable levels above their present valley floors. All these terraces are Pleistocene or Recent, but there is also a plateform found over wide areas at about 100 metres above the rivers, which belongs to the earliest Pleistocene or to the Pliocene.

Two papers referring to the Mediterranean area were read at the International Geographical Congress at Cambridge in 1928. The evidence of terracing in Tripoli seems to be very unreliable. Prof. G. Valsan described the river terraces of the Lower Danube. The system is complicated, but two terraces are specially persistent. The higher terrace is distinctly lower (25 metres) than the lacustrine deposits of the plain, which are of Levantine age, and appears to be about 45 metres above the river. East of the Argesh this terrace disappears, because a marsh—the remnant of the Levantine Lake—still existed in the eastern part of the plain. This terrace is considered to be contemporary with the fauna of *Elephas primigenius*, *E. Antiquus*, etc.

The lower terrace is 25–30 metres above the river falls towards the present river level in the east. At this time an important river crossed the Baragan steppe which has since been buried by a thick bed of loess. Terraces corresponding to those of the Danube can be traced in the tributary valleys up to the mountain region.

Is it possible to correlate the evidence of the river terraces and raised beaches in the lands surrounding the Mediterranean? In Greece and Italy there appears to be an early Pliocene base-level at about 1,000 metres above present sea-level, while marine Pliocene deposits are found at about the same level. But in Egypt the evidence of the Pliocene gulf is not found above 169 metres. These facts are not so conflicting as they first appear when it is recalled that there is conclusive evidence of a continued relative sinking of the Egyptian area since the beginning of Neolithic time, while elsewhere the movement has been one of relative uplift since the Pliocene.

In Greece and Italy the evidence of the Pleistocene terraces and Raised beaches is very complicated by later tectonic movements, which make their correlation impossible without fossil evidence.

The work done in Spain is thus of special importance, as the Iberian peninsula lies between the Atlantic and the Mediterranean, and has been relatively more static during

Quaternary time. The widespread occurrence of a late Pliocene or Pleistocene plateform at 100 metres above the present rivers falls in with Depéret's Sicilian stage. The records of the later terraces are on the whole consistent and point to three distinct breaks in the erosion cycle. The occurrence of Chello-Acheulean implements and *Elephas antiquus* in the middle, 23–30 metre, terrace of the Tagus, corresponding exactly to Depéret's Tyrrhenian stage, is noteworthy.

There appears to be no important difference between the quaternary histories of the Ebro and the Atlantic flowing-rivers, so that more detailed work on the river terraces of Spain may bring to light some very important facts. But at present the Mediterranean Basin is hardly a promising field for those in search of conclusive evidence in favour of the Eustatic origin of changes of base-level.

The Atlantic region dealt with in the report and other problems connected with Quaternary terraces and chronology will be discussed in a subsequent article.

(*To be continued*)

POPULAR SCIENCE

THE COBBLER'S-AWL DUCK

By REV. EDWARD A. ARMSTRONG

PICTURE a large bird, white as the snow, marked with a black cap and black bars on back and wings, standing high on long blue-grey legs, with a white neck, sinuous and graceful. Imagine further a long delicate bill, not straight or arched downwards in curlew fashion, but curving upwards like a cobbler's awl. Finally, endow these creatures of your imagination with grace of movement and such a degree of confidence in man that their interesting ways may be observed with great ease. You would think, would you not, that if any such creatures were known to exist, not in Florida or the Solomon Islands, but in this realm of England, that it would be the joy of individuals and the concern of authorities to protect them, that future generations might also take pleasure in their ways?

Yet this strange bird is a real bird, and until recently might have been seen nesting in flocks in the marshes and saltings of Norfolk and Suffolk, making the air ring with their cries and transforming the weary mud-banks and desolate pools and salt-pans (as gems shining in a dull platinum setting) by their bright unexpected beauty in what might have been but for them a scene as drab and drear as Ulalume. Their beauty has departed, their flapping wings are no more seen, their joyous calls are heard no more on those desolate wastes. One might wonder why God created such wildernesses did not one know that they were made as a setting for the beauty of the avocets which man has driven away.

Yes, the avocets are gone, save for an occasional wanderer, led, perhaps, by some ancestral memory to the ancient haunts. It is no wonder that they went. The marshmen, not satisfied with making puddings and pancakes with their eggs, were wont to loose their punt-guns into the flocks to save the trouble of unloading. The avocet was like a pretty girl who moves through the world confident in her virtue and innocence. She took men on trust, nesting in colonies without

concealment. She did not bother either to deck herself less attractively or to hide her eggs in tussocks like the redshank and the snipe. She lived where she could interfere with nobody and satisfied herself with the organic scum floating on the surface of the pools, so that the farmer could not grumble against her nor the fisherman cry out that she took his lawful prey. But the marshman liked her eggs, and the fisherman her feathers, to make into artificial flies, and so the avocet colonies, with every man's hand against them, dwindled away until the avocet as a nesting bird disappeared from our land.

The bird was sufficiently familiar to be endowed with several nicknames: "Cobbler's-awl duck" and "Scooper," obviously derived from the peculiar shape of the bill and manner of feeding; "Clinker," "Barker," and "Yelper," from the loud cries of the birds. Willughby, writing in 1676, mentions the avocet as occurring in winter on the east coast, and the statement of Plot, in 1686, suggests that at that time avocets were breeding in Staffordshire. Pennant, in 1769, speaks of the species as numerous opposite to Fossdyke Wash in Lincolnshire. At the beginning of last century the birds were rapidly decreasing and the extirpation of the last colony at Salthouse took place between 1820 and 1825, although there is a possibility that a few may have bred at the mouth of the Trent as late as 1840. The nearest place where avocets may now be found nesting is the Island of Texel off the Dutch coast. Its scattered European colonies extend from the Baltic to southern Spain, the Dobrogea, the Volga estuary, and the Kirghiz steppes. In Africa it breeds in Algeria, probably in Egypt and other localities as far south as Cape Colony. In Asia it is found from Asia Minor to the valley of the Hoang-ho. In the northern part of its range it is migratory.

One of the days of my life to which I look back with most pleasure was the day when, on the Island of Texel, I first beheld a colony of breeding avocets. On a marsh, only a few acres in extent, many pairs of these beautiful birds were nesting. What would have been but a dreary swamp without them was transformed into a fairyland by the dainty creatures. As we topped the dyke a wonderful scene spread dramatically before us. There seemed to be hosts of avocets in the air, flapping erratically hither and thither like big black and white butterflies, and in the water and on the green peninsulas which jutted into the shallow pools they stood or walked about, spangling the plain vesture of the marsh with living gems. The air was plangent with their calls and the entire little world pulsated with joyous and intense life. To look on such a scene is to be released momentarily from the limitations of sense experience, to feel in one's inmost being the happy

vitality of the world and to be caught into the rhythm of Nature's throbbing heart.

The warning cries of the old birds had sent the young into hiding, but they were easily discovered, grey, fluffy balls, with already upturned bills, and light slate-blue legs, crouching among the runted grass-tufts at the margin of the pool—very beautiful, and very fragile. Numbers were lying about dead. One we found had broken its leg, and was a most pathetic sight; we could do nothing for it and later found it dead. They are very susceptible to cold, and when the spring is severe the mortality is high. They feed independently of the old birds—I don't suppose their parents, with such queer bills, could feed them if they wished—and it is quaint and beautiful to see the youngsters wading industriously in an inch or two of water picking up minute specks of food, their tiny bills not allowing them to imitate with any degree of perfection the graceful and efficient movements of their elders. When newly hatched they are able, unlike the old birds, to pick up food by direct pecking.

When we settled into our hiding-place the avocets which had been flying to and fro, uttering a jangle of calls like the ringing of a multitude of bells, became reassured, and one by one dropped back to brood or feed. There is an air of dignity and nobility about the long-necked bird as he holds himself upright, suspicious of danger. Reassured, he stalks into the shallow water and starts feeding in his own peculiar way, resembling no other bird on earth. He steps along like a reaper, swinging neck and delicate upturned bill in swift rhythmic strokes back and forth on the surface of the water. It is difficult to see what he is obtaining—without doubt small crustacea, the larvæ of insects, fish spawn, and such like —but the subsequent movements of his neck and gullet show that the bird has not toiled in vain.

Those who have seen this nice correlation of form and function may be forgiven a chuckle when they read a statement from a French writer repeated in a popular work on British birds, "By aid of its webbed feet it is enabled to traverse, without sinking, the softest and wettest mud: this it searches with its curved bill, and when it has discovered any prey, a worm for instance, it throws it adroitly into the air, and catches it with its beak." Pennant, who had seen avocets "in considerable numbers in the breeding season near Fossdyke Wash, in Lincolnshire," was almost as rash in unguarded statement. "They feed," he wrote, "on worms and insects, which they suck with their bills out of the sand." Sir Thomas Browne wisely gave his imagination less play and contented himself with writing, "Avoseta, called shooing

AVOCET.

This photo is interesting as it shows slight curve in bill of young. It is still sometimes denied that the curve is noticeable so early.

Copyright photos: E. A. Armstrong.

AVOCET.

CLUTCH OF FIVE EGGS IN AVOCET'S NEST.

YOUNG AVOCETS.

RUFF.

Interesting as it is often stated, the ruff does not feed by day.

Copyright photos: E. A. Armstrong.

horne, a tall black and white bird, with a bill semicircularly inclining or bowed upward, so that it is not easy to conceive how it can feed."

The nests are primitive and are often, as on this Dutch "polder," within a yard of two of each other. They were composed of a few stalks and blades of dry grass on the turf or on a tussock, but in Spain the nest may be on dried mud, and on the shores of the Black Sea on shingle banks. There are usually three or four eggs, but we found one nest with five and were told that there had been one other clutch of the same number that season. Their appearance is accurately described by Mr. Jourdain, "The ground-colour is clay-yellow or stone colour with faint greenish tinge, with brownish black blotches or spots, generally rounded in shape, and underlying ash-grey shellmarks." Both male and female brood the eggs and are solicitous for the young. The period of incubation is about twenty-four days.

Surely there can be few purer joys on earth than to sit in such a hiding place as ours on the bank of the pool, drinking in the beauty of the surroundings and making notes of the behaviour of the birds. There were avocets all around, in the air, on land and in the water, and besides these other interesting and pretty birds of different species. Terns of various kinds were constantly flying past, an oyster-catcher—whose nest was near at hand—raised his penetrating cry from time to time, and a lazy-looking little Kentish plover fed on the muddy shores. "Kluit, kluit, kluit," rang the avocets' clamour as the birds called their Dutch name to all the world. The mere was dotted with the white figures, some with a brood of youngsters in their wake or feeding about them. Sometimes they gathered the chicks under them and brooded them on the water. Occasionally a bird would run swiftly through the shallow water with neck withdrawn and low in the water. The significance of this action escaped us, though it may have been nothing but an expression of the bird's anxiety for its eggs or young.

Prof. Julian Huxley, who studied the birds at this breeding-place, has recorded that the avocet has no courtship display as have other species. He also makes the comment, "The species is a singularly peaceable one," a conclusion reached, I can only believe, through insufficient observation. The dangers of generalisation are as great as the difficulties of accurate observation, and the field naturalist must ever be on his guard against believing or saying what he sees to happen once or twice happens always or happens in the same way in another place, or that what he has not himself seen does not happen. Quite apart from mistakes in observation, how

differently men may interpret what they see. Lord Bryce records that he, with two companions, made a tour in Iceland in the course of which one of their number came to the conclusion that the inhabitants were an irreligious people, another that they were very religious, and the third, that they were neither more nor less religious than other people. It is so easy for the imagination to supply what it thinks the eye ought to have seen, and for the mind, in its efforts to bring order into the varied world of experience, to make of occasional occurrences a general rule.

As we watched the avocets we noted again and again how quarrelsome they were. One bird would fly over another and try to strike it with its pendent feet. They would not suffer redshanks to feed in peace, and on two occasions I saw them chase spoonbills, forcing the larger birds into ignominious retreat. The old birds seemed specially intolerant of other species near their young.

In order to study and photograph the birds at close quarters we made a little hiding-place beside one of the nests still retaining eggs. This was on June 15, and by that time most of the eggs in the colony had hatched. One young bird which had just left the shell was in the nest when we started operations. The old birds were very suspicious of the lens peering out at them, and stalked about with disconsolate cries. At last one of them walked to within a short distance of the nest—an exquisite sight as I watched it at the range of a few feet—and, half-squatting, called the youngster from the nest. The little creature toddled out and, reaching its parent, was promptly tucked under her breast,—as pretty a sight as you could wish to see. I can still picture the scene vividly, the small ball of fluff looking up, the parent looking down with magnificent compassion and then rising a little to push the chick under her breast with her fine bill.

It was no easy matter obtaining a photograph, as the avocet would never come to the spot where the youngster was crouching, on which, naturally, I had focussed. Each time she would come some little distance away and call repeatedly until the young bird crept up to her. Little by little she lured it away.

When returning to the nest if her suspicions have been aroused, the bird, like Agag, walks delicately, with neck tucked in and hunched-up appearance. Even when sitting on the nest she will sometimes keep up her call. In addition to the ringing " kluit " a harsher cry is uttered occasionally.

Young and old can swim and the young have their bills more or less tip-tilted at all stages. One of my photographs of a bird just hatched shows the curve in the little bill.

We found that this colony of avocets was the outcome of

the protective work of one man who was in charge of one of the pumps which kept the island from being flooded. All honour to him! He had now been appointed the official watcher, but previously had done the work from his love for the birds, giving ten guilders to the boys of the neighbouring farm to induce them to leave the nests on their land. Thanks to him, the birds are increasing fairly steadily.

It is impossible to overpraise the labours of the Dutch in regard to bird protection, and particularly of the Dutch Society for the Protection of Birds (Nederlandsche Vereeniging tot Bescherming van Vogels), Heerengracht, 260–266, Amsterdam. They have been successful in saving numbers of interesting and rare birds from extermination, and in preserving tracts of country as breeding places, and the result is that Holland is one of the most delightful countries for the bird-lover. To pass from Holland into Belgium or France is to go from a land of birds to where they are sparse, skulking fugitives.

Why is it that birds are appreciated in Teutonic lands, and that they are persecuted where the Latin dwells? It would almost be possible for a man set down in a strange land to tell whether he was among a Latin or a Teutonic race by noting the relative timidity or scarcity of the birds! Even the robin which in Kent hardly avoids the gardener's spade is a shy woodlander in Normandy. In a region of the Engadine, where there is some doubt as to the origin of the Romantsch-speaking population, the fact that they have a fellow feeling for the birds adds weight to the belief that they are not a purely Latin people. It was the thought of the immigration of bird-destroying Italians into the Argentine which drew from W. H. Hudson the regretful declaration: "And when I recall these vanished scenes, those rushy and flowering meres, with their varied and multitudinous wild life—the cloud of shining wings, the heart-enlivening wild cries, the joy unspeakable it was to me in those early years—I am glad to think I shall never revisit them, that I shall finish my life thousands of miles removed from them, cherishing to the end in my heart the memory of a beauty which has vanished from the earth." It is true, as Kingsley reminds us in his beautiful lament for the birds of the fens, that our own bird life has been sadly depleted, yet the Saxon and the Celt have never thought of birds as a thing of nought, and at last strenuous efforts are being made, and made with a large measure of success, to protect our stock of birds. Let Kingsley's words remind us of what we have lost, and encourage us in our efforts to retain and increase what we have: "Dark green alders, and pale green reeds, stretched for miles round the broad lagoon, where the coot clanked and the bittern boomed, and the sedge-bird,

not content with its own sweet song, mocked the notes of all the birds around; while high overhead hung, motionless, hawk beyond hawk, buzzard beyond buzzard, kite beyond kite, as far as eye could see. Far off upon the silver mere would rise a puff of smoke from a punt, invisible from its flatness and its white paint. Then down the wind came the boom of the great stanchion gun; and after that sound another sound, louder as it neared, a cry as of all the bells of Cambridge, and all the hounds of Cottesmore; and overhead rushed and whirled the skein of terrified wild-fowl, screaming, piping, clacking, croaking, filling the air with the hoarse rattle of their wings, while clear above all sounded the wild whistle of the curlew, and the trumpet note of the great wild swan.

"They are all gone now. No longer the ruffs trample the sedge into a hard floor in their fighting rings, while the sober reeves stand round, admiring the tournament of their lovers, gay with ears and tippets, no two of them alike. Gone are ruffs and reeves, spoonbills, bitterns, avocets; the very snipe, one hears, disdain to breed. Gone, too, not only from Whittlesea, but from the whole world, is that most exquisite of English butterflies, Lycæna dispar—the great copper; and many a curious insect more."

Yet the tale is not as mournful as Kingsley would have us believe. The bittern has returned to its reed-beds and may be heard—

"Booming from the sedgy shallow"

as in the days of which Kingsley speaks, the coot and the sedge-bird are plentiful, and Harriers annually breed in the Norfolk Broads. The snipe is still with us, and within recent years a pair or two of nesting ruffs have been recorded. And who knows if only the Dutch can preserve and increase their spoonbills that one day the heart of every bird-lover will thrill with the news that a pair have bred successfully in England?

Think of the two great Dutch bird haunts together, Texel Island and the Naardermeer, and the above word picture is a very good description of them. When I wish to recall the multitudes of gleaming wings and the wild, joyous music of Holland's bird sanctuaries, I read Kingsley's words, and I am back again among the meres and polders, among the waving reeds or the acres of sea-pinks. I hear the water lapping against the boat as we quant slowly through the lily-spangled waterways, or the never-ceasing thunder of the waves on that island shore, I smell the briny pungency of the spindrift and the sweet fragrance of the sand rose, and I see water, earth

and sky adorned with a living jewellery, hosts of birds which sing and swim and dance and fly and dive, blythe, "jocund lyttel fowles" as gay as the day is long.

Our last night on Texel was crowned with an unexpected and exquisite sight. We set out to have one last look at the haunt of the avocets and found not only these birds, but eight spoonbills standing in a group in the shallow water. Some seemed asleep, while others were engaged in preening their snow-white plumage ; as they twisted their necks the beautiful feathers of the crest stood out against the dark background. One flew down and fed within a few yards of where we were concealed. It fed, advancing through the water, swinging its neck from side to side, with its bill open, apparently gobbling up small organisms. We gazed with admiration at the beautiful creature, white as the fairest snows of winter, save for the buff-tinged breast, the yellow face, the red eyes, and the black-barred yellow paddle-bill. An oyster-catcher stood stock-still gazing at the big white bird. It added a humorous touch to the picture, for it looked like a living question mark, its whole attitude expressing utter amazement, as if to say, "What on earth is this ?"

It was a wonderful evening, the stillness broken only by the murmurous chorus of countless frogs, and by the occasional cry of an avocet or the pipe of a redshank. The sun went down in a crimson glory, tinging the birds' plumage and adding new loveliness to the silver-tinselled pools. Then one of the spoonbills rose, swept round in a circle, neck and legs outstretched, and then, a dark figure against the sunset, flew swiftly away. As I stood there watching a scene so simple and yet so beautiful, the words of the poet Bryant, "To a Waterfowl," became suffused with a deeper meaning :

Whither, midst falling dew,
While glow the heavens with the last steps of day,
Far through their rosy depths, dost thou pursue
   Thy solitary way ?

Vainly the fowler's eye
   Might mark thy distant flight to do thee wrong,
As, darkly painted on the crimson sky,
   Thy figure floats along.

* * *

There is a Power whose care
   Teaches thy way along that pathless coast—
The desert and illimitable air—
   Lone wandering, but not lost.

He who, from zone to zone,
   Guides through the boundless sky thy certain flight,
In the long way that I must tread alone
   Will lead my steps aright.

ESSAYS

WILLIAM HIGGINS AND THE ATOMIC HYPOTHESIS. (J. H. White, M.Sc.)

In an age in which it is customary to attribute the very first attempt to fuse the Greek atomic conceptions with chemical experiment to John Dalton and Dalton alone, it will probably come as a shock to most readers to realise that William Higgins preceded him in most of his work by roughly fifteen years. Moreover, one finds in contemporary literature that it was quite customary, up to about the middle of last century, for writers on chemistry to speak of Higgins as the founder of the "chemical" atomic hypothesis. That we do not often encounter Higgins's name in modern histories of chemistry is due, as I propose subsequently to show, to a peculiar and unpleasant bias on the part of a man whom one might regard as the father of modern chemical history, Thomas Thomson. I therefore write this essay in the hope that it may fall under the eyes of any who are at present engaged in compiling chemical records, so that, their interest being aroused, they may reaward to Higgins his rightful place therein.

William Higgins was a Fellow of the Royal Society and was for some years Professor of Chemistry and Mineralogy to the Dublin Society. He had an elder cousin, Dr. Bryan Higgins, who was a noted chemist of the time, but beyond publishing one or two unimportant treatises and entering into an undignified dispute with Joseph Priestly, claiming that he, Higgins, was the originator of Priestly's famous "Experiments on Air," his work is not of great interest. He was a staunch phlogistian, but here and there in his writings one may encounter remarks that show the probable source of some of William's more mature ideas, *e.g.*:

"This proportion differs considerably from that which was computed in regard to the fixable air from acetous compounds; but whether the difference is owing to an erroneous estimation of the mere acid in acetous acid, and of the quantity of fixable air which it can yield by heat alone; or to a capacity of the principles of fixable air to unite in different proportions, when they meet in different circumstances, or with a great

excess of one or the other, I cannot yet determine." (*Experiments and Observations* [1786] Sect. 18, p. 272.)

"And of fixable air, dense inflammable air, acid airs, the phlogistic alkaline air, and others, I would observe that the atmospheres include molecules instead of solitary ultimate parts; for without this chemical union of heterogeneal parts, and the formation of molecules, an elastic fluid, of the kind that I now speak of, could not differ as it does from either kind of matter of which it is composed." (*Ibid.*, p. 317.)

William Higgins's chief work, *A Comparative View of the Phlogistic and Antiphlogistic Theories*, 1789, was published when the war between the two theories was at its fiercest. In this book Higgins takes a series of examples and, logically setting out the explanation from each of the two points of view in turn, shows that the Antiphlogistic is the only one acceptable. While he is doing this his atomic ideas are introduced, and though they are quite lucid, the fact of their publication in a subordinate position probably hindered, as he himself pointed out later, their making much impression, especially at a time when the phlogistic controversy was engaging so much public attention. In general the section headings of the book are the same as those in Richard Kirwan's *Essay on Phlogiston and the Constitution of Acids*, 1787, and it is the phlogistic conclusions reached by Kirwan that Higgins, by repeating and verifying the experiments, sets himself to refute. From the descriptions of these experiments that Higgins gives, it would appear that his work was carefully carried out, and that the results gained were as accurate as the crude methods of the time permitted. In the course of the treatise, Higgins is found to anticipate Dalton chiefly in respect of the following:

1. In "nitrous air," "every ultimate particle of phlogisticated air (nitrogen) must be united to two of dephlogisticated air (oxygen); and these molecules combined with fire constitute nitrous air." (2nd ed., p. 14.) This combination with fire was an idea probably derived from Bryan Higgins and appears to have been similar to the later "caloric" view.

2. Sulphur is a simple substance, "whose ultimate particles attract dephlogisticated air with forces inherent in themselves." (*Ibid.*, p. 35.)

3. On p. 36 occurs his first mention of the idea of the law of multiple proportions in relation to "fixed" and "volatile" vitriolic acids. (Sulphuric and sulphurous acids, although the combined water was not yet taken into account.) "In volatile vitriolic acid, a single ultimate particle of sulphur is intimately united only to a single particle of dephlogisticated air; and that in perfect vitriolic acid every single particle of sulphur is united

to two of dephlogisticated air, being the quantity necessary to saturation." (The term "saturation" was later defined by Higgins as "a limit to the proportions in which the particles of elementary matter unite.")

4. "Water is composed of molecules formed by the union of a single particle of dephlogisticated air to an ultimate particle of light inflammable air (hydrogen)." (*I.e.*, oxygen and hydrogen in the ratio 1 : 1 even as Dalton supposed.) Here he demonstrates also an example of the law of combining volumes later established by Gay-Lussac, and also approaches very near to the Avogadrian hypothesis. He is able to prove that the constituents of nitrous acid (see later) and vitriolic acid can unite together in various definite proportions to form different compounds, and here tries to determine if the constituents of water will behave in the same way, but concludes from experiment, "they are incapable of uniting to a third particle of either of their constituent principles." (Sect. 3. "Of the Vitriolic Acid.")

In the same section, also, he uses diagrams to illustrate the attraction of a single particle of sulphur for two different particles of dephlogisticated air, representing the different particles with letters of the alphabet and connecting them with lines bearing arbitrary figures to represent the forces of attraction between those particles.

5. "When vitriolic acid, whether diluted or not, is mixed with an oil, an ultimate particle of vitriolic acid influences with a certain force an ultimate particle of oil, while the latter attracts the vitriolic with the same force." (P. 66.)

6. Of the constitution of "hepatic gas" (hydrogen sulphide) he reaches the erroneous yet definitely atomic conclusion that "the number of the ultimate particles of sulphur in hepatic gas are to those of the inflammable air as nine to five." (P. 79.)

7. In Sect. 4 (pp. 83 and 84) we come to the second and perhaps the most striking example of the law of multiple proportions, profusely illustrated with diagrams, the constitution of the nitrous compounds. His results may be briefly summarised thus:

Substance.	Particles of phlogisticated air contained.	Particles of dephlogisticated air.
Perfect or colourless nitrous acid . .	1	5
Pale or straw-coloured nitrous acid . .	1	4
Red nitrous air or vapour . . .	1	3
Nitrous air	1	2

8. "Nitrous air and the dephlogisticated nitrous air" (nitric and nitrous oxides) "are composed only of two principles, namely phlogisticated and dephlogisticated air, and that they only differ in the proportion of these."

9. Concerning the subject of the oxidation of metals with steam he says: "If the calcination of metals depended solely upon their union to dephlogisticated air it must be supplied by water when steam is brought in contact with them; and as every particle of light inflammable air is united but to a single ultimate particle of dephlogisticated air, inflammable air must be disengaged in proportion to the quantity of dephlogisticated air which unites to the metal; or, in other words, according to the degree of calcination it requires." (P. 194.)

And later: "To account for the calcination of metals by steam, etc., and for the reduction of these again to their metallic splendour in inflammable air, we must, in my opinion, have recourse to a mode of reasoning quite different from the preceding; and consider metals to be simple bodies whose ultimate particles attract dephlogisticated air with greater force than light inflammable air." (P. 247.)

Enough examples have now been given to show the general nature and drift of his arguments, but for any who may be interested and able to refer to a copy of the work in question, other interesting and relevant passages may be found on pp. 120, 123, 132, 141, 157, 211, 256, 263, 274, etc.

Thus, from the above work, we cannot justly withhold from Higgins the merit of the first clear atomic concept and the foundational ideas which lie behind both the laws of definite and multiple proportions. Higgins claims that 1,000 copies of the book were sold, a fairly considerable number for those days, but for various reasons no one appears to have been impressed by the novel ideas therein contained.

The first publication of Dalton's theory appeared in 1807 in the third edition of Thomas Thomson's *System of Chemistry*, in which he relates how in 1804 he was informed by Dalton of his inspiration. Dalton's own book did not appear till 1808 (*A New System of Chemical Philosophy*). I think it possible that Dalton may have seen Higgins's book, although Dalton himself denied any conscious knowledge of having done so. The Portico newsroom and library at Manchester, which Dalton frequented, may very well have contained a copy; and the later researches of Roscoe and Harden (*A New View of the Origin of Dalton's Atomic Theory*, 1896), who prove from their discovery of Dalton's own lecture note-books that, contrary to the usual belief, founded on Thomson's statements, Dalton originally approached the theory from a physical standpoint and did not put it to the test of experiment till later, show that, in such a case, Dalton would be the more likely to have been influenced, consciously or unconsciously, by knowledge gained from any previous worker in the same field. Many arguments were put forward by various writers during the nineteenth

century to prove that Dalton never saw Higgins's work; but none is, to my mind, conclusive. However, the great advance of the work of the former on that of the latter lies in Dalton's effort at compiling a list of Relative Atomic Weights and his use of the hypothesis as the basis for all his subsequent work, the phlogiston theory being practically dead at the time of the death of Priestly (1804). He was not quite as clear as Higgins (as far as Higgins went) in the distinction between our present "atom" and "molecule."

The sequence of events which led up to Thomson's denial of Higgins was as follows. The Swedish chemist, Berzelius, wrote in the *Annals of Philosophy* for December 1813 (a monthly magazine of which Thomson was the Editor), in an essay on "The Cause of Chemical Proportions," the following words:

"As far as I know the English philosopher, Mr. John Dalton, guided by the experiments of Bergman, Richter, Wenzel, Berthollet, Proust, and others, was the first person who endeavoured to establish that hypothesis (*i.e.* the atomic hypothesis). Sir H. Davy has lately assured us that Mr. Higgins (*i.e.* William), in a book published in 1789, established the same hypothesis. I have not seen the work of Mr. Higgins, and can only notice the circumstance on the authority of Davy."

Now Thomson had used all his energies to champion Dalton's views, and had thereby gained no little prestige. Thus he was in no mood to welcome a rival and so adds a footnote of his own to this effect (apparently after a brief reference to the work mentioned).

"The work of Higgins on Phlogiston is certainly possessed of much merit, and anticipated some of the most striking subsequent discoveries. But, when he wrote, metallic oxides were so little known, and so few exact analyses existed, that it was not possible to be acquainted with the grand fact that oxygen, etc., always unite in determinate proportions which are multiples of the minimum proportion. The atomic theory was taught by Bergman, Cullen, Black, etc., just as far as it was by Higgins. (If he had any grounds for this statement I have been unable to discover them.—J.W.) The latter, indeed, states some striking facts respecting the gases, and anticipated Gay-Lussac's theory of volumes; but Mr. Dalton first generalised the doctrine, and thought of determining the weight of the atoms of bodies. He showed me his table of symbols, and the weights of the atoms of six or eight bodies in 1804; and I believe the same year explained the subject in London in a course of lectures delivered in the Royal Institution. The subject could scarcely have been broached sooner. But about the same time several other persons had been struck with the numbers in my table of metallic oxides published in my

Chemistry"; and the doctrine would have certainly been started by others if Dalton had missed it."

A champion for Higgins was, however, shortly forthcoming in the person of another resident of Dublin, a Mr. John Nash. This gentleman appears to have been for some years a member of the Manchester Literary and Philosophical Society with which Dalton was intimately connected, and he must therefore have been familiar with the work of both Higgins and Dalton. He may have known both men personally, in which case his judgment would be of increased value. Nash published a paper in the *Philosophical Magazine* for January 1814 entitled "The Discovery of the Atomic Theory claimed for Mr. Higgins," in which he tears Thomson's footnote systematically to pieces, pointing out among other features that if Thomson referred to the book as Higgins on "Phlogiston" he could not have been very familiar with it. It is noteworthy that nowhere does he depreciate in any way the work of Dalton, but simply claims recognition for Higgins in a candid and straightforward manner. The paper, however, called forth another from Thomson in the *Annals of Philosophy* (May 1814) headed "On the Discovery of the Atomic Theory," in which he drops any pretence at a non-partisan attitude, and becomes definitely biased and even unpleasant. However, the correspondence is distinctly interesting. The statement of Davy that led to Berzelius's remark appeared as a footnote to the Bakerian lecture in the *Philosophical Transactions* for 1811, p. 15, but Davy's entire attitude is hard to understand; he appears to have oscillated during his lifetime between Higgins and Dalton, but finally declaring in favour of Dalton. It seems probable that he allowed personal animosities to affect his judgment.

In the same year that saw the publication of Nash's paper, appeared another book by William Higgins entitled, *Experiments and Observations on the Atomic Theory, etc.*, in which he himself attempted to establish his claim to priority in conception of the atomic hypothesis on the basis of the views published in the *Comparative View*. This makes interesting and sometimes amusing reading, but unfortunately he sees fit to lodge the first charge of plagiarism against Dalton, though whether well-founded or not, possibly we shall never know with certainty. Higgins also has his tilt at Thomson's spurious footnote, but Thomson appears to have had no more to say on the subject. The state of his feelings, however, is sufficiently indicated by the calculated omission of all reference to William Higgins from his *History of Chemistry*, published some years later. This is in many ways an excellent work, and being, as it was, a standard reference book for many years, nearly all later histories acknowledge some indebtedness to it. It is in

this way, I feel, that one of our most brilliant pioneers has lost the public recognition that the merit of his work warrants.

NOTE.—If any reader should wish to refer to a copy of the *Comparative View*, the Library of the Chemical Society has one of the second edition published in 1791. The British Museum Library had not a copy at the time the above research was undertaken, though it is possible it has obtained one since. I have seen one other copy in private possession, also of the second edition. It should be noted that all page references in the text refer to the second edition, and it may be that the pages of a first edition copy will be found slightly different.—J. W.

LOCH NESS AS AN ICE-FREE BASIN. (Lieut.-Col. W. H. Lane.)

THE fact that Loch Ness never freezes is a phenomenon that is intimately connected with several branches of Science. Archæologically it is of some importance. Bathymetrically it gives rise to many problems, and geologically it is also of some moment.

Let us review the archæological aspect of the case first, introducing geological data where relevant.

Did palæolithic or neolithic man inhabit the shores of Loch Ness? Neolithic man certainly did so, as two flint flakes and a bone object have been found on the ancient raised fan of the Moriston River where it debouches into Loch Ness. The question then arises—what was the special attraction that drew Neolithic man to the shores of Loch Ness? But at this stage a geological retrospect may perhaps be expedient. When did the men of the Old Stone Age first populate the Loch Ness basin? This again leads up to the query—when did Loch Ness become a fresh-water basin? The following extract is taken from the *Memoirs of the Geological Survey of Scotland*, Sheet 83:

" Reference may here be made to the evidence pointing to the existence of a deep pre-glacial channel at the mouth of the valley of the Ness. In 1893 a bore was put down for water by the Highland Railway Co. within their goods yard at a point 1600 ft. N.E. of the junction of High Street and Inglis Street. The ground at this point is 29 ft. above Ordnance datum level. . . . An examination of the entries in the journal of the bore shows that the materials passed through to a depth of 21 ft. from the surface consist of sand, gravel, and mud, with thin intercalations of what is termed ' hard-bound gravel.' Beneath this sequence come 78 ft. of sand, underlain by 45 ft. of gravel, sand, and boulder stones. Farther down the borers encountered layers of ' hard-bound gravel ' alternating with boulder stones, which attain a thickness of 17 ft. At a depth

of 163 ft. a mass of clay 13 ft. thick was pierced. From this horizon to the bottom of the bore there is a succession of beds of sand, sand and gravel, coarse gravel and boulders, the lowest layer being 9 ft. 9 ins. of sand. The total depth of the bore was 319 ft. 9 ins. The local engineers and those who had charge of the boring operations were satisfied that no solid rock had been reached. The site of this bore lies about a mile to the east of the line of the Great Glen Fault. . . . There might well be a continuation north-eastwards of the pre-glacial channel of the Ness, now concealed by glacial, post-glacial, or marine deposits."

Now it is evident from this excerpt that, in pre-glacial times, the present basin of Loch Ness did not exist, but that a salt-water fjord must have extended well up the Great Glen, certainly as far as Fort Augustus, if not farther. However, of one thing we may be fairly well assured; whether the Moray Firth extended up to Fort Augustus or not, except perhaps in some cave, all traces of pre-glacial man will have either been obliterated by, or buried in, the deposits of the glacial periods. In this connection it may be interpolated that, as far as the Loch Ness area is concerned, the Ice Age is divided into three distinct periods, or phases.

(1) The period of maximum glaciation, when the whole surface was covered by a sheet of ice moving outwards from an ice-shed lying further to the west and north-west.

(2) The period of large confluent glaciers pouring out of the upper parts of the main valleys crossing the lower parts of the water-sheds, and leaving only the higher hills as "nunataks" above the surface of the ice.

(3) The period of local valley glaciers, gradually shrinking up the glens.

Now the ponding up of Loch Ness will have been occasioned during the second phase of glaciation, and it is probable that ancient man drifted up to these regions towards the close of the final glacial phase. Let us again repeat—what feature attracted ancient man to these latitudes? Surely it must have been the fact that Loch Ness was an open sheet of water summer and winter alike, while the adjacent glens were filled with glaciers. With men of the Old Stone Age it must frequently have been a case of "hunger gnawing at their bellies." Therefore they constructed their rude habitations where best they could obtain their food. During the winter months when, at the close of the final glacial phase, the hillsides and glens were snow- and ice-bound, Loch Ness was ice-free. As at the present day, so in the past, the wild beasts of the forest would congregate round the open shores of Loch Ness, where they could be more easily trapped and killed by ancient man.

Furthermore, we may be sure that ancient man was also expert in catching salmon, trout, and eels, all of which must have abounded in Loch Ness.

Now the Loch Ness area has never been surveyed archæologically. If the arguments which have been brought forward to demonstrate that ancient man probably established his colonies on the shores of Loch Ness are acceptable, then may we not hope that, when a systematic survey is conducted scientifically by archæological experts, the fact that Loch Ness is and has been for many thousands of years an ice-free basin will receive its due measure of recognition.

Let us next turn to the bathymetrical side of the problem.

What is the reason assigned as an explanation of the fact that Loch Ness is an ice-free sheet of inland water? In 1903 and 1904 the Bathymetrical Survey of Loch Ness was conducted. A perusal of the report would seem to indicate that the cause of Loch Ness being entirely ice-free was mainly attributable to its great depth—754 ft.—though it must be remarked that no *definite* statement to that effect was apparently recorded. Now let us probe into this view and endeavour to ascertain whether it is acceptable either in whole or in part. If the sole reason for Loch Ness being entirely ice-free is on account of its great depth, why is it that the shallow bays are also wholly ice-free? Secondly, why is it that the River Ness is entirely ice-free, while at the same time the Moriston River is frozen over to such an extent that the ice will bear a cart and horse? It may be added also that the Moriston River is a considerably swifter stream than is the River Ness. Thirdly, why is it that the ponding up of the Great Glen at its N.E. outlet ceased where it did, and again commenced at the present site of Fort Augustus? What does this particular geological feature imply? Boulder-clay has been deposited in many zones along the N.W. shore of Loch Ness. At Invermoriston there is a belt perhaps 150 ft. in thickness. Yet we find the deposits of glacial drift from the final phase of glaciation below the level of this belt of boulder-clay, notably on the raised fan of the Morison River. Then, when we reach Fort Augustus we again come upon the terminal and lateral moraines of the Great Glen glacier. Now, do not these facts indicate that, comparatively speaking, a very rapid thaw of the Great Glen glacier must have occurred which brought its terminal face with considerable speed from the neighbourhood of Aldourie to that of Fort Augustus? Then does not the presence of the terminal and lateral moraines in the neighbourhood of Fort Augustus lead one to suppose that from this point the shrinking process of the Great Glen glacier resumed its normal rate? What is the cause of this phenomenon?

Was the sudden melting of the Great Glen glacier due to climatic changes in temperature, or was it primarily due to geological forces?

These problems would rather indicate that there must be some other influences at work than mere depth to maintain the temperature of Loch Ness and of the River Ness invariably above freezing-point and to account for the rapid thaw of the Great Glen glacier between Fort Augustus and Aldourie. If any such influences exist, what is their scope?

The following extract is taken from the *Bathymetric Survey of the Fresh Water Lochs of Scotland*, Vol. I, p. 112:

"Towards the end of May 1904 the records of temperature at the surface of Loch Ness began to show rapid changes of great amplitude. So erratic did the curves obtained by means of the Callendar recorder appear that they were at first attributed to instrumental errors. But the changes were checked by means of mercury thermometers. It was not possible, either, to attribute the changes to wind, for they occurred on the calmest days. Nor can they have been due to river influences, for they were also observed at Dores, where there is no river entering the Loch to make the observations suspicious. On one occasion, in two minutes the surface temperature was found to change as much as 6° Fahr. On another occasion when there was a quantity of pollen from flowers on the shore suspended in the loch, it was observed from the motion of the particles that different layers of water were moving in different directions, and the surface waters were evidently in a very agitated condition, although the surface of the water was quite calm."

The problem would seem to resolve itself into the question whether geological influences may not be responsible, at any rate partially, for the continuous maintenance of the temperature of the waters of Loch Ness above 32° Fahr. In this connection it may be remarked that it has been estimated that the quantity of heat set free by Loch Ness annually is equal to the heat which would be set free by the combustion of 2·4 million tons of coal (*Bath. Survey*, Vol. I, p. 134).

The area represented by Sheet 73 of the Ordnance Survey has not yet been geologically surveyed, but Sheet 83 has been surveyed. Let us therefore study the report of this survey, which embraces the lower portion of Loch Ness. One of the outstanding geological features of this district is the Great Glen fault. Under the heading "Faults" in these *Memoirs* we read:

"The most important line of movement in the area under description is the Great Glen fault, which, as already indicated, probably skirts the western shore of Loch Ness, runs along

the valley of the Ness, and has determined the straight feature along the eastern coastal belt of the Black Isle. . . . The periodic recurrence of earthquake phenomena in the Inverness district apparently indicates intermittent movement along this line of fracture. . . . One of the most remarkable effects of the earthquake of 1901 was a long crack or fissure in the northern bank of the Caledonian Canal near Doch-garroch Locks. It was formed in the middle of the towing-path, and could be traced at intervals for a distance of 600 yards. In no place was the fissure more than half an inch wide."

Also: "The triangular area lying to the east of Loch Ness and the River Ness . . . furnishes important evidence. It shows the old floor on which the Orcadian Series was laid down, the basal conglomerates and sandstones overlying that floor, the ichthyolite bed with its characteristic limestone nodules, and a great succession of pebbly grits, sandstones, flagstones, and shales westwards towards Loch Ness and the River Ness. By means of this evidence an approximate estimate can be made of the minimum amount of downthrow of the Great Glen fault between Inverness and Loch Ness. . . . The accompanying horizontal section . . . shows the position of the Great Glen fault [along the west shore] whose minimum amount of downthrow must here be about 6000 ft."

Let us now endeavour to visualise what these facts and conclusions actually imply. The Great Glen fault, between Inverness and Loch Ness, has a minimum downthrow on the south-east side of the fracture of 6000 ft. Now if all the superficial deposits overlying the old floor could be removed, in the fracture of the earth's crust represented by the Great Glen fault, there will be a difference of 6000 ft. between the *same* stratified horizons on opposite sides of the fault. The temperature gradient for depth in the earth's crust is 1° Fahr. for every 50 ft. of depth below the general surface level. The temperature at a depth of 6000 ft. below the general surface level of Loch Ness would therefore be about 120° above normal temperature. Whether the downthrow below the whole length of the bed of Loch Ness is represented by the same figure, viz. 6000 ft., is not known, but the actual amount of downthrow does not affect our argument materially. Now in a fracture of such extent we should expect to observe a considerable outflow of water between impervious strata at temperatures varying, according to depth, from zero to 120° Fahr. above surface temperature. Owing to the unconformable nature of the deposits abutting the south-east face of the fault above the old floor it is conceivable that a considerable volume of this heated water might be driven into the bed of Loch Ness by pressure, and find an outlet into the side of the Loch Ness

rock basin by percolation. In other words, the fault-plane would provide a channel for the ascent of heated water.

The next question is—can this thesis be proved? Of course the only *definite* evidence would be the actual discovery of thermal springs below the surface of Loch Ness. But can we bring to light any analogous proof to support this contention, the acceptance of which would tend to confirm the opinion that one of the reasons why Loch Ness and the River Ness never freeze is the influx of warm water through the bed and sides of Loch Ness below its surface?

Now the hill Mealfuarvonie represents about the most western and southern outlier of the Old Red Sandstone deposits on the west side of Loch Ness. Further south-west the deposits have been entirely denuded, exposing the old floor to the action of the forces of nature. Not only have the overlying deposits been entirely denuded, but erosion has eaten large fissures in this ancient plain of marine denudation. However, the expanse of this floor can still be easily traced. It is represented by the general surface level of the plateau bounded on the north by Glen Urquhart, on the south-east by the Great Glen, on the south by Glen Moriston, and on the west and north-west by the margin of Sheet 73 and the River Glass; also by the level of the plateau of the massif separating Glen Moriston from the Great Glen. The altitude of this floor now stands about 1500 ft. above sea-level. It is evident, therefore, that although the downthrow on the south-east side of the Great Glen fault may be anything approaching 6000 ft., the uplift on the north-west side of the fracture will be somewhere in the vicinity of 1500 ft.

If there is an outflow of water from the face of the fracture, below the surface of Loch Ness, there should similarly be an outflow from the sides of the hills above surface level. Furthermore, bearing in mind the temperature gradient for depth, we should expect the temperature of springs, say at 100 ft. above sea-level, *i.e.* 1400 ft. below the old floor level, to be 28° Fahr. above temperatures at the old floor level. Other factors, however, would affect this calculation, such as the dip of the strata, etc. To put the case generally, if our arguments have any basis of fact, then we should expect to find warm springs emanating from the fracture of the Great Glen fault. It was with the object of putting this theory to a practical test that my wife and I set out on the afternoon of December 17, 1927. Meteorological and physical conditions appeared favourable to the conduct of the experiment. The whole country-side was held fast in the grip of an exceptionally severe frost. The Moriston River, where it debouches into Loch Ness, was a sheet of ice from bank to bank, and the ice

was of such thickness as to bear the weight of a man of average proportions. The tests were conducted in the company of my wife, who acted as witness to the authenticity of the records. It may here be stated that owing to the lack of suitable instruments the *absolute* accuracy of the temperature cannot be guaranteed, but two reliable thermometers were employed, and the temperatures were taken by each instrument independently, and the readings recorded up to the nearest half degree. The first test was that of a roadside spring opposite the gateway of our northern drive. During the summer of 1926 we experienced a six weeks' drought; the water supply for our house, which is taken from an adjacent burn, completely failed, and we had to convey water by hand from this spring for household purposes. The summer of 1927 hardly gave us a day without rain; yet to all outward appearances the flow of this spring remained constant. It seemed probable, therefore, that the origin of this spring was deep-rooted, and further that it had no connection with surface drainage. Our thermometers did not register below 30° Fahr., but as far as we could gauge the outdoor temperature was about 26° Fahr. at 2.30 to 3.30 p.m. The spring-head is really situated beneath a boulder-stone embedded a few feet farther into the bank of glacial boulder-clay which overlies the spring. From the spring-head a short length of drain-pipe empties into a rough horse-trough. The mercury was plunged into the spout of water at the lip of the drain-pipe. It immediately shot up from approximately 26° Fahr. to 46° Fahr., and remained at that level constant.

We next visited the spring-head of a burn which flows through our property. As far as surface indications could be accepted the source of this spring was situated at about the level of the top of the bank of boulder-clay. Actual spring-head was not found, but the channel a few feet lower down contained a considerable flow of water. By means of removing some rounded stones from the channel a small pool was formed. On the mercury being immersed in this basin it sprang from about 26° to 43½° Fahr. It is possible that if the hill-side were cut away until actual spring-head could be located the record of temperature would be slightly higher. At the culvert where the burn flows under the main Inverness–Fort William road, distant from spring-head about 150 yards, the temperature was again taken and the record was 37° Fahr.

We next visited the mouth of the Moriston River where it debouches into Loch Ness. An ice-floe had been driven by the north wind towards the south bank. This floe was gently oscillating from the action of the miniature waves, but between it and the shore-ice was a narrow channel of

water with a thin film of ice just forming on its surface. The mercury was immersed to a depth of 4½ ins. and recorded a temperature of 33° Fahr. The apex of the Rhu promontory was then crossed (a distance of about 25 yards), and kneeling on a stone the temperature of the Loch was taken at identically the same depth and 39° Fahr. was registered. Owing to the breeze, which was hardly perceptible, being from the north, it was blowing off-shore at this spot on Loch Ness, consequently there was not a ripple to disturb the surface. It was thus possible to immerse the mercury to *exactly* the same depth. Now as 39° Fahr. represents the maximum density point of fresh water we should expect that the water of the Moriston River being of a lower temperature and consequently lighter would overspread the surface water of Loch Ness at its debouchment. Yet the results of these tests would seem to indicate that the latent heat of the water of Loch Ness is of such intensity as to overcome the frigidity of the Moriston River waters.

Let us at this stage of the discussion review briefly the results of the tests carried out in connection with the temperatures of the two springs. It may be argued that the temperatures of these springs merely indicate that the water is derived from a level only a few feet below the surface, where it would be unaffected by frost. If such were the case, it could be argued with equal force that if the springs were merely derived from surface drainage, then both springs would infallibly have dried up during the prolonged drought of the summer of 1926, as did the burn from which our water supply is derived. On the contrary, both springs maintained a steady flow.

Two other tests were conducted. On midsummer's day, 1928, the temperature of the spring opposite the gate at our northern entrance was taken with the same thermometer as used in the previous midwinter, and the mercury recorded 48° Fahr. Again, the temperature was taken about mid-day on December 27, 1928. The ground temperature in the shade was 35° Fahr., and the temperature of the spring once more proved to be 46° Fahr. From these figures we might infer that this spring has its origin in the zone of constant temperature. In spite of the fact that the heads of these springs are situated about 50 ft. and 150 ft. respectively above loch level, and further that the water from the second spring has to travel a full quarter of a mile above ground before it empties into Loch Ness, yet the statement is open to verification by anyone that the combined waters of these two springs *do* pour into Loch Ness at a temperature some degrees above freezing-point when the pipes in our house are frozen solid.

Next—have we produced any facts which might account for the rapid retreat of the terminal face of the Great Glen

Glacier from Aldourie to Fort Augustus? Now it can readily be imagined that before the impounding of the Great Glen near Inverness had arisen, the effects of thermal springs emanating from the fracture of the Great Glen fault would not be felt as they would be pouring their waters direct into the sea through the vault at the base of the glacier. Immediately the outlet of the Great Glen was ponded up by glacial drift, assisted by the 100-ft. rise of the whole land, the warm water would be impounded and so come into direct contact with the glacier, with the inevitable result that the glacier would be, comparatively speaking, rapidly thawed. The very formation of Loch Ness may be attributable to the influence of thermal springs.

It may perhaps be asserted that insufficient evidence has been set forth to prove that one of the reasons why Loch Ness and the River Ness never freeze is the influx of heated water from thermal springs, yet it is hoped that the data collected and recorded may be considered of such importance as to warrant the conduct of further investigation with the aid of adequate scientific instruments such as aneroids and special thermometers, preferably by a trained geologist. The opinions of an expert will infallibly carry more weight than will the observations of an amateur, no matter with what care and zeal he may approach the subject.

NOTES

Fisheries Investigations

Growth-rate in North Sea Codling.—Following his previous researches on the Cod (1924, 26, 27), Mr. Michael Graham studies the growth-rate of the Codling in the North Sea and surveys the literature on the subject of the age-determination in fish (Ministry of Agriculture and Fisheries. Fisheries Investigations. Series II. Vol. XI. 1928. *Studies of Age Determination in Fish.* Part I. "A Study of the Growth-rate of Codling (*Gadus callarius* L.) on the Inner Herring-trawling Ground." Part II. "A Survey of the Literature," H.M. Stationery Office, 1929. Price 6*s.* and 3*s.* 6*d.* respectively.) The object of this particular work was to determine the age at which codling first enter commercial catches, forming part of the general plan of research into the life-history of the North Sea Cod, study of fluctuations in the yield requiring such information. A large collection of scales was obtained for the purpose and their complete investigation is not yet finished, but much has been learnt from them, especially with regard to the value of the scale method for age determination. The author defines certain limitations without which he finds the method unreliable and difficult, and in order that the elimination of personal judgment may be effected all scale tracings used are reproduced in the appendix, together with the serial number and length of the fish. The principal data used are scale tracings and fish lengths. Otoliths were only examined from one sample. In his treatment of the scales Mr. Graham has already in his former paper (1926) described his method relating to the first winter ring. It is assumed (1) that the first narrow ring and second wide band as defined in this paper are the first winter ring and second summer ring respectively, and (2) that the fish used were developed from eggs liberated in the middle of March. The first assumption is based on the argument that such very small fish must be less than one year old or just over, according to the time of successive observations. The fish that had not experienced a winter showed no narrow ring, those which had passed a winter had one. This worked in 98 per cent. of cases. The second assumption takes the spawning season as restricted to the

time when it is at its height. Scale tracings from ten samples were taken, each sample being found to have one year class more abundantly represented than any other. For reasons given it is accepted in this paper that the majority of scales in the sample record the true age of the fish and there may be, and usually are, misleading scales which are always in the minority. Scale-length and fish-length are not in linear proportion because in very early stages the scales are much farther apart than in the older stages when they overlap, consequently a curve expressing their relationship will be much farther from a straight line in the older stages than in those of 34 centimetres and under. That the majority of the fish from the early samples were from identical stock can be shown by the agreement in the modal length without recourse to scales, and from the later samples by calculation using both scales and length. The growth-rate of the herring investigated is apparently intimately connected with sclerite width but not always related to temperature. The results enumerated in the present paper only apply to the first three years of the life of the codling, which is then immature, the conclusions being that the modal length of the codling in the Inner Herring-trawling Ground was 20 cms. at two years and 29 cms. at three years of age.

The method of determining age from scale reading is stated to be in the cod only applicable if there is a majority of fish of one year class in the sample, or, in the case of a mixed sample, when there is no apparent majority, if by the length-distributions there is justification in assuming that the apparent year classes are real. Even with these limitations it is an extremely useful method as the chance of an incorrect majority reading is small and the dominant year groups in any year can be determined.

Part II contains an important survey of literature pertaining to the subject, with summaries of all papers and critical observations.

Television in Colours (S. K. Lewer)

The step from television in monochrome—the orange-red colour of the neon lamp—to colour television would appear, at first sight, to require considerable complications in the apparatus and method employed. It is, however, a simple multiplication of the monochrome method in principle, but the apparatus is more costly, requires more care in adjustment, and more connecting channels between the transmitter and receiver.

A system of television in colours developed by the Bell Telephone Laboratories is described by Dr. H. E. Ives in the

July, 1929, issue of the *Bell Laboratories Record*. This method involves the same light sources, driving motors, scanning discs, and synchronising systems as are used in the monochromatic system, but the photoelectric cells, amplifiers, and glow-discharge lamps are triplicated. Briefly, in place of one set of television signals (which may be transmitted by wires or through the ether), there are three sets, each corresponding to one of the three basic colours, red, green, and blue. These are transmitted and received simultaneously by three sets of apparatus, the impressions being finally combined to form a coloured image. The outstanding contributions which have made this possible are a new photoelectric cell, new lamps for reproducing the image, and the equipment which is associated directly with them.

The photoelectric cells at the transmitter had to be improved so that they responded to the red end of the spectrum, instead of stopping at the blue-green region, while the glow-discharge lamps at the receiver had to be designed for producing green and blue light in addition to the original red. These problems at first presented considerable difficulty, and even now it is rather troublesome to obtain sufficient intensity from the blue and green reproducing lamps.

The new transmitting cell uses sodium in place of potassium and is made sensitive even to the deep red part of the spectrum by a complicated process using sulphur vapour and oxygen in place of the hydrogen glow-discharge. Three sets of these cells are used at the transmitter, each behind a gelatine colour filter, so that each set of cells gives the series of signals corresponding to one of the three basic colours, red, green, and blue. Only one scanning disc need be used to intercept the light reflected from the object to these colour cells, as in the monochrome arrangement. The number of cells in each colour-set, twenty-four in all, and their positions, are determined by the relative sensitiveness to different colours, and are arranged so that an equal electrical response is produced for each of the three colours. Three sets of amplifiers are required, one for each colour, and three communicating channels in place of one. At the receiving end the three connections lead to three sets of lamps which generate impulses of each of the three colours. The red impulses are produced by a neon lamp behind a red filter, while the green and blue impulses are separately produced by argon lamps behind green and blue filters respectively. The argon lamp, at present the best source of green and blue light, is much less efficient than the neon lamp, and it is necessary to use special amplifiers to operate it. The three beams of light passing through the filters are combined by a pair of semi-transparent mirrors,

and the resulting beam traverses the ordinary scanning disc and lens system, eventually being focussed into a small aperture, through which the observer sees the image in its true colours with the general appearance of a small coloured motion-picture. The image may be displayed to a large audience by the use of a grid consisting of three parallel argon and neon tubes.

In practice, it is found to be far more difficult to obtain a satisfactory transmission in colours, for any deviations from the correct tone rendering would throw out the balance of the colours, so that while the three images might be adjusted to give certain colours properly, others would suffer from excess or deficiency of certain of the constituents. Further, unequal distribution of the light to the transmitting cells would cause the object to appear as if illuminated by lights of different colours shining from different directions.

Although not introducing any new principles, the achievement of colour television is regarded as a definite further step in the electrical transmission of images. The reproduction of the scene in its true colours greatly enhances the appearance of reality, but there still remains, however, the problem of stereoscopic vision.

Malaria Control in the United States (R. R.)

Last year a Symposium on Malaria was held at the Conference of the National Malaria Committee at Asheville, North Carolina. Twenty-one articles are given in the published Report, which readers can probably obtain from the Northern Medical Association, Birmingham, Alabama (Vol. XII, No. 4, April 1929). Evidently mosquito control is now beginning to spread widely in the affected American States. The Americans should have less trouble in generalising mosquito control than is experienced by British enthusiasts in places occupied by indifferent coloured populations. For instance, we were recently told that malaria-control is almost impossible in British Guiana owing to the fact that the negroes will not believe in the mosquito theory at all.

Panama and Malaria (R. R.)

I have often said in my lectures that the Panama Canal was dug with a microscope; but my audience has seldom believed me, and the Americans especially seemed to think that the work of Gorgas and his associates (which was very good, new, and successful) was principally concerned in the making of the Canal. So it was in one sense, but the fact that mosquitoes carry malaria had been ascertained before the time of Gorgas. It is curious that the subject of malaria and mosquitoes should be so much falsified, sometimes for the

express purpose of gaining credit for institutions or persons. I suppose that there is no other scientific subject which has been so grossly misrepresented since it first entered the minds, or no-minds, of the public. The old saying that if you give a lie a short start, no truth in the world will ever overtake it, has been fully confirmed in the story of malaria. When I see absurd statements that the great Professor So-and-So is responsible for the discovery that malaria is carried by mosquitoes, I make no attempt to catch that nimble falsehood. "Let your lies go free, wherever they be," is an excellent saying. The following letter from Gorgas himself to me will probably be of interest in this connection:

HYDE PARK HOTEL, KNIGHTSBRIDGE,
LONDON, ENGLAND.
March 23, 1914.

MY DEAR SIR RONALD,

Before leaving England I wish to express to you the debt of gratitude we all feel to you for the great work you have done in the field of Tropical Medicine. As you are aware, malaria was the great disease that incapacitated the working forces at Panama before our day. If we had known no more about the sanitation of malaria than the French did, I do not think that we could have done any better than they did. Your discovery that the mosquito transferred the malaria parasite from man to man enabled us at Panama to hold in check this disease, and to eradicate it entirely from most points on the Isthmus where our forces were engaged. It seems to me not extreme, therefore, to say that it was your discovery of this fact that has enabled us to build the Canal at the Isthmus of Panama. As this is an expression of my personal opinion as to the great value your important work has been to us in sanitation at Panama, you are at liberty to use this letter in any way you please.

With kindest regards and best wishes, I remain,

Yours sincerely,
W. C. GORGAS.

SIR RONALD ROSS,
18 CAVENDISH SQUARE, LONDON, W.

Science and the Monkeymen (R. R.)

The *Saturday Review* of May 18 contained an amusing story regarding Einstein. Apparently, a month or two ago, the municipality of Berlin granted him a life-lease of a charming lake-side villa. After he had accepted it, they found that it would not be vacant for five years. The city authorities then offered him a building-site near Potsdam, but the appropriation of £1,000 required by the Municipality for this gift has

been so attacked by Nationalist Members of the Municipal Council that Prof. Einstein has now refused any gift at all. Nevertheless, the Germans show better in this matter than do the English: the former at least try to reward eminent scientific investigators, and often do so; but the British are quite below even this humble effort, and seem to think that life-long work in research can be done without any payment or reward. This is due probably to an intellectual level even below that of the Germans.

Notes and News

The honours list published on the occasion of the King's Birthday included the following awards to workers in science: *Knights*: Prof. H. C. H. Carpenter, of the Imperial College of Science; Mr. J. J. Ralph Jackson, Chief Veterinary Officer, Ministry of Agriculture and Fisheries; Prof. W. C. MacKenzie, of the National Museum of Australian Zoology; Dr. Chalmers Mitchell, Secretary of the Zoological Society; Prof. C. V. Raman, of the University of Calcutta. *C.S.I.*: Mr. J. H. Field, late Director-General of Observatories, India. *G.C.M.G.*: Sir John Cadman. *C.I.S.O.*: Mr. J. F. Halpin, Superintending Chemist, Government Chemists' Department. *G.B.E.*: Dame Helen Gwynne-Vaughan, Professor of Botany in the University of London; Sir A. M. Duckham, Director-General of Aircraft Production. *C.B.E.*: Capt. R. S. Rattray, Government Anthropologist, Gold Coast. *O.B.E.*: Mr. H. Brown, of the Plant and Animal Products Department, Imperial Institute; Dr. F. Dixey, Director of the Geological Survey, Nyasaland Protectorate; Mr. J. C. F. Fryer, of the Ministry of Agriculture and Fisheries Pathological Laboratory, Harpenden; Lieut.-Col. F. J. M. Stratton, Professor of Astrophysics in the University of Cambridge.

The National Academy of Sciences, Washington, has elected Prof. F. O. Bower Foreign Associate of the Academy, and has awarded the Agassiz medal for oceanography to Prof. S. Gardiner of the University of Cambridge.

The Kelvin Medal for 1929 has been awarded to M. André Blondel, Inspecteur général des Ponts et Chaussées, France, who is distinguished for his work on signalling apparatus of all kinds.

The Albert Medal of the Royal Society of Arts has been awarded to Sir Alfred Ewing. Sir Thomas Holland has been appointed to succeed Sir Alfred as Principal of the University of Edinburgh, and Mr. H. T. Tizard, Secretary to the Department of Scientific and Industrial Research, succeeds Sir Thomas Holland as Rector of the Imperial College of Science.

Prof. F. A. E. Crew has been elected Foreign Member of the Czecho-slovak Agricultural Academy.

We have noted with regret the announcements of the death of the following scientific men during the past quarter: Prof. H. Andoyer, astronomer; Mr. G. Birtwistle, mathematical physicist; Prof. T. W. Cave, of the South-Eastern Agricultural College, Wye; Prof. R. J. Harvey-Gibson, emeritus professor of Botany in the University of Liverpool; Prof. L. T. Hobhouse, sociologist; Prof. G. Kassner, of Münster, chemist; Prof. W. Küster, of Stüttgart, biochemist; Sir E. Ray Lankester; Dr. E. F. J. Love of Melbourne, physicist; Prof. C. Mouren of the Collège de France, organic chemist; Sir B. Spencer, F.R.S., of Melbourne, biologist; Mr. M. R. Oldfield Thomas, F.R.S., naturalist.

Sir Sydney F. Harmer has been elected President of the Linnean Society; Dr. W. H. Eccles of the Physical Society and Mr. C. Thurstan Holland of the Röntgen Society, all for the session 1929–30.

The Ramsay Memorial Fellowship Trustees have made the following awards of new Fellowships for the session 1929–30: A British Fellowship, tenable for two years, to Mr. O. H. Wansborough-Jones, B.A., for work at the University of Cambridge. A British Fellowship, tenable for one year only, to Mr. R. J. Phelps, B.Sc., for work at the University of Oxford. A Canadian Fellowship to Mr. L. M. Pidgeon, B.A., M.Sc., Ph.D., for work at the University of Oxford. A Japanese Fellowship to Prof. Y. Nagai, for work at University College, London. A Spanish Fellowship to Don Andres Leon y Maroto, for work at University College, London. A Swedish Fellowship to Mr. E. K. Troell, Phil. lic., for work at the Rothamsted Experimental Station, Harpenden. The Trustees have renewed the following Fellowships for the session 1929–30: Mr. H. Bienfait, Ph.D. (Netherland Fellowship), for work at the Imperial College of Science and Technology, London. Mr. Peter Maitland, B.Sc., Ph.D. (Glasgow Fellowship), for work at the University of Cambridge. Sir Robert Waley Cohen, K.B.E., has been appointed Vice-Chairman of the Trust, *vice* Sir John Brunner deceased, and the Hon. Henry Mond, M.P., has been appointed a Trustee.

New Fellowships have been awarded by the Salters' Institute to Mr. C. G. Eltenton, Cambridge; Mr. D. L. Hodge, Imperial College of Science, and Mr. L. C. Bannister, Liverpool and Cambridge.

Sir Frank Heath, lately Secretary to the Department of Scientific and Industrial Research, has been appointed Secretary to the Universities Bureau of the British Empire. Among its other activities this Bureau is responsible for the publication of the *Yearbook of the Universities of the Empire*. The volume for 1929 (G. Bell & Sons, London. Price *7s. 6d.* net)

contains the usual features, including a summary of the facilities available for research, scholarships, and research grants, which make it an indispensable reference book for those concerned with educational matters.

At a meeting of the Executive Committee of the Imperial Botanical Conference (1924), held in London on January 18 last, it was decided to arrange a short Imperial Botanical Conference to be held immediately before the International Botanical Congress in 1930. The Imperial Botanical Conference, which it is intended should last only one day, will meet in London on Friday, August 15, 1930, at the Imperial College of Science and Technology, South Kensington, S.W.7. The agenda will be purely of a business nature. The proposal to hold a further Imperial Botanical Conference in 1935, on lines similar to that held in 1924, will be discussed, and, if necessary, the appropriate organisation for convening the Conference will be arranged. Reports of the Committees which have dealt with the Resolutions of the 1924 Conference will be received. Any other business which it is desired to lay before the Conference should be communicated to the Hon. Secretary, Professor W. Brown, Imperial College of Science, South Kensington, London, S.W.7.

The increasing importance of the British Industries Fair is indicated by the growth of the number of exhibitors from 600 in 1915 to 2,500 in 1929, and the great demand for space. Next year the Fair will be held in the enlarged Olympia from February 17 to 28. The section devoted to scientific instruments will be located on the ground floor in the Main Hall.

The examination for the Lloyd's Register Scholarships of the Institute of Marine Engineers will be held on May 12 and 13, 1930, the last date for entry being March 7. The scholarships are intended to assist marine engineering students to obtain an advanced course of instruction in engineering subjects. Their value is £100 per annum, and they are tenable for three years at an approved University. The age limits are 18–23 years, and intending candidates must have served for at least two years in a commercial engineering workshop, and until the time of application must have been employed in engineering workshops, either ashore, afloat, or at college, with the intention of entering upon the profession of marine engineering. Full particulars may be obtained from the Secretary, Institute of Marine Engineers, 85, The Minories, London, E.C.3.

The proposals for reform of the British Patent System recently put forward by the British Science Guild have been reviewed by a Committee of the Bar Council, the members of

which included, amongst others, Sir Duncan Kerly, K.C. (Chairman), Mr. James Whitehead, K.C., the Hon. Stafford Cripps, K.C., and Mr. Trevor Watson. The views of this Committee, which were arrived at after minute examination of the Report of the British Science Guild, are of particular interest in view of the recent appointment by the Board of Trade of a departmental committee under the chairmanship of Sir Charles (formerly Lord Justice) Sargant, to consider the same subject.

The Bar Council warmly commend the Report of the British Science Guild, which was the work of a Committee whose Chairman was Dr. W. H. Eccles, F.R.S., and whose Honorary Secretary was Capt. C. W. Hume; they consider that most of the proposals of the Report are reasonable and likely to be useful. Commenting on some particular points, they express the view that a patent ought not to be invalidated by mere "Paper" anticipations—that is, by the publication many years ago of ideas which were still-born and have never become known to the trade. They agree that there should be stronger remedies against unwarranted threats by which persons wrongly claiming to possess patent rights may seek to prejudice rival manufacturers, and they also support the proposal that appeals from the decisions of the Comptroller of the Patent Office should be heard by a special judge in chambers instead of by the Attorney-General or Solicitor-General as at present. The only proposals made by the British Science Guild which elicit opposition from the Bar Council are those whereby the judicial duties of the Comptroller of Patents would be extended. They hold that he should not be empowered to act as a Court of first instance for the trial of infringement actions; but they propose, as alternative remedies for the admittedly high cost of patent actions, (*a*) arbitration by consent of the parties and (*b*) agreement between the parties whereby the issues to be tried in court should be restricted.

The Annual Report of the Guild contains the addresses delivered at a Public Meeting held at the Mansion House, on April 24, for the purpose of drawing attention to the developments of British chemical manufactures. Sir Frederick Keeble dealt with the nitrogen fertiliser industry, and stated that the output from the Billingham nitrate works, which was 50,000 tons in 1924, is expected to be 750,000 tons in 1930. There are at present 6,000 employees at these works; in two years' time it is anticipated that over 12,000 will be needed. Mr. A. B. Shearer of Courtauld's gave an account of the artificial silk (rayon) industry, which employs 25,000 persons directly and probably more than 300,000 altogether. The silk duties appear to have had a most beneficial effect. Since

1924 (the duties were introduced in 1925) the production has doubled (50 million pounds weight in 1928), while imports fell from 10 million to 2·9 million pounds. Mr. F. H. Carr dealt with synthetic drugs, mentioning the help afforded to sufferers from asthma and hay-fever by the new drug ephedrine, and the production of vitamin D by the irradiation of ergosterol. He stated that the inclusion of the manufacture of synthetic drugs in the list of safeguarded industries is giving powerful help to those engaged in the reconstruction of the drug industry in this country.

The Norman Lockyer Lecture for 1929 will be delivered by Sir W. Morley Fletcher, K.B.E., M.D., F.R.S., on Tuesday, November 19, at the Goldsmiths' Company Hall.

The directorate of National Bureau of Standards, U.S.A., takes considerable pains to enlighten the public about work of the Bureau, and encourages visitors during the summer months. A *Visitors' Manual*, descriptive of its topography, organisation and work, is issued and a scheduled trip through the laboratories is made every afternoon (excepting Saturdays) from June 1 to September 30. Another publication gives an account of the scientific and technical posts in the Bureau, stating the qualifications required for entrance and for promotion to the various grades and the salaries paid. Juniors are admitted by examination after graduation and are paid from \$2,000 to \$2,500 per year; rising from this are the assistant, associate, and full professional grades, with salaries ranging from \$2,600 to \$4,400. Above these are the higher professional grades with salaries up to \$9,000, in which vacancies are usually filled by promotion. Conditions of service do not appear to be too good. Normal hours are from 9 a.m. to 4.30 p.m., with half holidays on Saturdays during the four summer months only. Annual leave must not exceed thirty days, and during illness salary is paid for a period not in excess of thirty days annually. Cost of room and board (two meals per day) in Washington is stated to be from \$45 to \$55 per month. Rentals for houses from \$55 per month.

In a letter to *Nature* (August 3), Dr. L. Silberstein gives an account of the results of several computations of the radius of de Sitterian space-time. These results, which are in excellent agreement, give for the radius 5·7 million light years. It follows that the greatest possible distance between two points in this space is about 9 million light years. If this be true, the distances now attributed to the nebulæ are far too great.

Messrs Adam Hilger, Ltd., have prepared a brochure descriptive of the construction and use of Prof. Coker's photoelastic apparatus for determining the distribution of stress in structural and machine members. It is emphasised that

the apparatus is simple to use, requires no knowledge of optics and gives definite and accurate information of the direction and magnitude of stress in any specimen in the form of a plane section. A model of the structure to be investigated is cut out of celluloid sheet and is examined first in plane polarised light, and then in circularly polarised light. In plane polarised light, with analyser and polariser crossed, dark areas on the image of the model indicate regions when the principal stresses are parallel and perpendicular to the plane of polarisation. By means of the circularly polarised light and a strip of celluloid (cut from the same sheet as the model) arranged so that a known tension can be applied, the difference between the stresses can be found, and finally, by measuring changes in the thickness of the model when loaded, stresses themselves may be determined. The use of circularly polarised light alone enables the most highly stressed areas to be located, so that unduly weak portions of a member can be found very quickly.

What are, in effect, short abstracts of papers on food form the contents of a new bi-annual publication bearing the title *Index to the Literature of Food Investigation* (H.M. Stationery Office, Kingsway, London, price 2*s.* net), of which the first number has just been issued. The *Index* originated in a recommendation from the Imperial Agricultural Research Conference, 1927, to the effect that the Low Temperature Research Station of the Department of Scientific and Industrial Research at Cambridge should issue from time to time lists of elaborated titles of publications dealing with the preservation and transport of food. The first issue has been compiled by Miss A. E. Glennie, and covers the period up to 1929. Abstracts have been taken from eighty-two periodicals (in English, French, German, and Italian), and are arranged in fifteen sections (*e.g.* Meat, Pig-flesh, Eggs, Fats and Oils, Fruit and Vegetables, Theory of Canning, Mycology, etc.). The abstracts are numbered, starting from 1 in each section. This is rather inconvenient for reference purposes, and it would have been better to start from 1 in each issue of the *Index*. The abstracts are, as the title indicates, quite short, but nevertheless sufficient to indicate the scope of the paper or article referred to. The publication should prove most helpful to all interested in the scientific aspects of the food industry.

The *Second Annual Report* of the Council for Scientific and Industrial Research of the Commonwealth of Australia shows that the community obtained very good value for the £80,000 expended in the year 1927–8, but that a very much larger expenditure is necessary if the programmes suggested by various experts, who have visited Australia and drawn up schemes of work at the request of the Council, are to

be carried out. Thus Mr. A. J. Gibson of the Indian Forest Service, who has surveyed the forest areas, recommends the immediate establishment of an Australian Forest Products Laboratory involving an initial outlay of £49,000 and an annual maintenance of £19,000. The Radio Research Board needs £8,000 per annum for its programme; the investigation of cold storage problems requires a large sum, and a personnel not yet available, and Dr. Tillyard's recommendations concerning research in economic entomology, which is urgently needed, will make large demands both for personnel and laboratories. The Empire Marketing Board is providing very considerable assistance towards the entomological work (in the aggregate over £60,000), but it is clear that the agricultural industries in Australia are badly in need of the help which should follow increased expenditure on research.

It is interesting to note that having completed the work in progress the whole staff of the pulp and paper section (with the exception of one newly-appointed member) resigned to take up positions with the Tasmanian Paper Pty., Ltd. This company is spending £50,000 in verifying on a semi-commercial scale the results obtained in the Government Laboratory. Looking through the Report it is to be noted that a member of one committee is designated F.P.S.L. These letters are not used in scientific circles.

Fishery Investigations, Series II, Vol. xi, No. 4 (H.M. Stationery Office, price 2s. net), contains an account of the attempts made at the Lowestoft Fisheries Laboratory to find a satisfactory method for marketing round fish. This turns out to be an astonishingly difficult matter. The best method so far devised is to use two discs of ebonite, celluloid or silver, joined by a silver wire or pin inserted in the caudal peduncle—or dorsally; but it appears that not more than 10 per cent. of such marks are retained for a full year.

Dr. G. P. Bidder, in his Presidential Address to the Devonshire Association for the Advancement of Science in July last, gave a most interesting account of the circulation of matter from sea to land and back again, pointing out its importance to human life. The daily evaporation of water from the surface of the seas is about 9×10^{11} tons. From a third to a quarter of this falls on the land, giving an average of about 36 inches of rain per year for the whole surface of the earth. On the average, therefore, the whole contents of the oceans pass through the clouds once in 4,500 years, and falls on the land once in 16,000 years. As compared with the circulation of other forms of matter this period is very small. In 50,000 years the rivers will bring down as much calcium carbonate as there now is in the oceans; for it to appear as

land again the time required is of the order of 50 million years. The vegetation in the sea exercises an important control on the oxygen content of the atmosphere, and in the carbon dioxide circulation. There is a circulation of phosphorus from the deep waters of the ocean (which contain between one and two grains per ton—thirteen thousand years' accumulation from rivers) brought by ocean currents to the shallow waters, used by the vegetable plankton, which is eaten by small fish, and these by birds whose droppings give us guano. The circulation of salt is familiar and there is also a circulation of iron, copper, and even arsenic, of which lobsters and oysters contain larger quantities than is allowed to be sold in foodstuffs.

Miscellaneous Publication No. 92 of the Bureau of Standards contains the United States *Code for Protection against Lightning*, together with many interesting details about lightning itself. For personal protection the following procedure is recommended: (*a*) Do not remain out of doors during a thunderstorm unless it is necessary. Stay inside a building where it is dry, preferably away from fireplaces, stoves, and other metal objects. (*b*) If there is any choice of shelter, choose in the following order: (i) Large metal or metal-frame buildings. (ii) Dwellings or other buildings which are protected against lightning. (iii) Large unprotected buildings. (iv) Small unprotected buildings. (*c*) If remaining out of doors is unavoidable, keep away from (i) small sheds and shelters in an exposed position; (ii) isolated trees; (iii) wire fences; (iv) hilltops and wide open spaces. (*d*) Seek shelter in dense woods, a grove of trees, a cave, a depression in the ground, a deep valley, or the foot of a steep or overhanging cliff. The code for the protection of buildings and all kinds of miscellaneous property is very complete and anyone having a practical interest in the matter should consult the book which may be obtained from the U.S. Government Printing Office, Washington, D.C., price 25 cents. An Appendix contains an account of the origin, characteristics, and effects of lightning, together with a very complete bibliography. Some of the numerical data may be of interest. The voltage seems to be about 100,000 volts per foot, so that the initial potential difference for a flash one mile long is about 500 million volts. The average quantity of electricity passing is about 20 coulombs, so that the energy is of the order of 10^{10} joules. In severe storms covering a large area flashes may occur at intervals of one second or less, so that energy is being dissipated at the impressive rate of 10 million kilowatts. The duration of a single discharge (there are several discharges to each flash—as many as forty have been detected) is about ·00003 second, so that the maxi-

mum currents may be of the order of a hundred thousand amperes—certainly thousands to tens of thousands of amperes. (Laboratory experiments with artificial discharges have produced 10,000 amperes at 2 million volts.) It is obvious from these data that very considerable inductive effects with serious results may be produced, even in circuits for which the mutual inductance is very small. The temperature to which air is raised is probably not very great (*e.g.* 500°–1,000° C.), and the luminosity of the flash is most likely due to ionisation of the air by the strong electric field. The damage produced, *e.g.* in trees, is probably due to decomposition with the formation of gaseous products, although repulsion between electrons travelling through adjacent conductors (*e.g.* fibres) may produce very large disruptive forces.

The greater portion of the energy of a flash is dissipated as heat in the air, so that an abrupt and large increase of pressure is produced (two or more atmospheres). This is the cause of the crash which accompanies a near-by flash of lightning. The rumbling and bumping sounds are due to the long and irregular character of the source of sound together with echoes and reflections. The distance at which thunder can be heard does not usually exceed 15 miles, and is often less. Casualties due to lightning generally arise from secondary phenomena, such as side flashes and induced discharges. First-aid treatment is the same as that for any other electrical shock or for burns. In particular artificial respiration should be tried as quickly as possible.

We have received from the McGraw Hill Publishing Co. copies of their catalogues of books on mathematics, physics, and radio engineering. These include several books of outstanding merit, among which Richtmeyer's *Introduction to Modern Physics* is perhaps the most important. This book, based on courses of lectures given at Cornell University, contains what is probably the best account of the development of physics, from the earliest times to the present day, which has yet been written. Every serious student of physics should have access to a copy. The price, 25*s.* for 596 pages, is really moderate. Other books include Joffe's *Physics of Crystals*, based on lectures given at the University of California (15*s.* net), Loeb's *Kinetic Theory of Gases* (27*s.* 6*d.*); Steinmetz's *Four Lectures on Relativity* (10*s.*); Shapley's *Source Book of Astronomy*; Humphrey's *Physics of the Air* (Second Edition, 30*s.*); and van der Bijl's *The Thermionic Vacuum Tube*.

No. 19 of the *Alembic Club Reprints* is entitled *Foundations of the Theory of Dilute Solutions* (Gurney & Jackson, 33, Paternoster Row, price 2*s.* 6*d.* net). It contains J. H. van't Hoff's paper, " The Rôle of Osmotic Pressure in the Analogy between

Solutions and Gases," and Svante Arrhenius's paper, " On the Dissociation of Substances in Aqueous Solution," both published in the *Zeitschrift für physikalische Chemie* for 1887 (Vol. i, pp. 481–508 and pp. 631–648). The groundwork of the theories contained in these two papers was presented to the Swedish Academy of Sciences in 1885 (van't Hoff) and 1883 (Arrhenius), but the two papers selected for translation by the Alembic Club formed the real starting point of the modern theory of solution.

We omitted to mention on p. 112 of SCIENCE PROGRESS for July last that Sir Ronald Ross was awarded not only the Manson Medal of the Royal Society of Tropical Medicine on June 21, but also received the Triennial Gold Medal of the West London Medico-Chirurgical Society on the same evening.

ESSAY-REVIEW

SOME CURRENT CONFUSIONS IN BIOLOGY. By J. H. Woodger. Being a review of **Modern Biology,** by J. T. Cunningham, M.A. [Pp. xii + 244.] (London: Kegan Paul, Trench, Trübner & Co., 1928. Price 10*s.* 6*d.* net.) **Queer Fish,** by C. M. Yonge, D.Sc., Ph.D. [Pp. viii + 193.] (London: G. Routledge & Sons, 1928. Price 5*s.* net.) **Blue Blood in Animals,** by H. Monro Fox. [Pp. v + 205.] (London: G. Routledge & Sons, Ltd., 1928. Price 5*s.* net.)

These three books illustrate certain difficulties which still seem to be troubling biological thought and which appear to rest upon insufficient analysis of the notion of causation in its application to biological problems. Mr. Cunningham is chiefly concerned to combat various opinions current among writers on biological problems and to urge instead alternative speculations from the standpoint of what is commonly called Lamarckism. Some of his criticisms seem to be well founded, and this is not surprising when we consider into what a rut of orthodoxy biology has allowed itself to fall. But it may be doubted how far Mr. Cunningham's suggestions help to clarify the situation, and his difficulties seem to be traceable to his somewhat incautious use of the notion of "cause." The author remarks in his Preface that "It is sometimes said that there are no such things as cause and effect," and although it is not easy to discover what precisely he means by "cause" and "effect," there seems to be little doubt that it is the absence of clarity on this important point which is responsible for some of his difficulties. Mr. Cunningham seems to believe that a "cause" is an entity which is related to another entity called the "effect" in much the same way as are the two ends of a piece of string, so that whenever we have one we are bound to have the other. Moreover, it seems to be supposed that the "cause" *makes* or *produces* the "effect," quite independently, apparently, of anything else. If this is what is meant, then there certainly do not appear to be any such entities to be discovered in nature. Mr. Cunningham writes (p. 40) that "Biochemistry has not yet told us what makes the male and female gametes unite, what causes the chromosomes of the two to undergo reduction . . . we know some of the results, but not the

causes. . . ." Now there no doubt are situations in which it is useful or convenient to speak of some essential element in a process as the "cause" so long as we remember that it *is* only one element—the explosion of gunpowder on the application of a spark is a familiar example. But no one would suppose that, in a case of this kind, the spark "made" or "produced" the explosion, and that the gunpowder, humidity of the air, etc., had nothing to do with it. And yet there are many places in his book where Mr. Cunningham appears to fall into this error. In the chapter in which he explains what he means by an acquired character he certainly writes as though he believed that, in regard to certain characters, *e.g.* sunburn, these were "produced" or "caused" or "determined" by external stimuli alone, and, in regard to other characters, *e.g.* shape of comb in fowls, that these were "produced" by the hypothetical "genes" in the germ-cells of the animal, irrespectively of what is happening in its environment. And yet it seems perfectly plain that just as what happens in an explosion depends upon the gunpowder as well as on the spark, so what happens in an organism depends upon the kind of organism it is, as well as on the kind of environment it is living in. In the case of such a character as shape of comb in fowls it seems obviously impossible to pick out some one element in the complex situation and call it "cause" of which the shape is "effect." Consequently when we have to do with cases which do not belong to the same type as the explosion (as is often the case in biology) we need to be more than ordinarily on our guard against being misled by common-sense uses of the notion of causation. For at the best of times the terms "cause" and "effect" do but give emphasis to elements we have isolated in what appears always to be a continuous process. All that Mr. Cunningham seems to mean by an acquired character is one which appears after birth or hatching, when the organism begins its independent existence in the outer world, unprotected by the body of the parent or the shell of the egg. And he believes that these are "entirely due to" some external "stimulus" and he professes totally to reject the view that they depend partly upon the kind of organism involved. He mentions the development of corns on the hand after rowing as an example. Even if these consisted of pieces of the oar which became detached and adhered to the hand it would still hardly be correct to say that they were "entirely due to" the oar, but as this is not the case, since they result from active changes in the epidermis cells of the oarsman, it hardly seems possible to ignore the share of the organism in the result. And when we speak of the oars as the "cause" of the corns we certainly

do not *mean* by this expression that the oars are "entirely responsible" for them.

There is one passage (p. 106) from which it is clear that even Mr. Cunningham himself does not really believe in the doctrine he wishes to advocate. Referring to the development of cyclopian monsters in fish embryos developing in abnormal sea-water he writes: "The abnormalities produced are entirely due to the abnormal conditions applied, if the genetic factors in the embryos under experiment are the same as in embryos which develop normally." Now it is perfectly plain that if the first sentence of this passage expresses a true proposition, *i.e.* if it were the case that "the abnormalities produced are entirely due to the abnormal conditions applied" there would be no need whatever for the qualification in the second clause. If the "genetic factors" have nothing to do with the result there is no need for them to be the same in the two cases. And if it *is* important that they should be the same then it is *not* true that the result is "entirely due to the abnormal conditions applied."

It is also clear from other examples given that what has misled Mr. Cunningham is the fact that in order to discover causal dependences it is necessary to compare two or more cases. If a constant *difference* is discoverable between two cases and we find a constant difference in the environment, then the *difference* between the organisms concerned can be said to be "entirely due to" the environmental difference, but this does not entitle us to say that the *particular character* exhibited by an organism on a given occasion is "entirely due to" any particular characteristic of its environment. The reader will find in Mr. Cunningham's book useful brief accounts of many recent observations and experiments in biology. There are also speculations about such topics as how horses got their hoofs and cattle their horns, but as, from the nature of the case, such hypotheses cannot be put to any experimental test they do not attract as much attention now as they formerly did. There is a slip on p. 5, where the names of "Driesch and J. B. S. Haldane" are coupled as "two of the principal supporters of neo-vitalism." "J. B. S. Haldane" is doubtless a slip for "J. S. Haldane," but in any case it is erroneous to couple these names together, because Dr. J. S. Haldane expressly repudiates vitalism, and his views and those of Driesch are poles asunder.

The other two books are new additions to the *Science for You* series of essays in popular science, published by Messrs. G. Routledge & Sons, in lurid orange bindings with blue lettering. Dr. Yonge's book is a good five shillings' worth, containing a great deal of solid information about fisheries with

which the general public will do well to be acquainted. This occupies rather more than one-half of the book. The rest consists of odd essays on other biological topics—genetics, tissue-culture, regeneration, and ecology. There are passages in some of these essays which suggest that some of the biologists of the younger generation are actually beginning tentatively to regard organisms *as* organisms, *i.e.* as entities whose parts stand in organising relations to one another, so that what is going on in one part is not independent of what is going on in others, but rather that their properties depend upon these relations, and the persistence of the whole as one thing depends upon them. On p. 122 Dr. Yonge says: "Work on tissue culture has shown that the development of the different tissues of the body is not determined by something within the cells, but by the environment in which the cells find themselves. The unspecialised cells of the embryo become differentiated according to where they lie in relation to other parts of the developing organism." This is a great advance on the nineteenth century, but we notice the same difficulty about causation again, because we cannot now go to the opposite extreme and say that the behaviour of a "cell" is *not* "determined" by something within it *as well as* by its relations to others. In another place Dr. Yonge says: "Sex is determined by the factorial content of the individual, and so, according to the old view, it ought to be unchangeable; yet there are many instances of the sex of an animal being dependent upon its environment." If this *is* what the old view meant by "determined," then it was clearly founded upon erroneous notions about causation. And there are passages in Prof. Monro Fox's book which lend colour to this interpretation of the "old view." On p. 23 he writes: "Several independent lines of evidence have convinced biologists to-day that the sex of the offspring is usually determined from the very beginning. Whether it shall turn into a male or a female is settled from the moment of the union of egg with sperm. In spite of the fact that the early embryo has the rudiments both of male and female organs, yet its sex has already been decided from the moment of the fertilisation of the egg by the spermatozoon." If the expressions "determined," "settled," and "decided" are intended to mean that sex is from henceforth independent of anything else, then this view is clearly wrong, and Prof. Fox himself gives an example of this fact on p. 33, in describing the free-martin.

Dr. Yonge does not appear to adhere with sufficient tenacity to the "organic view" in other places of his book. On p. 128 he writes, with reference to "somatic" and "germ"

cells: "There is an absolute distinction between these two sorts of cells. . . . No germ-cell can become a somatic cell—they can all be removed without affecting the rest of the body—and no somatic cell a germ-cell." Such dogmatic assertions are more in harmony with the old way of thinking than with the one put forward above.

One wonders what impression is left upon the "general reader" by the seemingly contradictory opinions of the various experts who write for his enlightenment on the subject of "heredity." Dr. Yonge says: "The recognition of the influence of environment represents a great advance in biology, but its social implications are no less important." "Herein lies the biological justification of all schemes of social amelioration, and the final answer to all those who advance the old argument that any improvement in social conditions, by abolishing the struggle for existence and so rendering natural selection inoperative leads to an increase in the unfit, who would not have been able to survive under 'natural' conditions." In apparent contradiction to this we find the following assertions of another writer on this topic, for the general reader: "There can be no doubt that the environment is not of paramount importance in determining the characters of individuals, of societies, or of races. The forces of environment are but one system in the mechanism which produces the characters of mankind. Slowly but surely it is becoming recognised that all efforts to deal with the problem of modern society, especially all proposals for the improvement of the race, must include not only schemes for the improvement of education and the material aspects of civilised life, but that they must also include as a basic feature suggestions for the improvement of the hereditary constitution of the stock."[1] But according to Dr. Yonge, "it is impossible to control the factorial content of even the lowliest animal except for some few factors, and it is quite out of the question for so highly organised and so slowly breeding a creature as man."

The volume by Prof. Fox contains twelve essays of varying length describing recent investigations in animal behaviour, biochemistry of pigments, ecology, sex, endocrines, and other topics, in a breezy style, which should appeal to the general reader. Two of the best essays appear to be the fifth, containing a history of the isthmus of Suez and its canals and their faunas; and the ninth, entitled "Youth and Age," dealing with growth-curves and their interpretation. There is a tendency in places, as is so often the case in popular science, to "play to the gallery" by emphasising the more "sensational" speculations; for example, it seems a little premature

[1] F. A. E. Crew, *Heredity*, Benn, 1928, p. 73.

in a book of this kind to describe the claims that "plants can induce cell-multiplication in animals by their radiation and vice versa," since these claims have been very severely criticised.

The notions employed in describing the animal behaviour experiments are rather crude. In an account of the physiology of the membranous labyrinth we are told that "the brain knows, unconsciously of course, in which direction the head has moved." Anyone but a physiologist would understand that it is a contradiction in terms to speak of "knowing" unconsciously. *If* one is unconscious then one cannot be said to *know* anything, and it is difficult to understand what is meant by speaking of the brain as "knowing" anything, in spite of the popularity of these expressions. It is surprising how long it is taking for the lesson of behaviourism to sink into the understandings of students of animal behaviour. This point is also illustrated in the accounts of experiments on training fishes and insects to respond to different "wave-lengths." The author supposes that these experiments "prove" that these creatures "see" colours just as we do, and in some cases they are said to "see" colours that we do not see. But, of course, these experiments do nothing of the kind, and no such "proof" is possible from the nature of the case. On p. 160 it is written: "Since we cannot directly know the sensations of animals, we study their responses to sensations." But if we cannot know the "sensations of animals," how do we know they "have" them, and how can we study their responses to them? Clearly we can only study their responses to "light" of various "wave-lengths" and this tells us nothing about their "sensations." But Prof. Fox sometimes speaks of "seeing" light and sometimes of "seeing" colours, whereas all that we see are colours. How can we speak of "visible waves"? Surely it is the colours that are visible, the waves being inferred. It seems absurd to say that fishes "know the differences between red, yellow, green, and blue-violet," or that "all doubt is set aside as to the capacity of bees to distinguish colours." All we can say is that they react differently under different circumstances, some of which are correlated with the visible differences between the above-mentioned colours.

REVIEWS

MATHEMATICS

Leçons sur l'Intégration et la Recherche des Fonctions Primitives. By H. Lebesgue. Second edition. [Pp. 343.] (Paris: Gauthier-Villars, 1928. Price 60 f.)

The first edition of Prof. H. Lebesgue's book *Leçons sur l'Intégration*, together with his *Séries Trigonométriques*, which appeared in 1904 and 1906 respectively, introduced a new era in the study of functions of a real variable. The new start which this theory was given at the beginning of the present century may be compared to the invention of the theory of functions of a complex variable by Cauchy and others early in the nineteenth century. The ideas about the problems of Fourier series, and the representation of real functions by means of them, which had been forming slowly for many years, seem to have taken shape almost suddenly; and it was Prof. Lebesgue's new definition of the integral which was the most typical and central of these ideas, and which has now become an essential part of the equipment of every pure mathematician. Many mathematicians in the last twenty-five years have made their reputations by working out the problems of the new theory, and many more will doubtless do the same.

The book has now been rewritten and considerably enlarged. But the author has preserved its character of an exposition of general principles. He has not attempted to give any account of those operations of integral calculus, however useful, which do not throw any particular light on the general principles. The first nine chapters cover much the same ground as the whole of the first edition. They give a general account of the problem of the discovery of primitive functions, and of the construction of definite integrals, and of their main properties. There are numerous references to the writings of Borel, Baire, de la Vallée-Poussin, Young, and others, but this part of the work remains substantially the same as it was twenty-five years ago. Other writers have since done the same thing in other ways, but Prof. Lebesgue sees no reason greatly to modify his own method. Indeed, as a whole, it has the inevitability of the greatest work, and is expounded with an elegance and force which make it a pleasure to read. No book on this subject can be exactly easy, but Prof. Lebesgue does these things in such a way that you scarcely notice how difficult they would have been without his guidance. On the other hand, the extreme brevity of some of the proofs is rather deceptive; for example, in the proof of the convergence theorem on p. 125 the fact that $\mathfrak{m}$ [C(En)] tends to zero is not quite obvious.

There is one point where, in teaching students, we should still hesitate to follow the method of this book. The "Lebesgue chain," a process which involves a transfinite number of steps, is still one of the standard methods of procedure. This is specially mentioned and defended in the introduction, and at the end of the book there is a note of twenty-five pages on the subject of transfinite processes in general. We do not suggest that there is anything wrong with such processes, or feel competent to judge the question at all. But, in an exposition of the theory to beginners, it is (as Prof. Lebesgue says in the introduction) possible to avoid transfinite processes; and we

are conservative enough to prefer to do this. On the other hand, for the purpose of provoking real thought about the subject Prof. Lebesgue's methods are entirely admirable.

The new section of the book consists of a chapter on totalisation, or Denjoy integration, and a chapter on Stieltjes integrals. Totalisation, a process invented by Denjoy about 1915, permits us to integrate any function which is the differential coefficient of another function, a problem which is far more difficult than might be supposed. This part of the book is, so to speak, much more transfinite than the previous part, and we can offer the rigid conservative little hope of being able to solve the problem just mentioned. However, it is a logical development of the theory, and we are glad to find such an adequate and compact account of it here. The same may be said of the section on Stieltjes integration, an important development of the theory in another direction. In a sense this includes all the other methods of integration. The author suggests that in this direction important future developments may be expected.

E. C. T.

An Introduction to Linear Difference Equations. By P. M. Batchelder, Ph.D. [Pp. viii + 209.] (Cambridge, Mass.: Harvard University Press; London: Oxford University Press, 1927. Price 18*s*. net.)

To many English mathematicians the modern work on difference equations is almost entirely unknown, and this little book will be of service in reminding them of an attractive branch of analysis and one still very incompletely explored. Dr. Batchelder has written something intermediate between a text-book and an original monograph. His first two chapters develop the fundamental theory of linear difference equations and the special properties of equations of the first order. The Gamma Function, defined by its difference equation, is introduced and its well-known formulæ derived. The variables are, throughout, considered to be complex, and some of the analysis is not easy. These two chapters have sets of exercises appended. They are clearly written and, upon the whole, achieve their object of making the subject intelligible to a beginner who is well grounded in the ordinary theory of functions. Some preliminary work on differences and a rather less perfunctory treatment of finite integration might not, however, be out of place.

The rest of the book is concerned with the theory of the hypergeometric difference equation

$$(a_2x + b_2)\cdot y(x + 2) + (a_1x + b_1)\cdot y(x + 1) + (a_0 x + b_0)\cdot y(x) = 0.$$

The nature of the solutions depends upon those of the "characteristic equation"

$$a_2\rho^2 + a_1\rho + a_0 = 0$$

and much attention is given to the cases in which the values of ρ are equal or in which one or both become zero or infinite. Some part of this work is new, but the author does not make it clear which are his own contributions, nor does he give systematic references. The book is also without an index.

The publishers cannot be congratulated either upon their cover or upon their choice of type. But perhaps they believe mathematicians to be without interest in the æsthetics of book production.

R. C. J. Howland.

Sechsstellige Tafel der trigonometrischen Funktionen. By Prof. J. Peters. [Pp. viii + 293.] (Berlin: Ferd. Dümmle 1929. Price 48 M., bound 52M.)

All mathematicians who make more than a casual use of tables are already very much in Prof. Peters's debt. The present volume of six-figure tables,

by its admirably clear arrangement and excellent printing, will add to the reputation of both the author and his publisher. The six trigonometric functions, with their first differences, are tabulated in parallel columns for angles from 0° to 45° at intervals of 10 seconds. Decimal parts of the differences are tabulated in the margins. For angles from 0° to 1° 20′, the functions w'' cosec w (*i.e.* w in seconds × cosec w) and w'' cot w are also included. These functions vary with extreme slowness and enable a very exact determination of cosec w and cot w to be made. A preliminary table gives the values of cot w and cosec w for angles from 0° to 1° 20′ at intervals of 1 second.

R. C. J. Howland.

Standard Table of Square Roots. By L. M. Milne-Thomson, M.A., Assistant Professor of Mathematics in the Royal Naval College, Greenwich. [Pp. xii + 90.] (London: G. Bell & Sons, 1929. Price 7s. 6d. net.)

This book contains a differenced table of the square roots of numbers from 100·0 to 999·9 and from 1,000 to 10,000. The results have been worked to six places of decimals and rules are given for obtaining the roots of numbers of more than four figures. Prof. Milne-Thomson compiled the table for use in connection with his tables of inverse hyperbolic functions and of elliptic functions (shortly to be published). It was used in its manuscript form for obtaining square roots to fourteen figures. In its new garb it will have a wide sphere of usefulness, and thanks are due both to the author and to Messrs. Bell for the clearness of the print and excellence of the spacing.

W.

PHYSICS

Mathematical and Physical Papers. By Sir Joseph Larmor, Sc.D., F.R.S. [Two volumes. Pp. xii + 679; xxxii + 831.] (Cambridge: at the University Press, 1929. Price £6 6s. net.)

This collection comprises a large part, though not the whole, of the scientific writings of Sir Joseph Larmor, one of the most eminent mathematical physicists of his time, and a member of the distinguished band, in which Lorentz was another prominent worker, that carried electrical theory forward, from the point where Maxwell left it, to the development of the theory of relativity by Einstein. Unlike the ordinary scientific worker in some comparatively limited domain, he belongs to the company of those who seek earnestly, and with some success, to gain a deep understanding of physical science in all its wide range. His works reveal immense reading, and a profound knowledge of the history of the subjects he deals with. His concern with the foundations of physics, and with the evolution of its ideas, has been maintained to the present time, as is shown by the numerous appendices in which he describes his present outlook on the questions that have engaged his own attention in the past, and that remain central in modern physics. Though he himself has not taken much part in the twentieth-century developments, his familiarity with what others (particularly in this country) have done is continually in evidence in the notes with which this collection is enriched.

The two volumes contain more than a hundred papers, dealing mainly with electromagnetic theory, thermodynamics, and optics, though also with many other subjects. The first eighteen papers were published in the eighties; the next decade, represented by thirty-four papers, produced his three great memoirs on the dynamical theory of the electric and luminiferous medium, in which the electron theory was developed before the unitary electric corpuscles, the negative electrons, had been identified and measured by Sir J. J. Thomson; the author's important work on the Zeeman effect

also appeared during this period. The next two decades include the years in which he was Secretary of the Royal Society and Member of Parliament for his University; they are represented in these volumes by no less than forty-three papers, while he published much besides that is not reproduced here—including "Æther and Matter" (1900), certain papers on relativity, many encyclopædia articles, and a number of scientific biographical notices, especially one of considerable importance on Kelvin's life and work. Nine papers, and numerous critical appendices and notes, written during the preparation of these volumes, belong to the present decade.

The work of most scientific investigators probably consists either in the discovery of new facts, and (or) their interpretation by means of fairly simple theories based on established laws; matters of this kind are discussed in these volumes—for example, the ascent of sap in trees, the propagation of wireless waves, the sun's magnetic field, the irregular movement of the earth's axis—and they form the part of the author's work which will be found easiest to comprehend by most readers. But his most characteristic work relates to more general questions of great intrinsic difficulty, and readers able to follow him in these discussions will be relatively few; his allusive style will seem obscure to those who do not share his deep familiarity with the history and evolution of dynamics, optics, and electrical theory.

The author's attitude to present-day physics, as expressed in two prefaces and many appendices, is conservative. While conscious of the great achievements of modern spectral theory and the wave-mechanics, he considers it premature to conclude that the classical foundations of electric and optical science are undermined, and he believes that the usefulness of the nineteenth-century efforts to construct models of the substructure of the universe is not exhausted: "the original working electronic model, as directly compelled by a rotational æther, . . . may still—crude perhaps but within its range wholly concatenated—have claims to be retained in mind alongside other clues to consistent theoretical formulation" (II, xii). The future alone, with its expected store of new experimental discoveries, and its further changes and consolidations of theory, will show whether this moderately stated conclusion is justified; however this may be, those who in each scientific generation will shape the course of fundamental physical theory will be grateful for this collection of the works of a kindred mind.

S. Chapman.

of Geophysics, as applied to Explorations for Minerals, Oil, and Gas. By Dr. Richard Ambronn, Göttingen; translated by Margaret C. Cobb, M.A., Ph.D. First Edition. [Pp. xi + 372.] (London: McGraw-Hill Publishing Co., 1928. Price 25s. net.)

Interest in geophysical methods of prospecting is already widespread in America; it is beginning also to be aroused in England, although, as yet, comparatively little work on the subject has been done in this country. Most of the publications are in continental languages, mainly German, and translations into English are naturally welcome in English-speaking countries. This applies to Dr. Ambronn's book particularly, as his close association with various branches of geophysics is well known; and one begins to read the translation with the pleasing anticipation that here at last there will be revealed precisely how the physical indications obtained at the earth's surface are related to particular forms of underground structure. But in this fundamentally important respect the book is extremely disappointing. There are elaborate descriptions of instruments and long discourses on the physics of the earth as ordinarily understood by the term "geophysics"; but when one at last reaches the paragraphs devoted to the small-scale applications to mineral location vagueness is generally found to supervene, and the reader can learn but little. The author evidently recognises this, particularly

in relation to electrical methods of prospecting, and in more than one place lays the blame on what is described as necessary secrecy, imposed on the one hand by commercial considerations, and on the other—a curious reason—on account of patent rights. This may be so, and is most regrettable, since, besides leaving in doubt the validity of the method described, it hinders the present state of knowledge reaching those who might be qualified to contribute to progress in the subject.

The book, which is evidently the result of much careful investigation, is divided into two main sections, the first dealing with the methods based n physical effects to which the earth's constituents themselves naturally rise—variations of gravity and magnetic field, also radioactive effects. second section is devoted to methods in which a stimulus is imparted to the earth, and the distribution of its effects examined; this includes electrical methods in which electric currents are passed into or induced in the earth, and seismic methods in which the earth is shaken by artificial explosions and its resulting tremors measured in the neighbourhood. A very lengthy bibliography is appended, comprising some sixty pages, which is referred to by numbers in the text.

It is too evident that the present volume is a translation. The sentences often reveal their German origin by their ponderous form, and sometimes obscurity results. What, for example, is the meaning of (page 169) "an essentially wide-open amplitude of ellipses of the tensional vectors in the soil"? The proof-correcting has been somewhat careless, also; a good many misprints survive, such as "gaging" on page 170 and "gauning" on page 186, presumably both signifying "gauging."

Much progress has been made since this book was originally written rather hurriedly three years ago. Dr. Ambronn intimates in his preface that he contemplates dealing with the subject more completely at a later date. It is to be hoped that he will be able to incorporate those details whose absence has been here deplored, even if it should mean the exclusion of material of less practical importance. He would thus enhance very greatly the value of his book as a work of reference on geophysics as applied to mineral exploration.

A. O. Rankine.

The General Properties of Matter. By F. H. Newman, D.Sc., and V. H. L. Searle, M.Sc. [Pp. 388, with 113 illustrations.] (London: Ernest Benn. Price 25*s*. net.)

The average student approaches the subject of properties of matter with very little interest, and, unless that interest is carefully fostered, it very soon becomes difficult for him to apply himself seriously to the study of this branch of physics. Usually, cumbersome methods of mathematical treatment, and the inclusion of much unnecessary material, tend to make available textbooks disliked. Now, the authors of the book before us have not entirely avoided the error of including unnecessary material; for instance, it will appear to many readers that the chapter on gyroscopic motion is more fittingly placed in a textbook of applied mathematics. However, they do make a praiseworthy effort to include sections dealing with the more important modern developments of the subject. Thus we find descriptions of the quartz micro balance and the Eötvös balance, although it must be mentioned that more attention to the interpretation of the measurements made with an Eötvös balance would be helpful, even at the expense of the omission of some of the theoretical treatment. Again, we find descriptions of Dushman's molecular gauge and the principle of action of the diffusion pump.

One or two of the chapters are frankly disappointing, particularly that on

surface tension, where the description of the experimental portions of the na is decidedly weak. For instance, only Jaeger's method and the Rayleigh jet method of measurement of surface tension are actually described, although other possible methods of measurement are treated in the theoretical sections. The drop weight method, however unsatisfactory it may be, is not even mentioned.

A particularly good bibliography is appended to each chapter, and this feature will render the book a useful addition to the literature on this branch of physics, even if the average student continues to find the study of properties of matter a boring occupation.

L. F. B.

Probleme der Modernen Physik. By ARNOLD SOMMERFELD. Zum 60. Geburtstage gewidmet von seinen Schülern. Edited by P. DEBYE. [Pp. viii + 221, with 52 illustrations.) (Verlag S. Hirzel in Leipzig, 1928. Price 19.50 M., geb.)

THE fact that this book on modern physics is edited by P. Debye is sufficient to commend it to the notice of English readers. It consists mainly of a collection of papers contributed by prominent mathematical physicists who have worked with Arnold Sommerfeld and who wish to express in this pleasant manner their congratulations on his sixtieth birthday. There is a short article by Debye himself on time phenomena in electrolytic solutions, and among the remaining twenty-seven articles there are many which will be of great interest to English physicists. It is perhaps a little late to wish Prof. Sommerfeld "many happy returns," but at any rate we can express the hope that his seventieth birthday will see the publication of an equally pleasing testimonial to the inspiration which he has given to the mathematical physicists of our time.

L. F. B.

Laboratory Physics: A short course. By H. W. HECKSTALL-SMITH, M.A., and B. A. FLETCHER, B.Sc. [Pp. vii + 224, with 97 diagrams.] (Oxford: at the Clarendon Press, 1928. Price 4*s*. 6*d*. net.)

THIS book—which "is intended to be used, in conjunction with any standard theoretical textbook, for all experimental work up to the standard of the Higher Certificate Examination"—is printed in an attractive type, on excellent paper, and is bound in the style which we have come to associate with the School Science textbooks issued by the Clarendon Press. But nothing more can be said in commendation of it. I do not remember ever before having read a book in which the final revision and the proof-correcting have been done so carelessly. There are, for example, points requiring correction on pages 14, 40, 106, 107, 145, 151, 152, 153, 165, 166, 167, 189, 190, 193, and Figures 54, 63, 72, 78, 81, 88, 92, and 95 require attention.

The slip-shod style of the book may be judged from the following points. On pages 8 and 9 instructions are given for carrying out the balancing columns method for measuring the coefficient of real expansion of a liquid; the students are invited to work out the result by answering a series of leading questions, but the authors have worded these questions in such a way that they will succeed in misleading the students as to the real significance of the method. On page 24 specific heat is so defined that it is dependent upon the gramme and the degree Centigrade. It is to be inferred from a paragraph on page 151 that the E.M.F. of a Weston cell is greater than that of a dry cell. Experiments with vibration magnetometers are discussed without any reference to the amplitude of the vibration. On page 116 the symbol for a cell is shown with the longer stroke marked as the positive, but when Figure 70 on page 153 is examined, it appears, from the description of the circuit,

that the shorter stroke denotes the positive pole of the cell; further, the circuit diagram for use in the investigation of the thermocouple gives no indication whatever as to the direction of the current.

There is yet another feature to which exception must be taken; in several cases the reader is required to read on one page, and at the same time to refer to a diagram two pages ahead; this is most objectionable.

VIVIAN T. SAUNDERS.

Physics for Medical Students. By SIDNEY RUSS, D.Sc., F.Inst.P. [Pp. 230 + 130, with diagrams.] (Edinburgh: E. & S. Livingstone, 1928. Price 10s. 6d. net.)

THIS is a book written for a special purpose, and it must be read and considered with reference to the gap in educational literature to fill which it has been written.

Although at first sight it may appear to the reader that in each of the sections—Mechanics, Heat, Light, Sound, Electrostatics, Magnetism, Current Electricity, Electromagnetism, X-rays and Radio-activity—a start, *ab initio*, as for beginners, is made, yet closer reading shows that the language and the method of presentation used assume quite definitely a general knowledge of the subject-matter, and a faculty for the application of thought and reasoning, which is to be found only in students who are of the age of seventeen years or more. For such students the style and method of treatment appear to be admirable.

The author has found it necessary to cover a very wide field, and for that reason, in some places the discussion is a little thin, and in others the connection between the subject-matter of successive paragraphs is not always obvious; but, on the whole, it must be said that the matter is well selected and clearly and precisely explained.

There are no questions or numerical examples to be worked out by the student, but the author in the text gives a number of fully-worked-out examples, illustrating the application of the principles under discussion.

Details of laboratory experiments are very rightly omitted; each laboratory has its own particular sets of apparatus and series of experiments, and it is impossible to write general experimental instructions which will be of any value.

The price of the book and its general style may militate against its use in schools, but there is no doubt that it is a first-rate book for the purpose for which it has been written.

The publishers are to be congratulated on the excellent form in which the book has been produced.

V. T. S.

Conduction of Electricity through Gases. By Sir J. J. THOMSON, O.M., F.R.S., and G. P. THOMSON. [Pp. 491.] (Cambridge: at the University Press, 1928. Price 25s. net.)

THE first edition of the well-known *Conduction of Electricity through Gases* appeared in 1903; it was followed by a second edition in 1906, and the book has long been out of print. In the preparation of the third edition Sir J. J. Thomson has had the co-operation of his son, Prof. G. P. Thomson, and the enormous development of the subject has made it necessary to publish the work in two volumes, the first of which is now before us.

Like its predecessors, the new work is full of detailed accounts of experimental work; in fact, the book appears to be mainly designed for the specialist, for the average reader is likely to find the amount of detail rather oppressive. The first volume is concerned with the general properties of ions and ionisation by heat and light. Commencing with the properties of a gas in the normal

and in the conducting state, the authors pass on to discuss the mobility of gaseous ions. Here, they fittingly devote considerable attention to the recent work of Tyndall and his collaborators. They then elaborate the mathematical theory of the conduction of electricity through gases and discuss the effect produced by a magnetic field on the motion of the ions. Their treatment of the determination of the ratio of the charge to the mass of an ion includes a description of the mass spectrograph and Aston's packing fraction curves. After describing the measurement of the charge on an ion, the authors discuss in detail the physical properties of ions as revealed by Wilson expansion chamber methods. The remainder of the volume is devoted to the treatment of thermionics, ionisation of the gases from flames, and photo-electric effects.

L. F. B.

Müller-Pouillets Lehrbuch der Physik. 11. Auflage. Erster Band.
Erster Teil—Mechanik punktformiger Massen und starrer Körper. [Pp. xvi + 860.]
Zweiter Teil—Elastizität und Mechanik der Flüssigkeiten und Gase. [Pp. viii + 1258.]
Dritter Teil—Akustik. [Pp. xii + 484.]
(Braunschweig: Vieweg & Sohn A.G., 1929. Price: Erster und Zweiter Teile, R.M. 83, Dritter Teil, R.M. 32, geb.)

The appearance of the first volume of the eleventh German edition of *Müller-Pouillets Lehrbuch der Physik* is particularly important to many physicists who have learnt to rely on the previous editions, of which the last appeared over ten years ago. It is particularly interesting, because, with the passage of time, the articles on mechanics and sound had become somewhat old-fashioned, and in the latest edition these have been completely rewritten. The books retain their characteristic accuracy, whilst giving a very readable account of a wide range of physics.

The new volume is in three parts, and it will certainly be a matter of regret that the first two parts cannot be purchased separately. Part I deals with the mechanics of particles and of rigid bodies, and is edited by E. Waetzmann. It opens with an introductory section on the concepts and methods of physics, by G. Mie, followed by an article on fundamental physical measurements of length, mass and time, by G. Berndt. H. Diesselhorst contributes the sections on physical units and dimensions and on the mechanics of particles. E. Madelung and W. Thomas are responsible for the section on Newton's laws of motion, whilst the mechanics of rigid bodies are treated by W. Hort, a special section on gyroscopic motion and its cosmic and technical applications being supplied by M. Schuler.

Part II deals with the elasticity and mechanics of liquids and gases. The mechanics of non-rigid bodies are treated by Th. Pöschl, who supplies excellent articles on the deformation and fracture of metal specimens and on the friction of solid bodies. P. P. Ewald writes on the mechanical structure of solid bodies in the light of our present knowledge of the constitution of the atom, and in the light of our knowledge of the behaviour of single crystals. The great feature of the book, however, is the beautifully illustrated article on the motion of fluids and gases, by M. Prandtl. This writer has attained such distinction in this branch of physics that his article will naturally be of great interest to a large number of persons engaged on problems in aerodynamics, and many of them will undoubtedly wish it were possible to purchase the article separately. The book closes with articles on the measurement of specific gravity and on apparatus for the measurements of pressure.

Part III, which may be purchased separately, is a very up-to-date account of acoustics. It contains descriptions of all important new developments of technique and of acoustics of buildings. The fundamentals of sound are

treated by P. Cermak, whilst A. Kalähne deals with the theory of hearing and the production of musical notes. Kalähne is also responsible for an excellent section on the reception and reproduction of sound, including loud speakers, and E. Meyer contributes an article on the measurement of intensity of sound. The propagation of sound and allied phenomena such as interference and the acoustics of buildings are discussed by E. Waetzmann, J. Friese, and K. Schuster.

It merely remains for us to state that the whole work is excellently written and completely up to date, and that references to original papers are everywhere freely given and excellent indexes are supplied. As was mentioned, in discussing other volumes of this edition which appeared a short time ago, the references have distinctly a bias towards articles printed in German, but as the work is primarily designed for German readers, this bias is excusable, particularly in a work which does not pretend to possess the completeness of a "Handbuch." We thoroughly recommend these books to the attention of all teachers of physics.

E. G. R.

CHEMISTRY

Nitroglycerine and Nitroglycerine Explosives. By Phokion Naoum, Ph.D. Authorised English Translation, with notes and additions, by E. M. Symmes. The World-wide Chemical Translation Series, edited by Prof. J. Emmet Reid. [Pp. xi + 469, illustrated.] (London: Baillière, Tindall & Cox, 1928. Price 31*s*. 6*d*. net.)

This is the first volume of a series of works on chemistry, in part original and in part translations into English, of foreign works of outstanding merit, under the general editorship of Prof. Emmet Reid of Johns Hopkins University. In the present case it was felt that a real service would be rendered to English-speaking chemists if Dr. Naoum's standard work on the subject of nitroglycerine explosives could be translated, since the only other work on the subject, that of Escales, is already some twenty years old, and it must be admitted that even where one can read German with relative ease it is none the less preferable to be able to read up a subject in one's own language, if possible.

The story of the development of the nitroglycerine industry is somewhat remarkable. Nitroglycerine was discovered by Sobrero in Turin in 1847, but remained a mere laboratory curiosity until the Swedish engineer, Alfred Nobel, took the matter up and started a small factory near Stockholm in 1862. The so-called "blasting oil" was a great improvement upon black powder in many respects, but, needless to say, it was a tricky business dealing with such a dangerously explosive liquid and an explosion in 1864 destroyed the original factory, killed Nobel's youngest brother and the chemist Hertzmann, and fatally injured the father, Emmanuel Nobel. In spite of this, however, Alfred Nobel obtained further financial assistance, built factories near Hamburg and Stockholm, and began to supply the world. Later, as a result of the accidental leakage of nitroglycerine into some diatomaceous earth used for packing the cans, it was found that this "Kieselguhr" could absorb about three times its weight of nitroglycerine to produce a cheesy, plastic mass which was less sensitive to shock than pure nitroglycerine, and thus was born the world-famous explosive "Dynamite." The immense demand for this product led to the erection of factories in all parts of the Old and the New Worlds, and still later Nobel made the surprising discovery that, instead of the inert kieselguhr, nitrated cellulose could be used, giving a safe product termed "blasting gelatine," which in very slightly modified form is still in use to-day.

Further, Nobel found that by increasing the amount of nitro-cellulose

and diminishing that of nitroglycerine he could obtain a solid horn-like product, Ballistite, the predecessor of all modern propellant explosives such as Cordite, which have helped to revolutionise ballistics and the methods of warfare on land and sea.

Nobel died at San Remo in 1896, leaving as a legacy the many bequests, such as the well-known Peace Prize, with which his name is associated.

At the present day nitroglycerine and its products are made in enormous amounts in Government and private factories all over the world, and an authoritative book on the whole subject of its manufacture such as Dr. Naoum's is very welcome. The translation is well done, though here and there some Teutonic expressions have been left; the notes inserted by the translator refer in most cases to American practice, but they will no doubt be of use also to British readers as well. Author and translator have collaborated to produce an excellent and up-to-date work on the preparation and uses of nitroglycerine, and related explosive products, which will without doubt be the standard work for many years, and should find a place in the library of all chemists and firms connected with the manufacture of explosives.

F. A. MASON.

Artificial Silk. By ING. DR. FRANZ REINTHALER, enlarged and revised edition, translated by Prof. F. M. ROWE, D.Sc., F.I.C. [Pp. xii + 276, illustrated.] (London: Chapman & Hall, 1928. Price 21*s*. net.)

ONE of the surprises of the Great War, at all events to the average citizen, and even to "Brass Hats" and others who should have known better, was the close interdependence between peace products and munitions of war.

In the metallurgical industries, of course, the connection was sufficiently obvious, but in such matters as dyes, drugs, and high explosives the relationship was neither seen nor understood until it was discovered that the fate of the whole Allied cause lay on the ability of organic chemists and the dyestuff factories to produce the necessary war products.

In a similar manner the very rapid developments of military aviation called for the speedy production of immense quantities of soluble cellulose derivatives, chiefly the acetates, for the "dope" required for aeroplane fabrics. As a result the chemists engaged in the young industry of "artificial silk" were able to produce in a very short time the necessary cellulose derivatives, and immense new factories were built to manufacture the large quantities required.

With the coming of peace these works were in due course altered to produce various forms of artificial silk for which there was an immediate and rapidly increasing demand which continues yet unabated.

As is so often the case, practice here is now considerably ahead of theory, and we are therefore doubly indebted to Dr. Reinthaler and Prof. Rowe for the timely issue of the present volume upon the subject. As the authors themselves point out, no attempt has been made to write an exhaustive treatise, but the book, whilst covering all the important aspects of the subject, will be well within the range of many readers who have relatively little special scientific training, but who are concerned with the utilisation of the products and who need to know something of the process of manufacture, and the special difficulties met with therein.

The book deals with the preparation of "Art Silk," or "Rayon," as we must now call it, by the Chardonnet, Cuprammonium, Viscose, and Acetate processes, with their testing and dyeing, and with various other points connected with this young and flourishing branch of chemical industry.

The large number of diagrams of special machinery and the excellent microphotographs help to enhance the value of the volume and both the authors

deserve well of their fellow chemists for having succeeded in condensing the important facts connected with the subject of artificial silk in so limited a space, yet without omitting anything of importance. F. A. MASON.

Kraemer's Scientific and Applied Pharmacognosy. Third edition, revised by EDWIN NEWCOMB, formerly Professor of Pharmaceutical Botany at the University of Minnesota, and by Profs. DARBAKER, EARL FISCHER and EDMUND GATHERCOAL. [Pp. xxxvii + 893.] (New York: John Wiley & Sons; London: Chapman & Hall, 1928. Price 37*s*. 6*d*. net.)

THE present is the third edition of Kraemer's well-known treatise of Pharmacognosy, which, since the author's death, had been thoroughly revised and brought up to date by an editorial committee to each member of which some special duty has been assigned. It is stated in the book that the pharmacognosist of the future should have as a foundation a thorough laboratory instruction in botany, chemistry, physics, and crystallography, an opinion to which all who study the subject of pharmacognosy seriously will subscribe; on the other hand, no amount of scientific training will take the place of a real interest in the subject.

It may be said at once that this book should go far towards creating and fostering such interest in the student; the book is well set up and profusely illustrated with many very useful plates as well as a proportion of somewhat conventional diagrams of microscopic sections. The subject-matter follows a botanical classification commencing with Thallophytes and proceeding *via* the Archegionates—to which only eight pages are devoted—to Spermophytes, which of course occupy the bulk of the book; these are followed by short sections on Animal Drugs, Powdered Drugs, and a Key for the Identification of Powders; this latter is here published for the first time, and it is claimed for the scheme that it has been subjected to several years of actual test in the laboratory. A further new feature is the inclusion in each monograph of a paragraph dealing with the uses and dose of each drug; for, while it is acknowledged that it is not the function of the pharmacist to prescribe, it is submitted that he must know the dose and general action of each drug. This is sound logic, and it will be admitted that the inclusion of such information adds a living interest to the subject. With so much that is good it would perhaps appear ungracious to find fault, but it may perhaps be suggested that the information regarding the chemical composition or constituents of the drugs is on the whole rather meagre, and not always quite accurate; thus, for example, the statement that the ash of *Chondrus* consists of calcium oxalate and compounds of sodium, potassium, magnesium, and calcium with chlorine, iodine, bromine, and sulphur can hardly go unchallenged; or, again, the work of King renders inaccurate the statement that the toxic principle of *Amanita muscaria* has been prepared synthetically by the oxidation of choline. Again, it is hardly correct to state that the chief interest of the lichens is in the colouring matters which they contain, since the colouring matters referred to are not contained in the lichens, as such, any more than *Indigofera* spp. contain indigo, but are produced from these materials by subjecting them to chemical action. These are, however, but minor blemishes in a really attractive book, which is full of good things for those who delve in its pages. P. H.

Inorganic Chemical Technology. By W. L. BADGER and E. M. BAKER. [Pp. viii + 228, with illustrations.] (London: McGraw-Hill Publishing Co., 1928. Price 12*s*. 6*d*. net.)

THE student of chemical engineering is concerned primarily with those factors which influence the design and operation of chemical plant. To him,

every chemical process is made up of a number of definite unit operations, such as dissolving, crystallising, evaporating, distilling, drying, etc. Unlike the student of applied chemistry, he is not so much concerned with definite processes and products as with the means by which these processes are most efficiently operated to obtain the products most profitably in the most suitable form.

With the exception of Levy's well-known book, *An Introduction to Industrial Chemistry*, there is practically no textbook published in this country which deals with chemical plant and processes from this chemical engineering standpoint. On this account, the excellent series of books on chemical engineering subjects, published in America, is all the more welcome.

Inorganic Chemical Technology, by Messrs. Badger & Baker, maintains the high standard set by the earlier books in this series. The essential facts of the more important processes of the heavy chemical industry are described and discussed fully from the chemical engineering standpoint. After a short introductory chapter, there are eight chapters devoted to the following subjects: common salt, sulphuric acid, nitric acid, minor acids, sodium carbonate, caustic soda, chlorine and bleaching powder, and miscellaneous products. There is an appendix of useful tables of properties of the more important materials.

Very clear diagrams of plant and flow sheets for different processes are given. The physico-chemical principles upon which the design of different plants and the operation of the different processes are based are clearly stated. At the end of each chapter, there is an excellent list of references to the literature, and a number of suggestive and stimulating problems for the student to work out.

The economics of the different processes and the various factors affecting the cost of operation are not discussed; this is unfortunate, since, in many cases, it is necessary to sacrifice actual process efficiency in order to make the process more profitable.

The book is written by Americans, and is based primarily upon American experience; this in no way detracts from the value of the book to English readers, but explains the statement, for example, that direct-fired pans are obsolete in the salt industry. This book can confidently be recommended to all who require a clear and critical account of the more important processes of the modern heavy chemical industry.

W. E. GIBBS.

Die Adsorption. By OTTO BLÜH and NANDOR STARK. [Pp. 136, with 30 figures.] (Braunschweig: Friedr. Vieweg & Sohn Akt.-Ges., 1929. Price 7.75 M.)

THE subject of adsorption is one which is very difficult to handle in a way that will satisfy both the experimentalist and the theorist. In this case the latter will be better satisfied than the former, although there are some unfortunate gaps in the theoretical presentation. These are most numerous in the last part, where surface-tension phenomena are treated. Szyszkowski's empirical equation connecting the surface-tension and concentration of aqueous solutions of organic substances is given and a table showing a close agreement for butyric acid. The authors mention that the agreement in other cases is not so good, but do not give any account of recent advances in this direction, which have come from kinetic consideration. In their treatment of the kinetic theory of surface films no mention is made of recent illuminating work of Adam and others.

The authors treat the theories of gas adsorption at solid surfaces in considerable detail. Accounts are given of the theories of Euken, Polanyi, Langmuir, Lennard-Jones, Lorenz and Lande, Henry, Williams, Ilun, Frenkel,

and Jaquet. The presentation is not as critical as might be desired, neither do the authors succeed in making a connected theme.

R. K. SCHOFIELD.

Alkaline Accumulators. By J. T. CRENNELL, B.A., and F. M. LEA, M.Sc., A.I.C. [Pp. x + 132, with 24 figures.] (London: Longmans, Green & Co., 1928. Price 10s. 6d.)

THIS book is difficult to place. The fact that an attempt is made to introduce the reader to the elementary principles of electrolysis can only mean that the authors hope their work will prove attractive to readers whose knowledge of electro-chemistry is so slight as to justify the enunciation of Faraday's Laws. Yet it seems hardly likely that many such folk will be found who will pay half a guinea for a book on alkaline accumulators. The considerable space devoted to "theoretical" discussion is out of all proportion to the relevant information it contains, and readers with a knowledge of physical chemistry, and who hope to find something novel and instructive, may be disappointed. The last seven pages, in which the relative merits of lead and alkaline accumulators are set out, undoubtedly afford a valuable guide to anyone desirous of selecting a battery best suited to any specific purpose. Scattered through the former 120 pages are other pieces of information on the construction, operation, and maintenance of alkaline cells, together with details of some early patents. But we cannot help thinking that a practical man will be disinclined to glean his information from a half-guinea book.

R. K. SCHOFIELD.

Inorganic Quantitative Analysis. By HAROLD A. FALES, Ph.D., Associate Professor of Chemistry at Columbia University. [Pp. xii + 493, with 49 figures and tables in appendix.] (London: G. Bell & Sons, 1928. Price 12s. 6d.)

IN preparing this work, the author has aimed at incorporating in a course of instruction in analytical chemistry more discussion of the physico-chemical principles involved than is to be found in other similar textbooks. The book is built round a skeleton of thirty-nine laboratory exercises. These start with calibration of weights and burettes and the preparation of standard solutions gravimetrically and volumetrically. Then follow examples of the estimation of the common elements and radicals, leading finally to the analyses of ores and alloys. The order of treatment is such that a student working through the course will acquire progressive insight into the theory underlying the analyst's trade, as well as technical knowledge and skill.

The book divides itself into four sections, with a little more than one hundred pages in each. The first is devoted to a general discussion of apparatus, reagents, manipulation, precision, weighing, and volumetric measurement. The next treats first of neutralisation and then solubility product: the author considers this the logical order.

Then follow oxidation and reduction and electrolytic determinations; the theoretical presentation here is exceptionally clear. Finally, the student is introduced to the systematic analysis of ores and alloys. The table on "Percentage Ionisation" in the appendix is woefully out of date, but this is doubtless due to the fact that, although only recently published in this country, this book has been in circulation in America for several years.

The book is undoubtedly a valuable contribution to pedagogic literature. While primarily designed as a training in analysis, it is likely to be helpful to many teachers who find laboratory exercises based on analytical methods very helpful as illustrations of physico-chemical methods. The publishers are to be congratulated on the reasonable price.

R. K. SCHOFIELD.

Colloid Symposium Monograph. Papers presented at the Sixth Symposium on Colloid Chemistry. [Pp. 341.] (New York: Chemical Catalog Co., 1928. Price $6.50.)

PERUSAL of each successive volume of *Colloid Symposium Monograph* has become an annual treat to those interested in these matters. In this, the sixth volume, there are fewer papers on general aspects of the subject than formerly and a marked increase in those on colloidal studies arising out of biology. Roughly half the papers are on technical subjects.

By far the most arresting communication is that by Sir William Hardy, the guest of honour of this Symposium, entitled *Living Matter*. We are tempted to make quotations, but the difficulty is to choose: the whole paper is so excellent. Scientists of all kinds, no matter what their special studies may have been, should read it. It concerns them all. Discarding the mechanistic theory of life as being entirely without foundation, and pointing out in passing that it helps in no way to call living matter colloid, he applied himself to the statement of the properties which distinguish living from dead matter. It is immensely satisfactory to see how, refusing to be led away by preconceived ideas, he builds his reasoning on the bedrock of direct observation. In conclusion, he expresses the belief that biology now stands in urgent need of real physicists. Apart from its intrinsic excellence, Sir William's paper will be appreciated by physicists on account of the absence of jargon used by lesser biologists as a cloak to ignorance. Sir William's object is to be candid about ignorance.

Papers were communicated by: W. D. Harkins, D. R. Briggs, F. B. Kendrick, J. W. McBain, M. E. Laing, W. D. Bancroft, E. F. Burton, A. J. Stamm, P. J. Moloney, H. A. Abramson, S. Mudd, A. Baird Hastings, H. Wasteneys, J. C. Krantz, Jr., E. A. Hauser, H. L. Trumbull, G. S. Whitby, J. R. Fanselow, F. Olsen, S. E. Sheppard, H. N. Holmes, J. B. Nichols, H. A. Neville, and H. B. Weiser.

R. K. SCHOFIELD.

Volumetric Analysis. By A. W. WELLINGS, B.Sc. [Pp. xi + 196.] (London: Methuen & Co., 1928. Price 5s.)

THIS book is designed for the use of pupils in the higher forms of Secondary Schools, and of junior students in Universities. It is a most useful and comprehensive compendium of volumetric exercises, treated in an elementary manner; many of the estimations described probably lie in advance of the average syllabus for which it is primarily written. At the end of each chapter a number of variants on the exercises described are suggested in the form of questions. Chapters on the general principles of volumetric analysis and on the theory of indicators are included. Mr. Wellings' book should be useful not only to students but also to teachers as a handy source for instructive alternatives and supplements to the usual time-honoured experiments. Two slips have been noticed; on page 110, lines 16 and 19, the interrelations of silver, chloride, and chromate ions are clearly wrongly described; on page 128 the neutralisation of residual hydrochloric acid in the barium chloride will not destroy the chloride of the acid and an error will thus be introduced. There seems to be no reason for selecting the method of emptying a pipette described on page 11, instead of Stott's accurate and simple method. The reviewer draws attention to these points because the book is a good one, and in the future editions which it merits any errors or weaknesses should be corrected. The book is pleasantly "Dedicated to the boy who wrote 'An indicator is a beautiful thing.'"

L. F. GILBERT.

Chemistry for School Certificate Examinations. By W. F. F. SHEARCROFT, B.Sc., A.I.C. [Pp. viii + 368, with 61 figures.] (London: G. Bell & Sons, 1928. Price 4*s*. 6*d*.)

MR. SHEARCROFT says in the Preface that "Although, most definitely, the matter has been selected with the aid of the syllabuses and examination papers of the eight recognised First School Examination Authorities, the treatment is not that of a cram work, designed solely to push pupils through an examination." The reviewer agrees with Mr. Shearcroft's remarks concerning the treatment of the subject-matter. A marked attempt has been made to generalise and classify as far as possible the facts and theories presented, in simple and forcible language. The author makes a real effort to interest junior students whilst teaching them: he takes them into his confidence—there is nothing esoteric about this book. Here is a different treatment of the subject from that which obtained at one time; one's own school chemistry books were usually compounded, apparently, by evaporating to a suitable volume Ostwald, Gmelin-Kraut, and a few out-of-date works on manufacturing processes. In other words, they were more suitable for elementary reference books than as connected efforts to teach chemistry. As far as the reviewer is aware Mr. Holmyard's *Inorganic Chemistry* was the first serious attempt to break away from the old traditions. That reminds one that Mr. Holmyard is the General Editor of the Series (Bell's Natural Science Series) in which Mr. Shearcroft's book is published. If all the volumes in that Series are as well written, Messrs. Bell are doing a great service for elementary education.

L. F. GILBERT.

The Problem of Fermentation. By M. SCHOEN. Translated by H. LLOYD HIND. [Pp. x + 211.] (London: Chapman & Hall, 1928. Price 21*s*. net.)

WHILST the subject-matter of this book is of use chiefly to those working on yeast fermentation, the handling of the subject by Dr. Schoen gives it an appeal to a considerably wider circle. Indeed it may be read with sustained interest by any biologist with a taste for chemistry: the literary style, at any rate, of Mr. Hind's translation, is well above the average for a purely scientific work. The intending reader should, however, be forewarned that the author has an axe to grind, and his presentation, while argued with fairness, cannot be said to be impartial or complete. The book is no less valuable for this, but the nature of its appeal is necessarily different from that of a textbook.

The author seeks first to prove that modern research on the subject confirms completely and without qualification the judgments and predictions of the great Pasteur—an attempt which, whether successful or not, seems superfluous. He then develops the thesis that pyruvic acid is the "keystone" of fermentation processes, dismissing as relatively unimportant the part played by phosphates. His rejection of yeast juice studies as artificial and misleading (pp. 21 *et seq.*) seems a little summary. There follows a rapid survey of some characteristic bacterial fermentations, and the author draws certain conclusions as to the general nature of fermentation mechanisms. The last chapter, which was added especially for the English translation, deals with current theories of biological oxidation and reduction as applied to fermentation phenomena.

The reference to p_H 7·7 as the "normal" reaction of the blood (p. 116) is presumably a misprint; but on the whole the printing is both accurate and clear. The bibliographical index at the end contains some four hundred references, and there is a useful subject index in addition.

The book may be recommended without reserve to anybody whose interest in fermentation studies is more than superficial.

P. E.

The Colloid Chemistry of Protoplasm. By L. V. HEILBRUNN. (Protoplasma-Monographien. Vol. I. [Pp. vii + 356.] (Berlin: Gebrüder Borntraeger, 1928. Price 19 Rm., bound 21 Rm.)

THIS book is a gallant attempt to weld into a single whole the diverse contributions that have been made to our knowledge of protoplasm as a chemical system—mainly studies of unicellular organisms. Unfortunately, in his attempt to include everybody's work, good, bad, and indifferent, the author has, we think, expanded the book to unnecessary proportions and made of it rather tedious reading. Moreover, despite his critical attitude to much of the original work quoted, the author occasionally passes without comment work of obviously doubtful value. On page 28 we find quoted an analysis of a certain type of protoplasm tabulated under twelve heads, each group of substances measured to one part in ten thousand and including a group of "unknown substances" (5·87 per cent.) put in to make the total right! Again, some measurements of the density of certain granules in sea-urchin eggs are quoted on page 75 to six significant figures: it is doubtful whether the density of a complete hen's egg could be measured so accurately.

After a critical survey of methods of studying the physical properties of the protoplasm of living cells, the author collects the facts concerning the action of physical and chemical agents on unicellular organisms (Chapters V to XII). There follows (in four chapters) a description of certain functions of cells—cytolysis, vacuole formation, cell-division, irritability, contractility, and so forth, in terms of protoplasmic activity, and the author develops in some detail the theory that all these activities are akin in their ultimate mechanism, being based on that primitive and universal property of protoplasm, reversible sol-gel transformation.

P. E.

The Fuel of Life. By JOHN JAMES RICKARD MACLEOD. [Pp. ix + 147.] (London: Oxford University Press, 1928. Price 11*s*. 6*d*. net.)

THE name of Prof. Macleod is associated with one of two schools of thought regarding the value of fat as a source of energy in mammalian metabolism. Graham Lusk, and with him many other distinguished physiologists, regard it as certain that fat can be, and normally is, metabolised direct in the animal body, and is as convenient a source of energy as any. He is opposed by others of no less distinction who consider it highly improbable that fat can be utilised without preliminary conversion to carbohydrate—the fuel of life.

In the book under review Prof. Macleod states the case of the latter school. "According to our view neither protein nor fat is burned by the muscles until it has been converted into carbohydrate or some related substance." The first of the four chapters deals with measurements of respiration, chiefly of muscle preparations; the second with the nature of blood sugar, and the evidence obtained by the study of it in normal and diabetic animals; the third treats in more detail of the diabetic organism; and the final chapter is devoted to a consideration of the few simple substances which, on one ground or another, have been considered possible intermediates in the catabolism of carbohydrate.

The author has drawn much upon recent work in developing his theme, making the book an excellent review of contemporary research.

P. E.

Colloid Chemistry. Volume II. Edited by JEROME ALEXANDER. [Pp. 1029.] (New York: the Chemical Catalog Company, 1928. Price $15.50.)

WE feel that this volume was hardly necessary. Several of the sections (each, as in the first volume, the work of a specialist) can only be resumed under the main title by a barely justifiable extension of its meaning. We refer particularly to such sections as "Dusts, Fumes, and Smoke and their

Relation to Health," "Colorimetry," "Changes in Blood Concentration and their Significance in the Systematic Treatment of Cases of Extensive Superficial Burns," to note only three. Nor do we think the scientific value of the book is enhanced by the following quite typical passage from "the Colloidal State in Living Organisms" (contributed by A. Lumière): "Is it not remarkable that neither physiologists nor physicians have heretofore taken note of the eminently curious fact that diseases produced by the most diverse pathogenic agents show the same symptoms? . . . Our colloidal theory of life and disease enables us readily to understand this community of symptoms. All these pathogenic agents, however they may differ, have one property in common: they determine the flocculation of certain humoral colloids whose coagulation produces the pathological symptoms."

However, more than half of the fifty-seven sections in this volume are unquestionably valuable expositions of applied colloid chemistry in the fields of biology and medicine. The book makes good reading, and should be excellent for reference, thanks to full author and subject indexes; but it would have lost nothing of its value by the omission of several contributions.

The high standard of printing and binding found in the first volume is maintained.

P. E.

Organic Laboratory Methods. By the late Prof. LASSAR-COHN. Authorised Translation from the General Part of the Fifth Revised Edition by RALPH E. OESPER, Ph.D. [Pp. xii + 469.] (London: Baillière, Tindall, and Cox, 1928. Price 30*s*. net.)

THE late Prof. Lassar-Cohn distinguished himself by his painstaking attention to general operations and laboratory technique in organic chemistry, and it may be said that his writings on the subject have materially contributed to the care which his countrymen have bestowed upon the details of such processes as nitration, sulphonation, and organic reactions generally.

In organic chemistry the reactants are usually complex compounds whose molecules are composed of many atoms of, perhaps, several elements. Consequently, it is rare for one reaction to occur to the exclusion of all other possible changes. Almost invariably the major product is accompanied by a number of other resultants, whilst one or more of the reactants can remain, in part, unchanged. Time, temperature, and other factors play an important rôle in determining the course of such interactions. Important as the study of the conditions and methods of organic chemistry undoubtedly is, there are few works dealing with the subject with the exception of the material so laboriously amassed by Prof. Lassar-Cohn. In the present volume he begins by emphasising the importance of starting an investigation with pure materials. The various methods and adaptation of distillation and the choice and types of apparatus occupy two lengthy chapters. Decolourising, filtering, crystallising, subliming, and drying, all form the subjects of chapters scattered through the book, whilst the physico-chemical methods of m.p., b.p., and molecular weight determinations, and ultimate analysis are discussed in equal detail from the standpoint of practical technique. Detailed as it is, the work does not claim to be complete. The field is too large and new methods are appearing every day. The book is intended for the experienced organic chemist, to acquaint him with an outline of the general methods and to indicate how typical difficulties have been overcome and to arouse resourcefulness in his own work. In preparing the English (or rather American) edition, four distinguished men of science have collaborated. The translation is the work of Prof. Oesper and has been edited by Roger Adams and Hans T. Clark, and the book itself is No. 2 of the World Wide Chemical Translation Series, edited by Prof. E. E. Reed.

J. G. F. DRUCE.

GEOLOGY

The Geology of Petroleum and Natural Gas. By E. R. Lilley, Sc.D. [Pp. x + 524, 173 figures.] (London: Chapman & Hall, 1928. Price 30s. net.)

The literature of petroleum geology has reached unmanageable proportions, and it is necessary from time to time to supply succinct statements of facts and principles in the form of textbooks. This book has been written to give to individuals whose work brings them into contact with geological problems in this field, a single volume in which they may find brief statements of principles, and examples illustrative of their application.

An introductory chapter leads to two chapters on the properties of petroleum and mineral bitumens, followed immediately by a chapter on the origin of petroleum and related substances. Then come chapters on the geology of petroleum, indicating the limiting conditions for its occurrence and its distribution in the various geological formations. The nature of the reservoir rocks and the concentration of oil and gas therein, the modes of representation, nature, and development of oil field structures, are dealt with in the next five chapters. Six succeeding chapters discuss various types of structural conditions and oil container-rocks, illustrated, as might be expected, chiefly by American examples, although Persia, Roumania, etc., are not entirely neglected. The book closes with a chapter describing exploratory and prospecting methods in new areas.

The author favours the view that the kerogen of oil shales is intermediate between the original, entombed, organic débris, and the oil; and that petroleum results from chemical and physical changes in kerogen brought about by geological forces.

The book is valuable for its concise descriptions of little-known American oil-fields, and for its summaries of recent experimental work on the movements of oil in rocks relative to varying porosity, water-content, and capillarity.

The illustrations are, in general, good and well-selected, but some of the photographic reproductions of maps and sections are on so small a scale as to try the eyes badly. Misprints are few, and there are good author and subject indexes. On a complete reading the book strikes us as eminently well-written, and as adequately fulfilling its declared purpose.

G. W. T.

Geological Maps: Their History and Development, with Special Reference to Wales. By F. J. North, D.Sc., F.G.S. [Pp. vi + 140. 12 plates, 16 figures.] (Cardiff: National Museum of Wales, and Press Board of the University of Wales, 1928. Price 1s.)

This is the third of a series of publications by the Keeper of the Department of Geology in the National Museum of Wales, designed to provide simple accounts of geological principles as illustrated by the rocks of Wales. This little work on geological maps fully maintains the excellent standard set up by the two earlier monographs on Slate and Coal respectively.

The book is divided into three parts: I. The Evolution of the Geological Map; II. The History of Geological Mapping in Wales; and III. Lists of the Geological Maps of Wales. The first part gives a succinct description of the methods of compilation of a geological map, followed by an historical account of the development of stratigraphical ideas. The second part contains a most interesting history of geological mapping in Wales from William Smith to recent workers. Part III will certainly prove of immense use to research workers and others who may desire to know what Welsh areas have already been mapped and described on modern lines, and what fields there are still to conquer. The book ends with an excellent

colour-printed geological map of Wales showing the distribution of the chief systems and formations. This work is published at the remarkable price of one shilling, and is a tribute to the enterprise of the Welsh Museum authorities.

G. W. T.

The Geology of Venezuela and Trinidad. By R. A. LIDDLE. [Pp. xxxix + 552, 83 plates, 22 text figures, 1 map in pocket.] (Fort Worth, Texas: J. P. MacGowan, 1928. Price 30*s.* net.)

As we are told in the Preface, the object of this work is to outline systematically what is known of the geology of the north-eastern part of the Spanish Main of romantic renown, the region in which the fabled El Dorado was fruitlessly searched for, but where also, in sober reality, the rich gold-mines of El Callao, and the silver ores of Aroa, were discovered. Of recent years a new source of mineral wealth in petroleum and asphalt has been exploited, and the author of this work has been in the forefront of geological exploration of the country in the search after oil.

The book is divided into two parts, the first, amounting to 414 pages, dealing with Venezuela; the second, of 87 pages, treating of Trinidad. In the Venezuelan part physiographic details occupy the first nine chapters, the remaining seven chapters dealing with the geology. The treatment is very full; the Pre-Palæozoic rocks of the Guayana Shield, the igneous rocks, the Palæozoic, Mesozoic, and Cenozoic sediments occupying long chapters. As the Mesozoic and Cenozoic sediments contain the oil-bearing strata they naturally come in for specially detailed description. The structural and economic geology of the region are each dealt with in some detail. Under the latter heading asphalt and petroleum naturally occupy the most space. An immense amount of geological material, both first-hand and second-hand, has been assembled in these pages, and is well illustrated by a large number of photographic plates, sections, and diagrams. A similar plan of treatment is utilised in the section on the much better known region of Trinidad.

A comprehensive bibliography and a very full index are provided, as well as a good but uncoloured geological map (in pocket) on which correlation tables are given. On the whole the book is well produced and printed; but a catastrophic error in the assembling of the manuscript and the subsequent proof-reading, resulting in the misplacement of major headings, is insufficiently remedied by an erratum slip, which fails to correct the error in the List of Contents.

This book is a worthy addition to the long recent list of comprehensive works on regional geology.

G. W. T.

Elements of Optical Mineralogy: An Introduction to Microscopical Petrography. Part 1. Principles and Methods. Third Edition, revised and enlarged. By A. N. WINCHELL. [Pp. viii + 238, 260 figures.] (New York: J. Wiley & Sons; London: Chapman & Hall, Ltd., 1928. Price 17*s.* 6*d.* net.)

THE third edition of Prof. Winchell's justly popular textbook on the optical methods used in mineralogical and petrographical work has been revised and enlarged. The additional matter, amounting to 22 pages, has mostly been appended in the form of a final chapter, which deals with the use of the Federof universal stage, and with new dispersion methods of measuring the refractive indices of mineral grains. The grains are immersed in liquids the index of refraction of which can be varied by changing its temperature, or by a modification of the wave-length of the light used, until the refractive index of the grains is exactly matched by that of the liquid. These methods are said to be exact to within $\pm \cdot 001$.

Various minor changes and improvements are scattered throughout the book, but its format and binding are entirely unchanged. We regard this book as the best to put into the hands of our more advanced students when studying optical methods in mineralogy and petrology.

G. W. T.

Handbuch der Regionalen Geologie. VII. Band, 7a Abteilung. The Union of South Africa. By A. W. ROGERS, A. L. HALL, P. A. WAGNER, and S. H. HAUGHTON. [Pp. 232, 53 figures, and 3 plates.] (Heidelberg: Carl Winters Universitätsbuchhandlung, 1929. Price 17 RM.)

THIS detailed work on the geology of the Union of South Africa comes to hand in addition to the recent massive volumes of Du Toit and Krenkel, which, however, cover a somewhat wider field, and are not confined to the political division of the Union. This work is severely practical, aiming at giving the relevant facts in as small a compass as possible consistent with the scheme of presentation adopted by the Regionalen Geologie. As it is written by four members, present and past, of the Geological Survey of South Africa, it also presents the authoritative Survey point of view on the outstanding questions of South African geology. Nevertheless, the interpretative aspect is always kept in subordination to the presentation of the actual facts.

Dr. A. W. Rogers starts off with two good chapters on the morphology and geological structure, and the geological history of South Africa, respectively. The bulk of the book, however, is occupied by the third chapter—that on Stratigraphy and Igneous Rocks—in which the material is dealt with in chronological order, the various sections being written by the author best qualified to do them. It is valuable and instructive, for example, to have Dr. Hall's account of the Bushveld Igneous Complex in addition to those of Du Toit and Krenkel.

The fourth and final chapter deals with economic geology, and has been written by Dr. Wagner. This is a remarkably good piece of work. The sections on the Witwatersrand Goldfields, the copper deposits of Namaqualand, the vast Transvaal platinum deposits (the extent of which Wagner declares is not even now appreciated), and the diamond fields are lucid and most instructive. Dr. Wagner states that the view of the Witwatersrand gold as an original constituent of the banket or conglomerate cannot be seriously questioned after the work of Mellor; but on this view it is necessary to assume that the gold has been dissolved and redeposited practically *in situ*.

The book is well illustrated with maps and sections. A full table of contents does not quite make up, however, for the absence of an index. The number of loose leaves in the book, which is paper-covered, is somewhat annoying.

G. W. T.

Stratigraphical Palæontology. By E. NEAVERSON, D.Sc., F.G.S. [Pp. xiii + 525, 64 figures illustrating about 500 different fossils.] (London: Macmillan & Co., Ltd., 1928. Price 18*s*. net.)

THERE are many books on stratigraphy and palæontology, considered separately; but there are few books in any language which combine the two subjects into an organic whole. And since the greatest service that palæontology renders to geology is in the realm of stratigraphy the lack of such books is very serious for both students and teachers. Hence the book under review fills a real gap in English geological literature, and Dr. Neaverson is to be commended for his enterprise, no less than for the skill and knowledge which have gone to the making of this work.

The first part of the book, entitled General Considerations, contains an introductory chapter on such topics as principles of correlation, evolution, and stratigraphy, and means of identification of fossils; which is followed by chapters on the morphological features of the chief fossil groups, the preservation and occurrence of fossils, faunas in relation to habitat, geographical distribution and migration, and fossils as indices of horizon. This part is, on the whole, so well balanced that it takes a petrographer to suggest that somewhat more extended treatment might have been given to the relation between the fossils and the lithology of the rocks.

The second part, which, like the first, is only headed as such in the Table of Contents, not where it begins in the text, deals in detail with the faunas and floras of the geological systems in Great Britain. Extensive use has been made in these chapters of the most recent palæontological work, and further problems for investigation have been indicated. Subjoined to each chapter is a list of the principal literature, and the fossils are illustrated by photographs and drawings which, if they may not satisfy the meticulous palæontologist in every detail, will nevertheless convey to students the general aspects of the organisms figured.

The compilation of material so widely dispersed in the literature as that digested in this work cannot fail to be extremely valuable to students and teachers in the geological schools of this country. The book is well arranged, well written, and well illustrated.

While there are full lists of genera and species, and of stratigraphical names, we think that a more general index, especially of the material in the first part of the book, might have been provided.

On the whole this is an admirable work, unique in British geological literature, and it should find a place in the working library of all geological teachers and students.

G. W. T.

ZOOLOGY

The History of Biology. A Survey. By ERIK NORDENSKIÖLD. Translated from the Swedish by L. B. EYRE. [Pp. xii + 629, with an index of xv pp. and 32 illustrations.] (London: Kegan Paul, Trench, Trübner & Co., Ltd., 1929. Price 25*s*. net.)

SINCE the time of Linné, and even before, there has always been an intellectual sympathy between British and Swedish biologists, and the present volume will serve to strengthen the bond. It is based upon a course of lectures given by the author in 1916–17 at the University of Helsingfors and published in three volumes in 1920–24 as *Biologins Historia*. In spite of its general interest and utility it has not been available to the English-speaking reader until the issue of the present translation. On the whole the translation is very well done, but throughout there are suggestions that the translator is not thoroughly conversant with the technical terminology of biology. For example, we find "natural research" instead of research in natural science (pp. 89 and 129 *et al.*); "calculus" instead of calculation (p. 117); "cystic" for hepatic portal (pp. 147, 370); "osseans" for teleosts (pp. 210, 606 *et al.*); "centre" for middle piece (p. 536), etc. On p. 419 also the translation of *kjökkenmöddinger* by "refuse heaps" instead of the obvious kitchen middens is surprising. It should be added, however, that these are only minor flaws which in most cases do not obscure the meaning or interfere seriously with the easy flow of the translation.

The theme of the treatise is based upon the ideational rather than upon the practical aspects of biology, and the author is more concerned with the theories that lead to important lines of investigation than with the actual facts that the researches may have revealed. On the whole the author has chosen extremely well, and has given a very fair estimate of the value of the

different theories. It is only natural perhaps that there is more room for divergence of opinion in the discussion of theories than there could be in the discussion of facts, and also that it is more easy to agree to the relative importance of theories the more remote, within limits, they are. Thus as the lectures were delivered in 1916–17 we find a tendency to over-estimate the importance of Mendelism, Pure lines, etc., which then promised, or should we say threatened, to solve all the problems confronting the biologist. Perhaps the same lectures written to-day would deal with these questions a little more critically. Not all biologists will be content to think of Darwin as a dilettante amateur who established facts of second-rate value, and who produced a hypothesis based on weak foundations. Nor will they all agree that his theory of natural selection is condemned and his theory of the origin of species long ago abandoned. The author does admit, apparently with astonishment, and does his best to explain by the fortuitous occurrence of a number of happy and fortunate conditions, political and otherwise, the stupendous effect of Darwin's work upon the intellectual world. Indeed, we feel that here we have the chink in the otherwise remarkably strong and well-constructed armour of the author—he has hardly done justice to the genius of Charles Darwin. In a minor point also he does not seem to be aware of Waller's analysis of the claims of Bell and Magendie to the discovery of the functions of the dorsal and ventral nerve roots, but has adopted the older and less critical estimate.

These points apart, we have nothing but praise for the masterly handling of the enormous mass of material that fills these 616 closely printed pages. We can say that we have read them all with the utmost pleasure, and, while venturing to differ from the author on certain matters, nevertheless consider that he has made a contribution of real merit to the history of Biology.

C. H. O'D.

Trails of the Hunted. By James L. Clark. [Pp. xiii + 269, with 47 plates.] (London: Chatto & Windus, 1929. Price 21*s.* net.)

Since the mere slaughtering of game for the sake of killing has now fortunately fallen into disfavour, there are two points of view from which a book of the present sort might be written. The first is that of the man who must hunt, and does so with moderation, and hands over his trophies to some museum, not infrequently adding many useful facts about the animals themselves. The second is that of the man who, on behalf of an institution, deliberately sets out with the intention of providing a satisfactory group of animals of a particular kind which shall reproduce, in as faithful and lasting a manner as possible, the impression that such creatures produce in their natural environment. The latter will naturally appeal most to the scientist, and it is to illustrate this ideal, its pursuit and its difficulties, that the present volume is written.

However much we may regret it, and however we may strive to postpone the evil day by the provision of reserves, it is evident, as the author points out, that with the advance of civilisation many of the large "game" animals will die out. In order that future generations may be able to appreciate what these striking beasts were like they will have to rely on museum exhibits. Fortunately, the cinematograph can record much of interest and importance, but it can never replace the impressions of well-mounted specimens.

The author is particularly well qualified for his task both in training and experience. In the field he has had the good fortune of working with Radclyffe Dugmore, Cherrie Kearton, and Carl Akeley. He is also an animal sculptor of no mean merit and in this also he has had the opportunity of working with Akeley. At the present time he is working in the American Museum of National History, which institution, as is well known, has taken a lead in the preparation of magnificent exhibition cases.

The book in itself is well written and beautifully illustrated. It gives accounts of collecting in America, Asia, and Africa, together with many observations on the lives and habits of the various animals encountered in their native haunts. The paths of the "museum man" are not always smooth, and one can readily imagine that being captured and tortured by Mongols is not an encouraging experience. One of the author's observations will come as a surprise to those not acquainted with wild creatures at home, and that is that in any herd it is almost always an old female and not a male that takes the lead. The work can be thoroughly recommended, and reflects credit upon both author and publisher.

C. H. O'D.

Bacteriology. A Textbook of Micro-organisms. By Fred Wilbur Tanner. [Pp. xviii + 548, with 138 figures in text and 2 plates.] (New York: John Wiley & Sons; London: Chapman & Hall, 1928. Price 22*s*. 6*d*. net.)

Prof. Tanner's new volume is an expansion of his work *The Bacteriology and Mycology of Foods*, published in 1919. The practical part of this revision has recently been issued, and reviewed in this Journal, as "Practical Bacteriology." The book cannot, for purposes of review, be conveniently divided into larger sections than its thirty-three chapters. It is designed to give students just approaching the subject a mental grounding in most of our contacts with bacteria, whether these be regarded as aids or enemies, but chiefly in their connection with questions of public health. There are many additions and improvements which justify this volume being considered a new work rather than a second edition, which it does not purport to be, of the earlier work. There is, for example, a chapter (IV) with forty-four illustrations, on the morphology of bacteria; a sixty-page chapter (VI) on classification, and a chapter (II) on their systematic relation to the other biological groups; while chapters on the bacterial cell (III), its chemical composition (V), one on industrial fermentations (Ch. XXIII) and one on bacterial plant diseases (Ch. XXXIII) make up a tale of seven new chapters giving essential facts which were previously either only touched upon or absent. Molds, yeasts, and protozoa are described, and the two former illustrated, in Chapters VII, VIII, and IX; the mutual relationships of micro-organisms, in Chapter XII, should follow, continued by Chapters XIII to XV, dealing with nutrition, growth, and enzymes; these lead to nitrogen and "sulfur" metabolism, the carbon cycle, and special metabolic products, such as pigments, in Chapters XVI to XVIII. Here should follow the matter in Chapters X and XI, the action of physical agents, and disinfection, for these lead more logically to the question of the origin of bacterial human disease from various habitats, such as air, water, sewage, and milk, which are briefly dealt with in Chapters XIX to XXII. It may be regarded as a commentary on the change which has taken place since the early workers in bacteriology, mostly of medical training, regarded the soil as a possible habitat for pathogenic bacteria, that the soil is not mentioned as a fomite (source of infection) either naturally or as air-borne dust. The question of food preservation and cleanliness, and the illnesses caused by their infection, occupy two chapters (XXIV, XXV). The human aspects of diseases caused by bacteria, their transmission, and modes of protection and immunity are covered in about 120 pages (Chapters XXVI to XXXII). Students may carry away the impression that the uses of bacteria in industrial fermentations are of little significance, since the chapter dealing with these is sandwiched between those on milk and the preservation of food. There is an appendix with a list of textbooks, a glossary of terms, a topical outline for forty-four lectures, and an index of the main paragraphs.

The author cannot expect students to have opinions of critical value;

he must expect reviewers and others "who have lost their active contact with beginners" (this is quoted from the preface) to express opinions. Students will be quick to note a multitude of misspellings, and frequent repetitions or rephrasings, which are too numerous to be detailed. Students, however, may miss the illogicality of the statement on p. 268, regarding the nodule organisms. They will gather that Beijerinck first proposed the binomial "*Bacillus radicicola*," and that Frank followed with "*Rhizobium leguminosarum*." The author then states, "It is quite definitely known that Frank probably did not have the right organism," and that the name he gave "however . . . has priority . . . therefore, is the one which should be used." Students should, nevertheless, be naturally intelligent enough not to need to ingest the double binomial *Escherichia coli* (*Bacterium coli*) ten times in 2½ pages; the author should have given it thrice more to justify his own (p. 298) intentions! Many authors, again, receive better consideration from critics when they make more effort to write sentences of moderate length, rather than a string of short ones. The author's affection for short sentences is seen in excess in a paragraph on p. 371, containing thirteen sentences in fifteen lines, and on page 389 one with sixteen sentences in fifteen lines. The reviewer wishes also to lay one ghost; on page 15 the name Schultz-Lupitz is quoted twice. This hyphenation is a misconception carried over from the original paper, where the author's name, Schultz, is conjoined with the name of his university town, Lupitz; as one should say Tanner-Illinois.

In the chapter on protozoa reference has not been made to these animals as destructive to bacteria; the student will get the idea that they are entirely pathogenic to man; their rôle in sewage is not mentioned even in the chapter on sewage. There is no reference to the possible pollution of shell-fish beds with town sewage, or to its corollary, the spread of epidemics through the eating of oysters. On p. 201 occurs the statement "hypotonic solutions cannot be of great value"; this appears to neglect the possibility that pathogenic bacteria (see p. 295) may die in river waters through changes in salt concentration. On p. 220 it is stated that cresols are not soluble in water. Two questions are also necessary; with reference to a statement on p. 231, what benefit does an aerobic organism obtain from the quoted association with an anaerobe; and on page 270, why cannot the non-symbiotic fixation of nitrogen be regarded as constructive? The statement on p. 376 regarding the nature of "ptomain" does not agree with that on page 444. Amongst the errors not likely to be found by students we would include the quotation of the binomial *Erwinia amylovora* (page 16) as though used in 1878; on page 39, the diagram No. 5 is doubly too large; page 56, the statement that involution has the opposite meaning to evolution implies that "involution forms are cells that retrogressively recapitulate their ancestry." These errors will not, however, detract from the value of the work as an excellent general outline of microbiology, with especial reference to the needs of students whose aim is a diploma in public health.

P. H. H. G.

Morphologic Variation and the Rate of Growth of Bacteria. By ARTHUR T. HENRICI, M.D. [Pp. xiii + 194, with 36 figures in text, and 2 plates.] (London: Baillière, Tindall & Cox, 1928. Price 13*s*. 6*d*. net.)

THIS is the first volume of monographs on General Agriculture and Industrial Microbiology, published under the editorship of Profs. Buchanan, Fred, and Waksman. A glance at the list of references, pp. 177 to 185, gives the impression that the author of this volume can be credited with but one paper on the subject, namely, on the differential counting of living and dead bacteria. The book itself, however, makes it clear that Dr. Henrici has been

very actively attacking the subject of bacterial polymorphism; six of the ten chapters (Chapters IV to IX) are mainly the result of the author's own work, while one (Ch. III) gives the details of the technique used (and partially devised) by him. The first two chapters and the last discuss critically the work of forerunners in this type of study, and contain suggestive ideas regarding "cytomorphosis."

Every bacteriologist discovers cytomorphosis in bacterial cultures. Some (Löhnis, Mellon, Almquist, and others) have developed from it theories of bacterial life-cycles, with sexual phases. The author discusses such theories in Ch. I. He himself has studied mainly the variations in morphology of bacterial cells during the different phases of growth, these latter being based on Buchanan's analytical curve. He discusses the problem of the rate of growth in Ch. II, where he deals also with the influence of the size and age of seeding upon regeneration. The author adduces facts from studies of higher organisms to show that these phenomena are similar among micro-organisms. The technique which he developed in his previously published work for distinguishing living from dead cells has evidently given some success as a basis for measurement. The problem still faces us to disentangle reproduction-rate, death-rate, and autolysis-rate; the author implies (p. 51) that the question of evidence of viability is not cleared up, since he made most of the counts by microscopic technique only. The statement on p. 55 does not help us here: "Because these stained cells become increasingly more numerous with increasing age of the culture, I have concluded that they are dead cells." Another bacteriologist, even if no statistician, may legitimately complain that no data are given to show how accurate were the counts of bacteria. The figures given in the tables (there are 27 of these) in the appendix refer to the number of cells per ccm. with no indication of the size of the errors arising from the technique. The organisms studied were *B. megatherium*, *B. coli*, a diphtheroid bacillus, the cholera vibrio, and *B. cohærens*, the last-named as a subject for the study of spore-formation (Ch. VII). The cell measurements are given in curves and further displayed as blocked frequency curves for sizes at different hours. It is not easy to follow these blocked curves when arranged as they are on pp. 78 and 79; it would be a help to the student if the ordinates were lined up. The author has made use of the factor "area-length index," which he gets by dividing the length of a cell by its breadth, the value naturally becoming smaller as the length decreases; surely the well-used term "length-breadth ratio" would meet the case? The author has a tendency to regard curves as throwing light on the morphology of the cells; this is especially evident on pp. 122, 123. A number of sentences are so constructed as to obscure rather than to illuminate. The last two sentences in the middle of p. 71 evidently mean "The longest cells occur in the 'logarithmic' phase; they become shorter at the onset of the phase of 'negative acceleration' until they reach a minimum size at the beginning of the 'stationary' phase." These tendencies, however, do not prevent us from reading the work as a connected whole. The author has done well to put his work together in this form, for it cannot fail to stimulate further research.

P. H. H. G.

A First Biology. By S. MANGHAM, M.A. (Cantab.), and W. RAE SHERRIFFS, M.A., D.Sc., F.Z.S. [Pp. vii + 184, with 87 figures.] (London: Sidgwick & Jackson, Ltd., 44, Museum Street, W.C.1, 1928. Price 2s. 6d.)

THE authors of this little book are averse from the type system and hold that the young student should have a wider view than is possible under that method. This idea seems to have led them to the other extreme and it may be questioned whether the figures on pages 20, 22, 23 and similar ones

with very short accompanying descriptions can have much educational value at the stage at which they are introduced.

The chapter on movement includes matters often considered under other headings, and the labels on Fig. 87 are wrongly placed.

The illustrations are good and numerous, the writing simple, and the price low.

E. M. C.

The Zoological Section of the "Nuzhatu-l-Qulub" of Hamdullah Al-Mustaufi Al-Qazwini. By Lieut.-Colonel J. Stephenson, C.I.E., M.B., D.Sc., F.R.C.S. [Pp. xix + 100, with 127 pages of the original text.] (London: The Royal Asiatic Society, 1928.)

It is generally recognised that during medieval times the Arabs were largely responsible for the continuance of scientific knowledge and the influence they had in Europe is fairly well appraised. This is not so, however, in the Islamic East, about which even yet comparatively little is known, and hence the importance and interest of the present work.

The *Nuzhatu-l-Qulub* of Mustaufi was a kind of Persian Encyclopædia of natural science written about A.D. 1340. Its contents were arranged in three portions, of which the first was subdivided into three sections dealing with the mineral, the vegetable, and the animal kingdoms. It is the third of these sections that is here provided with a well-annotated translation. Actually it may be regarded as not only the first but the only Persian work dealing with natural history as such, and even the Arabic writers who preceded or followed Mustaufi when they treated of animals did so form the philological or literary points of view. The number of animals described is two hundred and twenty-eight, if we consider the fish with its thirty-seven varieties as one. They are arranged in the alphabetical order of the Arabic names. Following the name are frequently the Persian, Turki, and Mongolian equivalents, then a brief description of the animal itself, and lastly its medical uses or those of its parts or organs.

The translator has somewhat exceptional qualifications for his task, for to a knowledge of Persian, which has enabled him to collate a number of the early manuscript copies of the *Nuzhat* in a scholarly fashion, he has been able to add that of a trained medical man and an expert zoologist in the more strict sense of the term. The result is a highly commendable addition to our knowledge of the early history of zoology and of the eastern spread of Arabic science.

C. H. O'D.

How to Stain the Nervous System: A Laboratory Handbook for Students and Technicians. By J. Anderson, with an Introduction by J. G. Greenfield. [Pp. x + 139.] (Edinburgh: E. & S. Livingstone, 1929. Price 5*s*. net.)

This little book, primarily intended for the laboratory technician, contains clear and concise instructions for carrying out the routine methods in common use for investigating the nervous system. The methods are given in detail as they have been used and standardised by the author. The methods chosen are not new, but many useful original modifications and excellent practical hints are incorporated in them. The book is not designed to supplant the larger reference books on histological technique, but provides a very useful introduction to those sections of them which deal with the nervous system. The book is well printed and bound, and the arrangement is good. An Appendix of standard formulæ adds to the usefulness of the book. It should receive a wide reading in pathological and histological laboratories.

F. W. R. B.

MANDÆANS By Svend Aage Pallis, M.A., Ph.D. [Pp. x + 216.] (London: Oxford University Press; V. Pio. Povl Branner, 13, Nørregade, Copenhagen. Price 10*s.* 6*d.* net.)

A Danish edition of this volume was first published in 1919 and was translated into English by Elizabeth Hude Pallis in the same year; a second revised Danish edition appeared in 1926. The present volume is presumably a translation of the second edition. The essential purpose of this study is historical in character; it is an attempt to trace the influences which have contributed to the growth and development of Mandæan Philosophy and to obtain indications of the original dwelling-place of the Mandæans. The author makes a close study of the Mandæan scriptures, notes the sometimes astonishing inconsistencies to be found in their different tractates—even, for example, on such a fundamental point as to whether good deeds during life determine the saving of the soul and its transference after death to the "realms of light," or whether their very important rite of baptism alone decides its fate—and seeks from a comparative study of the religions of neighbouring peoples to show some of the influences which have led to the moulding of Mandæism.

The fundamental ideas of Mandæism are compared with those of the Babylonian religion, the Persian religion, Judaism and Gnosticism, and the volume constitutes an exceedingly interesting study; it leads to no definite knowledge concerning the origin of the Mandæan cult, but it does suggest some of the influences which have contributed to its subsequent development. Certain fundamental ideas common to all Gnostic systems are a conspicuous feature of Mandæism, but to which branch of Gnosticism the latter cult may owe its origin remains in obscurity.

Mandæans have been found in Southern Mesopotamia from A.D. 800 to modern times, but they have evidently not always dwelt in these regions; the author's endeavours to discover their original habitat through a study of their language, and of evidence from their tractates, meets with no success.

We appreciate the great amount of labour involved in such a study as this, and feel that all who are interested in the science of comparative religions must acknowledge indebtedness to this author for his valuable contribution to the topic.

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Engines. By E. N. da C. Andrade, D.Sc., Quain Professor of Physics in the University of London. [Pp. xv + 267, with 35 plates and 75 figures in the text.] (London: G. Bell & Sons, 1928. Price 7*s.* 6*d.* net.)

Prof. Andrade's book is by now so well known that any further review may appear unnecessary; a second edition is, however, in the press and another Christmas season, with its inevitable "What shall I give?" is approaching, so that a brief review is not inappropriate.

The six chapters of the book are based on the six lectures which formed the 102nd course of Christmas lectures delivered at the Royal Institution at the end of the year 1927. These lectures are intended for juvenile audiences, that is, they are designed to interest youngsters over the age of fourteen, and for anyone over this age who takes an intelligent interest in things mechanical this book will make an admirable gift. Its appeal is instantaneous, for on turning over the leaves one finds a series of plates forming a unique collection of photographs of engines of all kinds, from the odd-looking fireless locomotive, built for use in places where steam is cheap and sparks dangerous, through all kinds of British and foreign giants, moving and stationary, to such novelties as the turbine locomotive built by Berger and Peacock, and the steam-Diesel locomotive designed by Mr. Still and

built by Kitson of Leeds. In the letterpress it will be found that the author has neither hesitated to tackle nor failed to explain quite difficult things, such as the Michell bearing and the Walschaert gear. Moreover, he has drawn a little on his vast store of historical knowledge and tells us, *e.g.*, how Papin measured his temperatures; how Savery was treated by Mr. Dummer; Watt's opinion of Mr. Washborough, and why he patented five different ways for turning reciprocating motion into rotation; and then, as material for the historian of the future, why Diesel-engined ships have funnels.

If the reader is helped by simplicity of style, by analogy and by story, he is nevertheless given a thoroughly sound and scientific review of the subject and at the end really will know a great deal about engines of all kinds; better still, he will realise how much there is to know and how much patience, ingenuity and scientific knowledge are embodied in the design of a modern engine.

D. O. W.

Being Well-born: An Introduction to Heredity and Eugenics. By MICHAEL F. GUYER. Second edition. [Pp. 490.] (London: Constable & Co. 1928. Price 21*s*. net.)

THE first two chapters deal with heredity and the method of studying it; there will be general agreement with the author's statement that our knowledge of heredity is derived mainly from three lines of investigation: (1) from a study of embryology, (2) through experimental breeding of plants and animals, and (3) through the statistical observation of a large number of parents and offspring.

We think that the statistician will maintain that he, as easily as the experimenter, is able to determine whether certain characters are truly hereditary or merely environmental—(1) "by varying the environment and observing whether or not the character in question remains constant, and (2) by keeping the environment as uniform as possible and determining the range of expression of the traits under observation." For a study of *man* the statistical method must take the place of the direct experimental method suitable for plants and animals; investigations into inheritance in man can only proceed by observing the results of the unconscious experiments made by man himself and it can only be determined by statistical methods whether certain characters are truly hereditary or merely environmental.

The succeeding chapters deal with reproduction and development, with chromosomes, with sex-linked characters, etc., and with the Mendelian theory of inheritance in plants and animals. The technical terms used are defined so that it is possible for persons other than zoologists to grasp the substance of these chapters; a glossary of terms is also provided. We wish all writers on similar subjects would follow Prof. Guyer's example in this respect.

In Chapter XIII the author deals with human heredity, and first with Mendelian principles. He suggests that future investigators will have to establish how many human traits follow Mendelian laws, but considers that, "While it must be said that in many cases no simple form of Mendelian tabulation has been unequivocally established, yet the general behaviour of the various inheritable traits in question is so obviously related to the conventional Mendelian course that there seems little reason for doubting that they are at bottom the same." The question of the breeding out of recessive defects is introduced; we do not like to see it suggested in a book introductory to eugenics, that any person carrying a recessive defect should marry into a normal stock thus infecting that stock; breeding out defects by this method is only possible on the Mendelian theory of inheritance

if the defect is a rare one; practically it would be very dangerous if applied to any character such as mental weakness which is at all widely spread. We think, from remarks later in the book, that Prof. Guyer is of this opinion. In the course of this chapter the author refers to the "wrangle" over the relative importance of heredity and environment; this "wrangle" seems to us the essential matter at the moment, if by the term "environment" is meant the actual range of environment in which man, or any other organism, is found existing, not an environment artificially produced. It will have to be decided and stated clearly whether heredity or environment is the more important factor in race improvement before anything can be done with confidence to achieve progress for the race.

A considerable number of debatable points are introduced in the chapter on the origin of new characters, and in those on the inheritance of acquired characters; in the chapter on prenatal influences the author dismisses the "myth of maternal impression." He then discusses injurious prenatal influences, such as lead poisoning, alcoholism, and venereal diseases.

In the chapter on human conduct we feel that Prof. Guyer regards conduct as largely dependent on the germ, but that he wishes at the same time to make out as good a case as he can for the power of training and for the influence of environment.

The chapter on mental and nervous disorders contains some alarming statistics as to the proportion of insane in the United States, and of the increase in epilepsy and feeble-mindedness. Prof. Guyer quotes with approval the opinion of those who consider that defective mental and unstable nervous conditions are transmitted as surely as physical defects, but he points out that it is the unstable nervous organisation that is inherited, not a particular neurosis or psychosis. He does not agree with the group of psychiatrists who "are inclined to attribute much insanity to focal infections instead of a hereditary predisposition," and considers that they will have to explain why such infections produce insanity in certain individuals and not in others equally or more highly infected. Considering the question of restraining the mentally unstable from having children he quotes some striking pedigrees from a paper by Dr. Barr, and evidently does not consider that by preventing the propagation of the feeble-minded we may be depriving the world of geniuses. In considering crime and delinquency, Prof. Guyer is uncertain how much of crime is to be attributed to heredity and how much to environment, but the evidence put forward gives more weight to heredity.

Immigration into America is regarded as a serious question; the contributions from other countries of late have not been desirable; the intelligence of the foreign-born, as judged from the army tests of alien conscripts, was found to be far below that of the native-born, and in State and Federal custodial institutions the foreign-born contribute more than their share of feeble-mindedness, epilepsy, insanity, etc. The cross-breeding of races is also discussed.

The final chapter is entitled "Race Betterment through Heredity," and here the author has almost entirely banished the idea that race betterment can be achieved through improvement in environment. He raises the questions of segregation and of sterilisation of those who would be undesirable parents, but thinks our present knowledge does not justify any drastic measures. The reader must, of course, weigh the evidence, but if crime, epilepsy, and insanity are increasing in America at the rate stated in this book, it will be unwise to practise caution too long, and we consider that the evidence given here is sufficient to justify steps being taken to prevent certain groups of persons from adding to the population.

Field and Colliery Surveying: A Textbook for Students of Mining and Civil Engineering Surveying. By T. A. O'Donohue, F.G.S., M.I.Min.E., F.S.I., and T. G. Bocking, M.I.Min.E., F.S.I. [Pp. xvi + 327, with 232 illustrations.] (New and revised edition, Macmillan, 1928. 10*s*. 6*d*. net.)

This book was published primarily with an eye to the needs of the Colliery Surveyor, but apparently the requirements of those entering for the more elementary examinations in Surveying have been kept closely in view as well.

The examinations to which special reference is made are those in Mine Surveying, especially of the City and Guilds Institute, and the Board for Mining Examinations, Mines Department.

This being so, it is natural to find that surveys by compass—and especially by the miners' dial—are treated somewhat fully, as is, also, the question of magnetic declination.

The sections devoted to Levelling and Theodolite Surveying are, at any rate from the point of view of the Civil Engineer, much less satisfactory.

For instance, though a chapter is devoted to "Mathematical Tables" (in which reference is made to logarithmic sines, etc.), yet, in a worked example, differences of latitude and departures are worked out by long multiplication, while the formulæ from which they are calculated do not appear to be clearly stated at all.

Again, in dealing with the adjustments of the theodolite (p. 144) the adjustment for horizontal axis is given before that for collimation. In the description of the method to be used, there is no statement that the angles of elevation and depression to the marks observed should be equal; yet, without this precaution, the test is affected by any error in the collimation adjustment.

In the chapter on Levelling apparently nothing is said about the "equalisation" of back- and fore-sights, though this is important in most engineering work; and the sections relating to the use of contours is far from complete.

Strangely enough, only about two pages are devoted to the Plane-table.

Such omissions are, however, no doubt due to the fact that it is impossible to put everything into one book of this size; and one's criticism, in the main, amounts to the suggestion that it might have been better to restrict the scope, and not to try to include Civil Engineering Surveying.

Most chapters conclude with several examination questions on the work of the chapter (to which, however, there appear to be no answers), and other questions are given at the end of the book.

These will, doubtless, be of use to those preparing for the examinations to which reference has been made.

M. T. M. O.

The Technology of Low Temperature Carbonisation. By Frank M. Gentry. [Pp. xvii + 399, with 80 figures and 131 tables.] (London: Baillière, Tindall & Cox, 1928. Price 34*s*. net.)

In this volume the author has attempted to give a full review of the most important researches upon coal, with special reference to its behaviour on carbonisation, in order to bring as clear a light as possible upon the essential factors which influence the technique of the processes of so-called low temperature carbonisation.

The first thirty-eight pages are devoted to a statement of the theories, and their experimental foundation, for what occurs during the treatment of coal by heat and the differences which are characteristic of the use of "high" and "low" temperatures. Four chapters then follow, to page 205, dealing with the properties of "low temperature" products, gas, coke, tar, and nitrogenous and other products respectively.

Only a small section of the book, forty-eight pages, is devoted to the

description of specific plants. Thirteen of these are described briefly, but special attention is drawn to Fieldner's useful tabulation of the general principles made use of in modern processes. The following chapter on operation, design, and materials is a very useful one in that it collects together in convenient form information upon such physical data as the thermal conductivity, etc., of materials and the limiting conditions under which economic service can be expected.

The concluding chapter on economics is written chiefly from a consideration of American markets, but is of value in other countries, as the many pros and cons are clearly stated. The importance of local conditions, the limitations of certain markets, and problematic value of the tar products are all carefully discussed.

The chief value of the book lies in the wide bibliography given of experimental work—413 references in all are given—and in the indications of the nature and results of these researches. It is perhaps unfortunate that the conditions have not been clearly stated in every instance when experimental work is quoted, and the reader is often in doubt as to the justification for comparisons which are made, as, for example, when tar from a large-scale process is compared directly with a laboratory product without this essential difference being stressed. These faults make the book of less value to the beginner, but the collection as a whole should be a useful work of reference for students of this subject. In addition there are rather many inaccuracies which offend, such as the description of acetylene as a polymer of benzene (page 16), of phenols as acidic hydrocarbons (page 15), and analytical data which are obviously incorrectly stated (pp. 8, 41, 53, etc.).

The book is very well presented by the publishers, and in particular the graphs, of which there are many, are very clearly reproduced. It should form a useful addition to the library of all connected with carbonisation processes.

J. G. King.

Practical Steelmaking. By Walter Lister. [Pp. xii + 414, with 212 illustrations.] (London: Chapman & Hall, 1929. Price 25*s.* net.)

Steelmaking is an art, and the various operations connected with it require that intellectual and physical skill gained by experience. This skill cannot be acquired in the study or in the lecture-room, and anyone who wishes to possess a true knowledge of the processes of making steel must enter the works and take part in the activities at the furnace and at the mill. Steelmaking is essentially a practical matter, in fact one cannot conceive of theoretical steelmaking, although it is true that theories can be advanced to explain the various changes and conditions at the different stages of production. These theories have their roots in the sciences of chemistry, physics, physical chemistry, mechanics and mathematics, and it is these sciences which form the foundation of what can be called the science of steelmaking. A sound knowledge of these sciences enables the steelmaker to bring a superior intellectual skill to his art. A good example of this is the improved quality of steel which is now obtained by the application of a knowledge of physical chemistry to furnace practice.

The book under review gives in a concise form the results of over twenty years' experience of making steel by many of its various processes. It may be considered that the first word of its title is almost redundant, but its purpose is to emphasise and define the fact that actual works practice is described. As the author points out, there are not many books devoted to the practical side of this subject. This defect, however, is due to the practical man, who, as a rule, does not find the writing of books an attractive task. For this reason alone the author should be congratulated on having produced the present volume, but he can, in addition, be congratulated on having written a sound book on the subject. Considering the different processes

and their relative importance, the amount of space allotted to each is reasonable. For the purpose of review it is convenient to divide the subject-matter into four sections. The first section, consisting of two chapters, is devoted to the acid and basic Bessemer processes. The next section of about thirty chapters deals with open-hearth practice and its various modifications and concludes with a chapter on heat treatment and testing, and one on producer gas. This portion of the book contains much useful and detailed information on such subjects as, making the hearth, starting a new furnace, charging, melting, working down, tapping, repairs, working a hot-metal charge, manufacture of certain high-grade steels (tube, spring, stamping, forging, axle, tyre, wire, file and alloy steels), types of furnaces, equipment, valves, etc. The third section of five chapters covers making steel in the electric furnace, while the last three chapters, which form the fourth section, are on Pit Work, Defects in Steel, and the Manufacture of Ferrosilicon. In regard to pit work this is a subject on which steel-makers hold widely divergent views ; in fact, in steelmaking generally there are many differences of opinion as to the details of practice. Methods which are the outcome of practical experience are frequently met by an alternative array of counter-methods which are likewise the result of experience. The chapter on defects in steel is somewhat brief and is liable to give the student the impression that they can be easily avoided. Throughout the volume there are many working records and a large number of good illustrations.

The book can be strongly recommended to young steelmakers and students of the subject as an excellent guide to many of the details of steel-work practice. The thanks of all metallurgical students who are interested in steel are due to the author for having set forth so clearly the results of his experience. The book is a most useful addition to the literature on steel.

E. COURTMAN.

Popular Map Reading. By E. D. LABORDE. [Pp. xii + 118.] (Cambridge : At the University Press. Price 6*s*.)

THE author of this book writes in the Foreword that the aim has been to produce a popular book, a textbook which the non-specialist can appreciate, and above all one that is not too unpleasant to read. The book may be considered to have fulfilled these conditions.

The book may be divided into three main sections, the first of which deals with the representation of topographical features and the recognition of such features on the map ; the second with the problems of slope and intervisibility ; and the last with the uses of maps and their interpretation. The interpretation of the map has been gradually evolved and this later statement is in the nature of a general review. It has the merit of being inspiring and it will do much to cause people to try to read maps and not regard them as elaborate road guides or jig-saw puzzles.

Mathematical and other relevant matters have been introduced where necessary, but the simple treatment they have received will not cause any difficulty.

The book is well and fully illustrated and the carefully chosen exercises which are incorporated in the text increase its value. There is also an index.

May the reviewer suggest that, as the author has defined " hillock," he should treat " horst " in the same way (p. 16) ? It would also be advisable to insert heights in such figures as 17, 32, 44, and 47 ; and the points P. C. A. in figure 3. There is an obvious mistake in figure 2, which, to meet the requirement of the text, should read G2.

In spite of these minor faults the book is one which will appeal to all who use maps and will prove extremely useful to geographers and students.

J. ELING COLECLOUGH.

Imhotep. By JAMIESON B. HURRY, M.A., M.D. [Pp. xvi + 211.] (London: Oxford University Press, 2nd Edition, 1928. Price 10s. 6d. net.)

THE first edition of this book was published in 1926 and was reviewed in this Journal. The second edition has been carefully revised and some 90 pages of new matter and fresh illustrations introduced. The latter include an excellent coloured reproduction of a mural relief at Philæ, representing Imhotep as Deity of Medicine, and several drawings from the temples at Philæ, Karnak, and elsewhere illustrating Imhotep as one of a group of deities. Numerous additions to the text have been made in each chapter, and the Bibliography has been considerably extended. To the last chapter, which has been renamed, has been added a section in which are collected such meagre references to Imhotep as are to be found in works dealing with the History of Medicine. β_4.

Observations on Human Heredity. By J. S. MANSON, M.D. [Pp. x + 84, with 19 illustrations and 8 plates.] (London: H. K. Lewis & Co., 1928. Price 6s. net.)

IN this small book the author has collected together a number of pedigrees illustrative of a tendency towards hereditary transmission of diseases of various kinds. The observations were made by Dr. Manson himself, between 1911 and 1926, in the course of a busy medical practice, and his labours earned for him the Sir Charles Hastings Prize of the British Medical Association in 1927. Most of the family histories have been previously published by him in medical journals.

Only those who have personally endeavoured to obtain detailed and reliable family histories from their patients, and to verify them, will be able to appreciate the amount of patience and labour involved in making even a small collection of this kind. This, the necessary ground-work for research into the laws of inheritance, is essentially a work which general medical practitioners are best able to carry out owing to their contact with families, as distinct from individuals, which is usually denied the hospital or consultant specialist. It will be well if Dr. Manson's book stimulates more of his colleagues to follow his example. The pedigrees here studied include a family with 13 cases of lamellar cataract in 4 generations and a family with 27 cases of syndactylism and polydactylism in 5 generations, the account of the latter being well illustrated by radiographs from 3 of the cases described. There are also shorter pedigrees of epicanthus and ptosis, spastic paraplegia, deaf mutism, ichthyosis, hare lip, spina bifida, and hereditary icterus.

In the closing chapter the author rather suggests that the "observational" method of studying individual pedigrees and drawing conclusions therefrom should suffice to unravel the laws of heredity without invoking the aid of the statistical method, but the two methods are surely complementary, for only the broadest generalisations can be made from the observational method alone, and neither method by itself can lead us far towards a scientific treatment of the question without the aid of the other.

Dr. Manson concludes with a plea that the study of human genetics should become part of the work of state medicine. β_4.

Astronomy for Surveyors. By M. K. RICE-OXLEY, A.C.G.I., A.M.I.C.E., and W. V. SHEARER, B.Sc., Whit. Schol., A.M.I.C.E., M.I.Mech.E. [Pp. ix + 208, with 34 figures, 2 charts, and 6 appendices.] (London: Methuen & Co., 1929. Price 8s. 6d. net.)

HERE is a treatise in which both the ideas and the results are set out with commendable clarity. The student of the subject who is not an expert mathematician need not be afraid to tackle the book, for it does not require advanced training in order to follow the mathematical work which has

introduced. The detailed setting-out of specimen calculations, of which those on pages 133 and 134 may be taken as examples, is good and has evidently been designed to be a real help to those who desire to make similar ones; even the various steps in the logarithmic processes are duly recorded. On the other hand, the general arrangement of these mathematical pages with respect to the volume as a whole is such that the reader who does not wish to see any mathematics at all can run his fingers nimbly over them and turn on to the next section of the interesting narrative. The Appendices will be useful to technical readers and the star-charts to all. It is good to see that several pages are devoted to the application of wireless time signals to the determination of longitude, for such signals have, of course, completely revolutionised the work.

H. S. Toy.

Some Notable Surveyors and Map-makers of the Sixteenth, Seventeenth, and Eighteenth Centuries, and their Work. A Study in the History of Cartography. By Sir Herbert George Fordham. [Pp. xii + 99, with 9 illustrations and a List of Works of Reference.] (Cambridge: At the University Press, 1929. Price 6s. net.)

With this little volume the author makes another valuable addition to his previous works. The scheme of the book is the linking-up with the personality of some one notable individual or family, in each case, of an outline of the development and progress of the art and science of cartography during the long period which preceded the modern systematic surveys organised by the Governments of the countries of Europe and the American Continent. It is divided into four sections: (1) The Elizabethan Surveyors, their Work and its Influence on the British Cartography of the Seventeenth Century, the sub-sections of which are (*a*) Christopher Saxton, Philip Lea and the Quartermaster's Map; (*b*) John Norden, Philip Symonson and Estate Surveys; (*c*) The County Maps of England and Wales of the Seventeenth Century; (*d*) The Art and Technique and General History of the Cartography of the Sixteenth and Seventeenth Centuries. (2) The French School of Cartography of the Seventeenth Century, subdivided into (*a*) Nicolas Sanson and his Engravers and Publishers; (*b*) The Younger Sansons and the Jaillot Family of Cartographers; (*c*) The Art of the French School of the Seventeenth Century and its Relations with the Cartography of the following Century. (3) Cartography as an Exact Science, dealing with the work of the Cassini family in the Eighteenth Century in France. (4) British Cartography of the second half of the Eighteenth Century, dealing with the work of John Cary, General William Roy, and early history of the Ordnance Survey.

The nine plates are reproduced admirably, but it seems rather unfortunate that such a useful guide to the subject should not have been printed in a rather larger font of type.

H. S. Toy.

Excavations in Malta. Part III. By M. A. Murray. With Chapters by C. Ainsworth Mitchell, M.A., F.I.C., and Thomas J. Ward. [Pp. 38, and 35 plates.] (London: Bernard Quaritch, 1929. Price 7s. 6d. net.)

With Part III of the *Excavations in Malta* Miss Murray brings to a close her account of the exploration of Borg en Nadur, for in the seasons of 1926 and 1927 the site was excavated down to bed rock. In one sense the result was disappointing. The apparently stratified soil at the west of the apsidal building of the temple was found really to be stratified in appearance only. In the two lower strata the grey "neolithic" and the gritty "bronze age" ware were mingled and in the lowest stratum they were found with modern Birchicara ware. This result is intensely disappointing, and Miss Murray's patient and painstaking work, carried out in the intense discomforts of Maltese summers, deserved a greater reward. It would, however, be over-hasty

to conclude that Miss Murray's book contains nothing but a record of failure. Her "finds" were of great interest and of no little value to the archæologist. Among the more interesting are the pottery objects which she regards as anchors, votive offerings from grateful sailors who had successfully avoided the dangers of the stormy, rock-bound coast of the south of the island. She supports her view ingeniously by topographical argument, but archæologically the early use of this form of anchor in the Mediterranean has yet to be proved. In her "suggestions as to dating," Miss Murray has made a helpful analysis of the elements which point to the sequence of the Maltese temples. A word of praise must be given to the very full and careful illustrations of the objects found and of various aspects of the temple itself.

E. N. F.

Source Books in the History of the Sciences: A Source Book in Astronomy. By Harlow Shapley, Ph.D., LL.D., Professor of Astronomy in Harvard University, and Director of the Harvard Observatory, and Helen E. Howarth, A.B., A.M., Research Assistant at the Harvard Observatory. [Pp. xvi + 412, with 47 illustrations.] (London: McGraw-Hill Publishing Co., 6 & 8 Bouverie Street, E.C.4, 1929. Price 20s.)

This book is the first of a series which is to present the significant passages from the work of the most important contributors to the major sciences since the Renaissance. Judging by the book before us, the series is destined to be of considerable importance.

In other branches of knowledge, material from the original sources has been printed and has long since been in use; it is difficult to imagine, for instance, what the historian, or even the local historian, would do without the Rolls Series or the valuable work of the Record Commission, to which access can be obtained in most towns and counties in the municipal library or in that of the local archæological society. Now we are to have something of the sort for science and the fact that the project has been approved by various learned associations and societies, and is assisted in the initial financial arrangements by the Carnegie Corporation, is an indication of the value of the enterprise. The wonder is that it has not been undertaken before.

The original publications in which the astronomers of old announced their discoveries to the world are practically inaccessible to the public and are generally unknown. A great many extracts are printed in this first Source Book, which includes passages, sometimes several, from the work of more than sixty writers.

Galileo's account of his discovery of Jupiter's satellites is a fair example of the charm of the passages in this book: "I have now finished my brief account of the observations which I have thus far made with regard to the Moon, the Fixed Stars, and the Galaxy. There remains the matter, which seems to me to deserve to be considered the most important in this work, namely, that I should disclose and publish to the world the occasion of discovering and observing four Planets, never seen from the very beginning of the world up to our own times, their positions, and the observations made during the last two months about their movements and their changes of magnitude; and I summon all astronomers to apply themselves to examine and determine their periodic times, which it has not been permitted me to achieve up to this day, owing to the restriction of my time. I give them warning, however, again, so that they may not approach such an inquiry to no purpose, that they will want a very accurate telescope, and such as I have described in the beginning of this account." And then he goes on to describe in vivid fashion what he saw "on the 7th day of January in the present year, 1610, in the first hour of the following night," when "the planet Jupiter presented itself to my view."

H. S. T.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Algebraic Geometry and Theta Functions. By Arthur B. Coble, Professor of Mathematics in the University of Illinois. American Mathematical Society Colloquium Publications. Vol. X. New York: The American Mathematical Society, 501 West 116th Street, 1929; Cambridge: Bowes and Bowes. (Pp. vii + 282.) Price $3.00 net.

The Elements of Mechanics. By W. D. Hills, B.Sc. Senior Applied Mathematics and Mechanics Master, Dartford Grammar School, Kent. London: University of London Press, 10 Warwick Lane, E.C.4, 1929. (Pp. viii + 140, with 77 figures.) Price 2*s.* 9*d.* net.

Leçons sur les Équations Linéaires aux Différences Finies. Par N. E. Nörlund. Rédigées par René Lagrange. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1929. (Pp. 153.) Price 50 fcs.

Der Vierdimensionale Raum von Dr. Roland Weitzenböck, Professor an der Universität, Amsterdam. Braunschweig: Friedr. Vieweg und Sohn Akt.-Ges., 1929. (Pp. viii + 142.) Price 9 M. Bound, 10·5 M.

Standard Table of Square Roots. The square roots to eight significant figures of all four-figure numbers, with printed differences. By L. M. Milne-Thomson, M.A., Assistant Professor of Mathematics in the Royal Naval College, Greenwich. London: G. Bell & Sons, 1929. (Pp. xii + 90.) Price 7*s.* 6*d.* net.

An Introduction to the Geometry of Dimensions. By D. M. Y. Sommerville, M.A., D.Sc. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xvii + 196.) Price 10*s.* net.

The Conduction of Electricity through Gases. By K. G. Emeléus, M.A., Ph.D. Lecturer in Physics, Queen's University, Belfast. With a General Preface by O. W. Richardson, F.R.S. London: Methuen & Co., 36 Essex Street, W.C. (Pp. x + 94.) Price 2*s.* 6*d.* net.

Experimental Physics. A Laboratory Manual. By Albert Edward Caswell, Ph.D., Professor of Physics, University of Oregon. New York: The Macmillan Company, 1928. (Pp. ix + 181.) Price 6*s.* net.

Polar Molecules. By P. Debye, Ph.D., Professor of Physics and Director of the Physical Institute in the University of Leipzig, Germany. New York: The Chemical Catalog Company, 419 Fourth Avenue at 29th Street, 1929. (Pp. 172.) Price $3·50.

Ions, Electrons, and Ionising Radiations. By James Arnold Crowther, Sc.D., F.Inst.P. Professor of Physics in the University of Reading. Fifth Edition. London: Edward Arnold & Co., 1929. (Pp. xii + 353.) Price 12*s.* 6*d.* net.

Experimental Hydrostatics and Mechanics for School Certificate students. By E. Nightingale, M.Sc., A.R.C.S., Senior Science Master at St. Albans School. London: G. Bell & Sons, 1929. (Pp. xi + 244.) Price 4*s.* 6*d.* net.

Applied Geophysics in the Search for Minerals. By A. S. Eve, C.B.E., M.A., D.Sc., F.R.S.C., F.R.S., Macdonald Professor and Director of the Macdonald Physics Laboratory, and D. A. Keys, Ph.D., F.R.S.C., Associate Professor of Physics. Cambridge: at the University Press, 1929. (Pp. xiii + 253, with 92 figures.) Price 12*s*. 6*d*. net.

Crystal Structure and Chemical Constitution. A General Discussion held by the Faraday Society. March, 1929. London: Messrs. Gurney & Jackson, 33 Paternoster Row, E.C.4. (Pp. 253 + 420.) Price 8*s*. 6*d*.

Physics. Part II, Sound; Part III, Light. By W. J. R. Calvert, M.A., Science Master at Harrow School. London: John Murray, Albemarle Street, W. (Pp., Part II, vii + 142, Part III, vii + 202.) Price 3*s*. net.

The Pyrolysis of Carbon Compounds. By Charles Dewitt Hurd. American Chemical Society Monograph Series. New York: The Chemical Catalog Company, 419 Fourth Avenue at 29th Street, 1929. (Pp. 807.) Price $12·50 net.

Practical Chemistry for Matriculation. By James Bruce, Ph.D., B.Sc., F.I.C., and Harry Harper, B.Sc., A.R.C.S., A.I.C. London: Macmillan & Co., St. Martin's Street, 1929. (Pp. xi + 227.) Price 2*s*. 6*d*. net.

Elektrostatik in der Biochemie, Vorträge des Kursus in Basel vom 8. bis 12. Oktober 1928. Sonderausgabe aus Kolloidchemische Beihefte. Band XXVIII, Hefte 7–10. Herausgegeben von Prof. Dr. W. Ostwald, Leipzig. Dresden und Leipzig: Verlag von Theodor Steinkopff, 1929. (Pp. 209–390.)

The Foundations of the Theory of Dilute Solutions. Papers on Osmotic Pressure. By J. H. Van't Hoff; and on Electrolytic Dissociation by Svante Arrhenius, 1887. Edinburgh: Oliver & Boyd, Tweeddale Court; London: Gurney & Jackson, 33 Paternoster Row, 1929. (Pp. 67.) Price 2*s*. 6*d*. net.

Practical Chemistry for Senior Forms. By Alwyn Pickles, M.Sc., Senior Science Master. Wyggeston Boys' School, Leicester. London: G. Bell & Sons, 1929. (Pp. xv + 224.) Price 4*s*. 6*d*. net.

Chart to Illustrate the History of Biochemistry and Physiology. By Joseph Needham. Cambridge: at the University Press.

Perfumes, Cosmetics and Soaps. With Especial Reference to Synthetics. By William A. Poucher, Ph.C., Consulting Chemist. Vol. II. Being a Treatise on Practical Perfumery. Third Edition. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1929. (Pp. xiii + 521, with 66 illustrations.) Price 25*s*. net.

An Introduction to Modern Organic Chemistry. By L. A. Coles, B.Sc., A.I.C., Senior Chemistry Master, Batley Grammar School. London: Longmans, Green & Co., 1929. (Pp. xiv + 452.) Price 7*s*. 6*d*. net.

An Introduction to the Chemistry of Plants. Vol. II. Metabolic Process. By Paul Haas, D.Sc., Ph.D., and T. G. Hill, D.Sc., A.R.C.S. Second Edition. London: Longmans, Green & Co., 1929. (Pp. viii + 220.) Price 10*s*. 6*d*. net.

Volumetric Analysis. By Dr. I. M. Kolthoff, with the collaboration of Dr. Ing. H. Menzel, Dresden, Germany. An Authorised Translation based on the German Text by N. Howell Furman, Ph.D. Revised and enlarged by the Author. Vol. II. Practical Volumetric Analysis. New York: John Wiley & Sons; London: Chapman & Hall, 1929. (Pp. xiv + 552.) Price 25*s*. net.

Chemistry in the Home. By J. B. Firth, D.Sc., M.S. London : Constable & Co. (Pp. 246.) Price 5*s*. net.

Elements of Mineralogy. By Frank Rutley. Revised by H. H. Read, D.Sc., A.R.C.S., F.G.S., F.R.S.E. Introduction by the late G. T. Holloway. London : T. Murby & Co., 1 Fleet Lane, E.C.4 ; New York : D. van Nostrand Co., 8 Warren Street. (Pp. xxii + 394.) Twenty-second Edition. Price 6*s*. net.

Ore Deposits of Magmatic Origin. Their Genesis and Natural Classification. By Dr. Paul Niggli, Professor of Mineralogy and Petrology, Zurich. Translated from the Original German Edition by Dr. H. C. Boydell, M.I.M.M. Revised and supplemented throughout by Dr. Niggli and Dr. R. L. Parker. London : Thomas Murby & Co., 1 Fleet Lane, E.C.4 ; New York : D. van Nostrand Co., 8 Warren Street, 1929. (Pp. xi + 93.) Price 9*s*. 6*d*. net.

Structure and Surface. A Book of Field Geology. By C. Barrington Brown, M.C., M.A., A.R.S.M., F.G.S., and F. Debenham, O.B.E., M.A., F.G.S., Fellow of Caius College and Reader in Geography in the University of Cambridge. London : Edward Arnold & Co., 1929. (Pp. vii + 165, with 104 figures.) Price 10*s*. 6*d*. net.

Palæontology. By Edward Wilber Berry, Professor of Palæontology in the Johns Hopkins University. London : McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4. (Pp. xii + 392, with 174 figures.) Price 17*s*. 6*d*. net.

The Origin of a Land Flora, 1908–1929. By F. O. Bower, F.R.S. Emeritus Professor of Botany in the University of Glasgow. Huxley Memorial Lecture 1929. London : Macmillan & Co., St. Martin's Street. (Pp. 27.) Price 1*s*. net.

The Work for Agriculture of two great Englishmen. Cawthron Lecture. By Sir John Russell, F.R.S., Director of the Rothamsted Experimental Station, Nelson, New Zealand : The Cawthron Institute. Printed by R. W. Stiles & Co., Waimea Street, 1929. (Pp. 12, with 6 plates.)

Economic Biology. A Text for Students of Agriculture and General Biology. By George P. Welson, Instructor of Biology, Entomology, and Pomology, Chaffey Union High School, and Junior College, Ontario, California. London : McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4. (Pp. xi + 457.) Price 12*s*. 6*d*. net.

This Bondage. A Study of the Migration of Birds, Insects, and Aircraft, with some Reflections on Evolution and Relativity. By Commander Bernard Acworth, D.S.O., R.N. London : John Murray, Albemarle Street, W. (Pp. xxiv + 229.) Price 7*s*. 6*d*. net.

What Darwin Really Said. Connected Extracts from the "Origin of Species," with an Introduction by Julian Huxley. London : George Routledge & Sons, 68 Carter Lane, E.C., 1929. (Pp. 79.) Price 6*d*.

Animal Psychology for Biologists. By Dr. J. A. Bierens de Haan, Lecturer on Experimental Zoology in the University of Amsterdam. London : University of London Press, 10 Warwick Lane, E.C.4, 1929. (Pp. 80.) Price 4*s*. 6*d*. net.

Haliotis. By Doris R. Crofts, M.Sc., Lecturer in Biology, King's College, of Household and Social Science, London University. Liverpool Marine Biology Committee Memoirs on Typical British Marine Plants and Animals. No. XXIX. Edited by James Johnstone, D.Sc., and R. J. Daniel, M.Sc. Liverpool : at the University Press, 177 Brownlow Hill, 1929. (Pp. viii + 174, with 8 plates.) Price 10*s*. 6*d*. net.

An Introduction to the Study of Bird Behaviour. By H. Eliot Howard. Cambridge: at the University Press. (Pp. xi + 136, with 10 plates.) Price 42s. net.

The Cowbirds. A Study in the Biology of Social Parasitism. By Herbert Friedmann, Amherst College. Baltimore, U.S.A.: Charles C. Thomas; London: Baillière, Tindall & Cox, 8 Henrietta Street, W.C.2, 1929. (Pp. xviii + 421, with 28 plates and 13 figures.) Price 27s. net.

A Handbook of the Dragonflies of North America. By James G. Needham, Professor, Entomology and Limnology, Cornell University, Ithaca, and Hortense Butler Heywood, assisted by Specialists in Certain Groups. Baltimore, U.S.A.: Charles C. Thomas; London: Baillière, Tindall & Cox, 8 Henrietta Street, W.C.2, 1929. (Pp. viii + 378.) Price 31s. 6d. net.

Quain's Elements of Anatomy. Eleventh Edition. Editors, Sir Edward Sharpey-Schäfer, LL.D., Sc.D., M.D., F.R.S., Thomas Hastie Bryce, M.A., M.D., F.R.S., and Johnson Symington, M.D., F.R.S. In Four Volumes. Vol. IV, Part III. The Heart, by Thomas Walmesley. London: Longmans, Green & Co., 1929. (Pp. vii + 152, with 4 coloured figures and other illustrations.) Price 16s. net.

The Rôle of Oxidation in Maintaining the Dynamic Equilibria of Life. Being the Thirtieth Robert Boyle Lecture delivered before the Oxford University Junior Scientific Club on June 9, 1928. By Prof. A. V. Hill, M.A., Sc.D., O.B.E., F.R.S. London: Oxford University Press, 1929. (Pp. 24.) Price 1s. net.

Protozoology. A Manual for Medical Men. By John Gordon Thomson, M.A., M.B., Ch.B., Director, Department of Protozoology, London School of Hygiene and Tropical Medicine, and Andrew Robertson, M.B., Ch.B. London: Baillière, Tindall and Cox, 8 Henrietta Street, Covent Garden, 1929. (Pp. xiii + 376, with 220 figures and 4 plates.) Price 30s. net.

Bainbridge and Menzies' Essentials of Physiology. Sixth Edition. Edited and revised by H. Hartridge, M.A., M.D., Sc.D., F.R.S. London: Longmans, Green & Co., 1929. (Pp. xii + 497, with 192 figures.) Price 14s. net.

Sleep, and the Treatment of its Disorders. By R. D. Gillespie, M.D., M.R.C.P., P.D.M., Physician and Lecturer in Psychological Medicine, Guy's Hospital. London: Baillière, Tindall & Cox, 8 Henrietta Street, Covent Garden, W.C.2, 1929. (Pp. ix + 267.) Price 7s. 6d. net.

Catalogue of Lewis's Medical and Scientific Circulating Library. New Edition. Revised to the end of 1927. Part I. Authors and Titles. London: H. K. Lewis & Co., 136 Gower Street, and 24 Gower Place, W.C.1, 1928. (Pp. 567.) Price 15s. net. To subscribers, 7s. 6d. net.

Mort Véritable et Fausse Mort. Par Henry de Varigny. Essais sur la Mort. Paris: Librairie Félix Alcan, 108 Boulevard Saint-Germain, 1929. (Pp. viii + 318.) Price 20 frs.

The Custom of Couvade. By Warren R. Lawson, F.R.S.E., Fellow of the Royal Society of Medicine, of the Society of Antiquaries of Scotland, and of the Royal Anthropological Institute. Manchester: at the University Press, 1929. (Pp. ix + 118.) Price 7s. 6d. net.

Psychological Conceptions in other Sciences. By Charles S. Myers, C.B.E., F.R.S., M.D., Sc.D. The Herbert Spencer Lecture delivered at Oxford, May 14, 1929. Oxford: at the Clarendon Press, 1929. (Pp. 24.) Price 2s. net.

Science and the Unseen World. By Arthur Stanley Eddington, F.R.S., Plumian Professor of Astronomy, University of Cambridge. London: George Allen and Unwin, Museum Street. (Pp. 56.) Price 2*s*. 6*d*. net.

Weather. Practical, Dramatic, and Spectacular Facts about a little studied subject. By E. E. Free and Travis Hoke. With Maps and Drawings by Elise Seeds. London: Constable & Co., 1929. (Pp. 337, with 50 illustrations.) Price 4*s*. net.

Spitsbergen Papers. Vol. II. Scientific Results of the Second and Third Oxford University Expeditions to Spitsbergen in 1923 and 1924. London: Oxford University Press, 1929. Price 30*s*. net.

The Modern Dance of Death. By Peyton Rous. The Linacre Lecture, 1929. Cambridge: at the University Press, 1929. (Pp. 50.) Price 2*s*. 6*d*. net.

Airplane Structures. By Alfred S. Niles and Joseph S. Newell. New York: John Wiley & Sons; London: Chapman & Hall, 1929. (Pp. xi + 413.) Price 25*s*. net.

The Rhythms of Life, and other Essays in Science. By D. F. Fraser-Harris, M.D., D.Sc., F.R.S.E. London: George Routledge & Sons, 68 Carter Lane, E.C., 1929. (Pp. vii + 185.) Price 5*s*. net.

Speech and Hearing. By Harvey Fletcher, Ph.D. Acoustical Research Director, Bell Telephone Laboratories. With an Introduction by H. D. Arnold, Ph.D., Director of Research Bell Telephone Laboratories. London: Macmillan & Co., St. Martin's Street, 1929. (Pp. xv + 331, with 151 figures.) Price 21*s*. net.

Four Years' Farming in East Anglia, 1923–1927. Being a detailed Investigation of the Costs and Return on Twenty-six Farms. By R. McG. Carslaw, M.A. (Dip. Agric. Econ.). With a Foreword by J. A. Venn, M.A., Fellow of Queen's College, Gilbey Lecturer and Advisory Economist. University of Cambridge, Department of Agriculture. Farm Economics Branch, Report No. 12. Cambridge: W. Heffer & Sons, 1929. (Pp. x + 125.) Price 3*s*. net. Cloth 5*s*. net.

The Will to Work. By G. H. Miles, D.Sc., Assistant-Director of the National Institute of Industrial Psychology. London: George Routledge & Sons, 68 Carter Lane, E.C., 1929. (Pp. 79.) Price 6*d*.

Research in the Social Sciences. Its Fundamental Methods and Objectives. By Robert Ezra Park, and others. Edited with an Introduction by Wilson Gee. New York: The Macmillan Co., 1929. (Pp. x + 305.) Price 8*s*. 6*d*. net.

Index to the Literature of Food Investigation Department of Scientific and Industrial Research. No. 1, March 1929. Compiled by Agnes Elizabeth Glennie, B.Sc. London: Published under the Authority of His Majesty's Stationery Office, 1929. (Pp. iv + 85.) Price 2*s*. net.

Pastures of Wonder. The Realm of Mathematics and the Realm of Science. By Cassius Jackson Keyser. New York: Columbia University Press, 1929. (Pp. xii + 208.) Price 14*s*. net.

Experimental Building Science. By J. Leask Manson, B.Sc., M.R.S.I., and Francis E. Drury, M.Sc., F.I.S.E. Vol. II. Being an Introduction to Mechanics and its Application in the Design and Erection of Build- Cambridge: at the University Press, 1929. (Pp. xiii + 468.) 18*s*. net.

The Growth of the World, and of its Inhabitants. By H. H. Swinnerton, D.Sc., A.R.C.S. London: Constable & Co. (Pp. 211.) Price 5s. net.

Proceedings of the Seventh International Congress of Photography, London, July 9–14, 1928. Editors: W. Clark, T. Slater Price, B. V. Storr. Cambridge: W. Heffer & Sons, 1929. (Pp. xiii + 571.) Price 25s. net.

Einführung in die Wellenmechanik. Von Louis de Broglie, Professor am Institut Henri Poincaré, Paris. Übersetzt von Rudolf Peierls. Leipzig: Akademische Verlagsgesellschaft m.b.H., 1929. (Pp. iv + 219, with 14 figures.) Price 11 Rm. Bound 13·80 Rm.

Some Problems of Indian Meteorology. Being the Halley Lecture delivered on May 31, 1929. By Sir Gilbert Thomas Walker, Professor of Meteorology at the Imperial College of Science and Technology. Oxford: at the Clarendon Press, 1929. (Pp. 23.) Price 2s. 6d. net.

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Hazell, Watson & Viney, Ltd., London and Aylesbury.

SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

ASTRONOMY. By W. M. SMART, M.A., D.Sc., Cambridge Observatory.

THE *Nautical Almanac* for 1931 has just been published, and its appearance calls for some comment. Until recently, astronomical computations were almost invariably done with the help of logarithmic tables and in previous *Nautical Almanacs* many quantities were tabulated logarithmically to ease the computer's labours. The modern tendency in computing work is in the direction of the use of the very wonderful calculating machines now on the market. The *Nautical Almanac* for 1931 is compiled with the need of the computer, possessing such a machine, kept in view; needless to say, it still gives the quantities still required by anyone who prefers the old-fashioned logarithmic methods. There are many new features which will commend themselves to the practical astronomer, but the two additions of greatest interest and real usefulness are, firstly, a learned historical article on the Calendar by Dr. J. K. Fotheringham, Reader in Chronology and Ancient Astronomy in the University of Oxford, and, secondly, a full and clear explanation of the matters, theoretical and practical, with which the Almanac deals. It is inevitable that the many improvements and additions result in a book on a much more generous scale than heretofore; the 1930 Almanac consisted of about 650 pages, its successor now under consideration has about 200 pages more. Dr. P. H. Cowell, the Superintendent of the Nautical Almanac Office, contributes a short preface in which he acknowledges Dr. Comrie's share in the enlargement and rearrangement of the Almanac.

From the Royal Observatory, Greenwich, comes volume v of the *Astrographic Catalogue*. Except for a short discussion by Dr. H. Groot on the double stars measured on the astrographic plates, the volume deals with the proper motions of the faint stars in the Greenwich Zones + 64° to the pole. The original astrographic scheme had and still has its critics, but one result is of great value, namely, that by strenuous

effort in the past a complete photographic record of the sky was obtained; at Greenwich, this was done over thirty years ago. This interval is sufficiently long for the accurate determination of the proper motions. The Greenwich zones were re-photographed between 1923 and 1927 by Mr. Melotte; these recent plates and the original plates form the material from which the value of the stellar proper motions can be derived. The recent plates were photographed through the glass. This allows an old and a new plate to be fastened together, film to film, with the corresponding images of the stars slightly displaced; each star thus contributes a pair of images, one belonging to the old plate and the other to the recent plate, forming what looks to be a double star when viewed in the measuring machine. The catalogue of proper motions given in the volume includes stars as faint as magnitude 13·0; it is divided into two parts, the first dealing with "reference" stars the values of whose proper motions can be supplemented from other sources and for a large percentage of which the spectral types are known, and a second part dealing only with the faintest stars. In the second part only stars with proper motions exceeding 0″·04 per annum are included. One of the great needs at present is the accurate determination of small proper motions, that is, lower than the limit just quoted, and it is to be hoped that this need will be satisfactorily met. An analysis for star-streaming has been made with regard to the reference stars, and an interesting result emerges; the stars of early type adhere remarkably closely to the directions of the two streams very much as if stream I stars were pursuing nearly parallel courses and similarly for stars of stream II. The determination of the solar apex for both the reference and the faintest stars reveals a phenomenon observed elsewhere which can only mean some yet unexplained peculiarity in the stellar motions. If the apex is determined for groups of stars within definite magnitude classes it is found that the declination of the solar apex increases from about $+35°$ for stars brighter than $7^m{\cdot}0$ to $+48°$ for the faintest stars concerned. One region has been analysed for the two star-drifts; the drift-velocities are considerably smaller than those usually derived in similar investigations. The probable error of the annual proper motions has been estimated to be about 0·″008 in each co-ordinate, but the unexpected flattenings of the drift-curves—or, in other words, the comparative smallness of the drift-velocities—can hardly be thrown entirely on the accidental errors. In connection with this Greenwich work, the first of its kind in the utilising of the old astrographic plates for the measurement of proper motions, it is a pleasure to be able to report that Dr. Herrero, of the San Fernando Observatory in Spain, has begun

the measurement of proper motions, using the old astrographic plates taken at Cadiz thirty years ago.

In *Harvard Circular*, No. 326, Dr. W. J. Luyten gives the measures of proper motion of 500 stars in the region of the greater Magellanic Cloud. The photographs were taken with the Bruce telescope, and the interval on which the proper motions are based is about thirty years. In Circular No. 328 Luyten discusses the measures statistically. After deriving the magnitude error (which, unfortunately, seems inherent in most investigations of this kind) he considers the distribution of the proper motions in direction for magnitude groups as faint as $15^{m}\cdot 5$. For the brighter stars, drift I is shown strongly and drift II very weakly. As the fainter stars are considered in turn the relative numerical strength of drift II stars increases with magnitude reaching finally for the fifteenth magnitude stars the approximate proportion of 1 : 2. From an analysis of all the proper motions, the writer of this note concludes that for every star belonging to drift II, there are three stars belonging to drift I, all magnitudes being included. At first sight this might seem to indicate either that in this part of the sky there is a real deficiency of drift II stars—the usual proportion is about 2 : 3—or that the drift II stars are really at a much greater distance on the average than the others, as Luyten's figures for the division into magnitude groups appear to suggest. Actually, the annual proper motions concerned are limited to those greater than $0''\cdot 03$ and it is a well-established fact that when the smallest proper motions are excluded from a statistical discussion, the apparent preponderance of drift I stars generally increases with the limit at which proper motions are excluded When this has been taken into account a further analysis indicates that, without any restriction as to the magnitudes of the proper motions, the ratio of the number of the drift II stars to the number in drift I is 1·9 : 1, which is not so very anomalous.

In No. 180 of the *Bulletin of the Astronomical Institutes of the Netherlands* Dr. J. Woltjer contributes a note on the contours of the H–K emission lines of the sun's chromosphere. Recently Dr. Unsöld (*Ap. J.*, **69**, p. 209) determined the contours concerned and three interesting features emerged from his work concerning the central depression of the lines, their considerable width (of the order of 1 Å), and the rapid decrease of intensity in the wings. To account for these observed facts, Unsöld worked on the hypothesis that the ionised calcium atoms (which give rise to the lines in question) had large random velocities, the mean being about 15 kms. per second. In his paper, Woltjer avoids Unsöld's hypothesis by utilising some of his own results, derived some months earlier, which

were based on the outward motions of the ionised calcium atoms due to radiation pressure. When this idea is applied, the observed features of the emission lines are apparently accounted for satisfactorily.

In *Monthly Notices*, vol. 89, p. 739, Prof. E. A. Milne writes a mathematical paper entitled "Integral theorems on the equilibrium of a star." The first theorem, involving integrals of pressure and mass taken throughout the volume of a star, is entirely general and is true whatever the constitution of the star (gas or liquid) may be, whatever the ionisation or chemical composition, and whatever the distribution of the sources of energy. Other theorems relate to the lower limit for the central pressure in a star and upper and lower limits to the gravitational energy and the mean value of gravity. Milne also gives a general proof of Lane's law with applications.

In *Astronomische Nachrichten*, No. 5661 (Bd. 236, p. 329), Dr. C. D. Perrine, of Cordoba Observatory, contributes a paper on the motions and status of the spiral nebulæ and globular clusters. Much of the present-day information regarding the distances of the spiral nebulæ and globular clusters has been derived from observations made at Mount Wilson. In particular, Shapley's work on the clusters and Hubble's recent work on the spiral nebulæ are too well known to require little more than mention here. Briefly, Shapley placed the clusters on the confines of the galactic system and Hubble has placed the spirals at such remote distances that each spiral must be regarded as an independent stellar universe, comparable in dimensions with the galactic system. From these views, so steadily in fashion at the present time, Perrine strongly dissents in the paper under consideration. The paper is mainly concerned with the statistical relations between the observed radical velocities of these objects, their galactic latitude, size, and ellipticity. Perrine arrives at the following conclusions: (i) the radial velocities of the spirals vary with apparent size, the smaller spirals having the higher velocities; (ii) the relation between the radial velocity ρ and the apparent diameter d is best expressed by the formula $\rho^2 d =$ constant; (iii) the radial velocities increase with galactic latitude; (iv) they are larger for the more elongated objects than for the circular ones; (v) the solar motion with respect to the system of nebulæ and clusters agrees well in direction with that derived from the study of the stellar motions in the galactic system. Perrine adduces additional arguments in support of his thesis that the spirals and clusters are dependents of the galaxy. Their ejection from the galactic system he considers to be due to radiation pressure, a suggestion which he and Prof. Lindemann put forward independently about six years ago. It will

be seen that Perrine's views concerning the structure of the universe are in marked contrast with those at present held by the majority of astronomers, but this fact does not necessarily imply that the correctness of any scientific theory depends on the measure of support it receives. In any event, Perrine's statistical relations have an interest of their own and are sure to be tested further when additional observational facts become available.

No. 242 of *Publications of the Astronomical Society of the Pacific* (August, 1929) contains the summaries of papers read at the Berkeley meeting of the Society last June. We note a few of these papers. Adams and three associates present a paper giving radial velocities of 742 stars and one of the interesting things about it is the limit to which spectroscopic observations can now reach, as stars as faint as 10·8 visual magnitude are included in the list. Hubble provides a paper on a preliminary estimate of the distance of the Coma cluster of nebulæ. This cluster is situated nearly at the galactic pole, and within a circular area of about $1\frac{3}{4}°$ in diameter there are 800 nebulæ, most of which are of the elliptical type. From a photometric investigation, the distance of the cluster is estimated at about 50 million light-years—the greatest distance so far inferred from astronomical measurements. A paper by J. S. Plaskett and Pearce concerns the radial velocities of 870 stars of spectral classes O to B5 ; these are used to test further Lindblad's hypothesis as to the rotation of the galaxy. The results lend weighty support to the theory. Five papers are contributed by Prof. Leuschner and his associates on the orbits of minor planets and comets.

PHYSICS. By L. F. Bates, B.Sc., Ph.D., F.Inst. P., University College, London.

Ortho- and Parahydrogen.—The heading of this section may at first appear somewhat unfamiliar, but for some time it has been known from the application of the new quantum mechanics that ordinary molecular hydrogen must be a mixture of two components. This knowledge arose in the first place from the explanation of the existence of ortho- and parahelium given by Heisenberg (*Zeit. für Phys.*, vol. 38, p. 411, 1926, and vol. 41, p. 239, 1927). In parahelium we have an electron system which emits a series of single lines, or singlet spectrum, whilst orthohelium emits a triplet series. In the parahelium system the magnetic moments of the electron orbits may be considered to oppose one another, and in the orthohelium system to assist one another. Acting on the suggestion provided by the helium model, Hund (*Zeit. für Phys.*, vol. 42, p. 93, 1927) showed that the

hydrogen molecule should behave in an analogous manner. In the case where the nuclear magnets possessed even rotational quantum members they should oppose one another; in the other case, where the nuclear magnets possessed odd rotational quantum numbers, they should assist one another. We should therefore have two kinds of hydrogen molecules, parahydrogen and orthohydrogen, respectively. Since orthohelium and parahelium arise from peculiarities of the electron orbits, it is rather unfortunate that orthohydrogen and parahydrogen, arising from peculiarities of the nuclear orbits, should be so termed.

In his treatment of the specific heat of hydrogen, Hund divided the molecules into two such groups and assumed that transitions between symmetrical states or between antisymmetical states would be of the usual order of intensity, whilst transitions between symmetrical and antisymmetrical states would be very weak. Hund found that the experimental results for the specific heat of hydrogen could be accounted for on his theory only if the symmetrical states occurred twice as frequently as the antisymmetrical states, and he deduced the value of $1{\cdot}54 + 10^{-41}$ gm. cm.^{2} for the moment of inertia of the hydrogen molecule. Now, the value of the moment of inertia found by Hori (*Zeit. für Phys.*, vol. 44, p. 834, 1927) from a careful analysis of the far ultra-violet bands of hydrogen was $4{\cdot}67 + 10^{-41}$ gm. cm.^{2}, and it was also found that the transitions between antisymmetrical states were about three times as intense as the transitions between symmetrical states, and that no lines corresponding to transitions between symmetrical states and antisymmetrical states were found. Moreover, Hund's theoretical curve for the specific heat of rotation of hydrogen gave a sharp maximum, not in agreement with experiment (*cf.* Beutler, *Zeit. für Phys.*, vol. 50, p. 581, 1928). Dennison (*Proc. Roy. Soc.*, vol. 115, p. 483, 1928), therefore, made the assumption that the time of transition between a symmetrical state and an antisymmetrical is very long compared with the time in which observations of the specific heat are made, *i.e.* we have really to deal with two distinct gases. In fact, if C_S is the specific heat of the symmetrical state, C_a the specific heat of the antisymmetrical state, and C_r the rotational specific heat of the mixture, then $\frac{C_r}{R} = \frac{\rho \cdot C_S + C_a}{(1+\rho)R}$, where ρ is the ratio of the number of symmetrical to the number of unsymmetrical molecules. C_S and C_a are obtained from Hund's work; the first rises to a maximum of about $1{\cdot}5R$, whilst the second rises steadily to R, and for $\frac{C_r}{R}$ to fit the experimental curve, ρ must be less than unity. Using the

experimental values of Heuse, Giacomini, Brinkworth, Partington, and Howe, a curve was drawn from which the variation of specific heat with temperature was found. Dennison thus found that for $\rho = 1/3$ the calculated values of C_r fitted the values deduced from the above curve, *i.e.* the antisymmetrical states were three times as numerous as the symmetrical states. He also obtained a value $4{\cdot}64 \times 10^{-41}$ gm. cm.^{2} for the moment of inertia of the hydrogen molecule, in very good agreement with Hori's value. Above 200° K, then, the ratio of the number of othohydrogen molecules to the number of parahydrogen molecules is three to one. Below this temperature, however, the relative number of parahydrogen molecules must increase, and at very low temperatures, *e.g.* just above the liquefaction point, nearly all the molecules should be in the symmetrical or para state. Dennison suggested that this change might be detected by experiment, and that the two constituents might even be separated by experiment.

The direct experimental proof of the existence of the two constituents of hydrogen was first provided by Bonhoeffer and Harteck (*Sitz. Ber. der Preuss. Akad.*, vol. vii, p. 103, 1929, and *Zeit. für Phys. Chem.*, vol. B4, p. 113, 1929) and also by Eucken and his collaborators (*Zeit. für Phys. Chem.*, vol. B4, p. 135, 1929). Eucken measured the specific heat of hydrogen under various conditions at low temperatures in order to obtain information concerning the two forms. Bonhoeffer and Harteck did not measure the specific heat of the gas, but measured its thermal conductivity, which, at low temperatures, may be regarded as directly proportional to the specific heat. Their apparatus was comparatively simple, as absolute determinations of the thermal conductivity were not necessary. The hydrogen under investigation was placed in a glass vessel which was immersed in liquid hydrogen. Inside this vessel was mounted a Wollaston wire. This formed one arm of a Wheatstone bridge, across the terminals of which a source of current at constant potential was connected. The wire was thus heated to about 200° K, its temperature being measured by its resistance. The surrounding hydrogen was always at a pressure of 40 mm. and the temperature of liquid hydrogen, so that convection currents and variation of thermal conductivity with pressure did not appreciably influence the results. The hydrogen was very pure, as all impurities except helium and neon were frozen solid at this low temperature. The authors were very fortunate, because they found that they could make practically pure parahydrogen and use it for calibration purposes. Hence if the resistance of the wire in a hydrogen mixture under the above conditions of pressure and temperature was known, so then was the constitution of the mixture known. This meant that

the authors were able to work far more quickly than Eucken and his collaborators.

It was found that if a flask of hydrogen at a pressure of 200 mm. at air temperature were immersed in liquid hydrogen, then the thermal conductivities of samples drawn off at varying periods up to more than three weeks, and tested in the apparatus described above, were constant within the limits of experimental error. This was very surprising, for it led to the conclusion that orthohydrogen changed to parahydrogen, under these conditions, with a half value period of over a year. As a matter of fact, Wigner repeated this experiment, carefully excluding radiation, and he actually found a half value period of about 300 years. If the cooled gas were under a high pressure, then, as Eucken and Hiller (*loc. cit.*) also showed, a marked change took place in a few days. Thus in Bonhoeffer and Harteck's experiments with the cooled gas under a pressure of 350 atmospheres, the ratio of ortho- to parahydrogen changed from 75 : 25 to 55 : 45 in six days. It was found that liquid hydrogen initially gave a ratio of 75 : 25 which changed to 71 : 29 after standing for five hours, and 60 : 40 after standing for thirty hours. It was found that the transition of ortho- to parahydrogen was greatly accelerated by the use of catalytic agents. Thus a quartz vessel of 50 c.c. capacity was filled with degassed charcoal which was allowed to become saturated with hydrogen under atmospheric pressure and at the temperature of liquid hydrogen. After standing for a quarter of an hour about 8 litres of hydrogen were absorbed. The unabsorbed hydrogen was removed, and the gas which then escaped from the charcoal was found to be 99·7 per cent. pure parahydrogen, with a thermal conductivity about 20 per cent. greater than that of the usual three to one mixture. The pure parahydrogen was very stable and could be kept in a vessel at atmospheric pressure and room temperature for several days without suffering appreciable change. If, however, platinum black were placed in the vessel, or if the pressure were markedly increased, or if a discharge were passed through the vessel, then the gas quickly changed to the ordinary mixture. It was found that simply by drawing ordinary hydrogen through a tube of charcoal in liquid air a mixture with a ratio of 50 : 50 was obtained, and this mixture remained unchanged in glass vessels for days, but changed quickly when drawn through a heated tube of unglazed porcelain. The heat of transformation of orthohydrogen to parahydrogen at absolute zero is 329 cal. per gm., whilst the latent heat of evaporation of liquid hydrogen at the boiling point is 229 cal. per gm. Parahydrogen was observed to have a saturation vapour pressure 25 mm. greater than that of ordinary hydrogen at the temperature of liquid hydrogen, *i.e.* at 20·39°

parahydrogen has a saturation vapour pressure of 787 ± 1 mm. At the triple point the saturation vapour pressure of parahydrogen was 53·D ± ·1 mm., whilst that of ordinary hydrogen was 53·9 ± ·1 mm. Clusius and Hiller (*Zeit. für Phys. Chem.*, vol. B4, p. 158, 1929) have described a comparatively simple method of making parahydrogen more or less based on the above catalytic method, and have determined the specific heats of liquid and solid parahydrogen, and have shown that they agree with the corresponding values for ordinary hydrogen.

It is interesting to note that McLennan and McLeod (*Nature*, vol. 123, p. 152, 1929) established the presence of two distinct kinds of molecules in liquid hydrogen by means of the Raman effect. Liquid hydrogen was exposed to radiation from a mercury arc. The Raman effect showed that both kinds of molecules were set into oscillation with a frequency of 4,159 wave numbers, but one kind exhibited a rotational frequency of 354, corresponding to a transition from a zero to a two-quantum rotational state, whilst the other exhibited a similar frequency of 588, corresponding to a transition from a single-quantum rotational state to a three-quantum rotational state. Since the Raman effect in liquid hydrogen is quite sharp, the Van der Waal's forces cannot appreciably distort the motions of the molecules, and since Bonhoeffer and Harteck have shown that the three to one ratio is not disturbed by liquefaction or solidification, hydrogen appears to be a special substance in which the molecules may rotate in the liquid and solid states.

It is clear that a new line of research of considerable importance has been opened up. Unfortunately, the British contributions in this line are bound to be limited by lack of liquid hydrogen plant.

The Raman Effect.—The Raman effect has already been briefly mentioned in this section, but we must devote further space to the interesting results and discussion which are to be found in a memoir on the Raman effect in liquids by A. S. Ganesan and S. Venkateswaran (*Ind. Journ. of Phys.*, vol. iv, p. 195, 1929). The Raman effect provides us with a powerful weapon for the investigation of infra-red spectra which would otherwise have to be made with difficult and complicated experimental arrangements. The memoir gives a systematic study of a large number of compounds, with special reference to the influence of chemical constitution on Raman spectra, the substances investigated being representative of well-defined organic and inorganic types. The experimental arrangements were those described by R. W. Wood. (*cf.* SCIENCE PROGRESS, No. 91, p. 395, 1929).

The members of the paraffin series, pentane, heptane, and octane, show as their most prominent feature a band corre-

sponding to a shift of 2,850 to 2,960 wave numbers. This band, however, really consists of five lines, of approximately the same intensity, with a spacing of 27 wave members, and it is suggested that this spacing may arise from a rotation of the molecule about the long axes of the carbon chain, but further experiments are necessary to prove this point. The simple halogen derivatives of methane give only a few, rather intense, Raman lines, which are fairly sharp, with the exception of one line. This diffuse line corresponds to a shift of 762 for chloroform and 657 for bromoform. The Raman spectra of the alcohols are characterised by a band with a shift of about 2,960 wave numbers. In methyl alcohol this band is clearly seen to consist of two more or less well-defined lines, but higher in the series a continuous spectrum between the lines becomes so intense that a structure of the band may only be detected with difficulty, and, in addition, fresh lines appear. These are probably due to the increase in mass and complexity of the molecule. It is interesting that whole groups of fresh Raman lines occur in the spectra of the isomers which are not present in the spectra of the corresponding normal alcohol. The Raman spectra of the fatty acids exhibit a general resemblance to those of the corresponding monohydric alcohols. The characteristic band of the alcohols also appears in the spectra of the fatty acids, and also broadens as we go up the series. All the Raman lines of these acids seem to be more diffuse than those of other liquids. The same band also appears prominently in the spectra of the ketones. We also find a line corresponding to a shift of 1,700 wave numbers in the fatty acids and in the ketones, which seems to be characteristic of the carboxyl group. Nitrocompounds all appear to exhibit an intense Raman line corresponding to a shift of 1,347 wave numbers.

In the case of the aromatic compounds, more or less intense Raman lines, with a shift of 3,000 wave numbers and characteristic of the hydrocarbons, again appear. In the aliphatics this radiation occurs as a diffuse band which shows a distinct structure, the band becoming more diffuse as we proceed to higher members of the group. The two edges of the band are fairly sharp and correspond to shifts of 2,850 and 2,960 wave numbers respectively. Yet benzene shows a single intense Raman line whose shift is 3,060 wave numbers. It is suggested that this is because in benzene the H atoms are identical, whereas in the aliphatics the H atoms in the CH_3 groups are probably different to those in the CH_2 groups, and, in addition, there may be some differences between the various CH_3 groups themselves in the case of the isomers. (It should be mentioned here that Dadieu and Kohlrausch (*Phys. Zeit.*, vol. 30, p. 384, 1929) state very definitely that pure benzene gives a Raman frequency of

2,946, characteristic of the CH group in the aliphatics, as well as the frequency 3,060, and they feel that we are bound to make the unlikely assumption that the H atoms are not all identically bound in the benzene ring). Another sharp line in the Raman spectrum of benzene has a shift of 990 wave numbers. In the aliphatic compounds we get a shift of about 1,050, the actual value varying from group to group. These frequency shifts are attributed to oscillations of the carbon atom, in fact, 990 seems to be a characteristic shift of the aromatics and 1,050 of the aliphatics. The Raman spectrum of pyridine shows lines with shifts of 988 and 3,056, which are clearly to be correlated with the 990 and 3,060 shifts of benzene, and this provides support for the suggestion that benzene and pyridine have similar structures. In addition to the above, pyridine exhibits a few characteristic frequencies, of which that corresponding to a shift of 1,067 is the most conspicuous, and appears due to the substitution of N in place of the CH groups. Both quinoline and naphthalene give lines with a shift of 1,378. A line with a shift of 1,580 occurs in quinoline, benzene, pyridine, and naphthalene, and seems to be characteristic of ring structure, as it is absent from open chain compounds. Dadieu and Kohlrausch (*loc. cit.*) give frequencies of 600, 1,000, and 1,600 as characteristic of ring structure.

Details of the Raman spectra of sulphuric, hydrochloric, and nitric acids are given. The first gives a continuous spectrum in addition to lines. The second shows only the water bands which tend to sharpen as the concentration increases. In the case of 65 per cent. nitric acid, the water bands appear as sharply defined as in ice, accompanied by many prominent lines. Particular attention is paid to the Raman spectrum of water. The most prominent of the very broad bands exhibited by water has a shift of about 3,400, which closer examination has shown to consist of three bands which gradually shade into one another. Of these, the central is the brightest, and they correspond to infra-red wave-lengths of 2·77, 2·90, and 3·13 μ. In addition, there are two other faint bands corresponding to wave-lengths of 1·82 and 4·25 μ. Ice shows three more or less sharp bands corresponding to wave-lengths of 2·82, 2·95, and 3·13 μ, in excellent agreement with the values obtained by Krishnan (*Nature*, vol. 122, p. 477, 1928) for water in the form of water of crystallisation in crystals of selenite, though the bands are much less distinct in the latter case. It is suggested that the widening of the water bands as we pass from the crystalline to the liquid state is connected with the increase in the freedom of rotation, the rotational frequencies combining with the characteristic frequencies of the molecules. This suggestion is supported by the experiments of Brickwedde

and Peters (*Bull. Amer. Phys. Soc.*, p. 111, 1928), who find that the Raman lines in quartz crystals broaden with rise in temperature. Of the three components of the 3 μ band mentioned above, the 3·13 μ component disappears when the temperature is raised to 80°, whilst the remaining components are only slightly changed; this disappearance is not yet understood.

Carbon disulphide shows such an interesting effect that the description of its spectrum has been so far deferred in this section. There is no correlation whatever between its Raman lines and the lines in its infra-red spectrum. Rassetti (*Nature*, vol. 123, p. 205, 1929) has shown that carbon dioxide exhibits a similar state of affairs. Carbon disulphide exhibits strong absorption bands at 13·4, 11·65, 6·5, and 4·6 μ which have no corresponding Raman lines, and the shifts of 655 and 800 wave-numbers exhibited by its Raman spectrum have no corresponding infra-red bands. Lack of exact parallelism between the Raman and the infra-red spectra is of course very frequent, but carbon disulphide provides a very striking case of complete lack of correspondence. It is known from the theoretical treatment of Foersterling (*Ann. der Phys.*, vol. 61, p. 577, 1920) that the maximum of a reflection or absorption band may not exactly coincide with the frequency characteristic of the medium, whereas the Raman spectrum gives the natural frequency exactly. The large differences, such as those exhibited by carbon disulphide, must have a different explanation to that given by Foersterling. This is provided by Langer (*Nature*, vol. 123, p. 345, 1929) and Dieke (*Nature*, vol. 123, p. 564, 1929), who make use of the conceptions of wave mechanics. If the molecule possesses two energy states, represented by E_l and E_k, then the condition that a particular frequency ν_{kl} may appear in the absorption spectrum is of course given by $h \cdot \nu_{kl} = E_l - E_k$. The condition for the production of a Raman line of shift ν_{kl} is, however, quite different. As Dieke shows, the intensity of the Raman lines is settled by the probability of transition to one or more discrete levels, whose energy we may represent by E_n, which may combine with both the levels k and l. That is to say, an energy quantum $E_n - E_k$ is absorbed and $E_n - E_l$ is radiated. Hence the Raman frequency can always be taken as the frequency difference between two lines, one of which must be an absorption line. If the transition l to k is possible, and no *n* level exists, we shall only observe an absorption line ν_{kl} and no Raman line. These statements are excellently illustrated by the work of McLennan and McLeod (*Nature*, vol. 123, p. 160, 1929), and particularly by the work of R. W. Wood (*Nature*, vol. 123, p. 279, 1929) on hydrochloric acid. Taking the case of hydrochloric acid, and confining our attention to the molecule in a definite rotational state *j*,

there is no state which can combine at the same time with the j or $j \pm 1$ rotational states. This means that an absorption line of the HCl band cannot be written as the difference of two other lines, and therefore cannot occur in the Raman spectrum. On the other hand, it happens that for the lines under consideration, every transition in which j does not change or changes by two units can be written in more than one way as a difference of two line frequencies, and these frequencies might therefore appear in the Raman spectrum. The particular transitions in which j does not change are forbidden as absorption lines. Of course, the Raman lines corresponding to the change of frequency ν_{kl} become particularly intense as the frequency of the exciting line approaches $(E_n - E_k)/h$. Now the intensity of the Raman lines increases very rapidly with the frequency of the exciting line, and it is therefore suggested that as the frequency $(E_n - E_k)/h$ is being continually approached, and as E_k/h is an infra-red frequency, E_n must lie in the ultra violet at some distance from the visible region of the spectrum. More evidence is, however, needed to settle this point, and we may look forward with pleasure to further memoirs on the subject.

Another interesting paper on the Raman effect is that by Ornstein and Rekveld (*Phys. Rev.*, vol. 34, 720, 1929), who, by considerations analogous to those employed by Einstein in his treatment of Planck's formula, show that the ratio of the intensity I_S of the Stokes lines to the intensity I_{AS} of the anti-Stokes lines is given by

$$I_S / I_{AS} = \frac{\nu - \nu_i}{\nu + \nu_i} \cdot e^{h\nu_i/kT},$$

where ν is the incident frequency and ν_i is the frequency shift of the Raman lines. This formula was verified by means of experiments on carbon tetrachloride, which is particularly suitable for the purpose, as the frequency shifts are rather small and the anti-Stokes lines are correspondingly intense. In these experiments the above ratio was never greater than twelve. Wood's experimental arrangements were followed, the outer tube being used as a thermostat for maintaining the liquid at temperatures 274, 300, 312, 323·5, and 339·5° K. It will be noted that the above formula gives a method for the determination of the ratio h/k, and measurements of this ratio by this method are in progress.

BIOCHEMISTRY.

By P. Eggleton, Ph.D., M.Sc., University College, London.

In August of this year the physiologists and biochemists of the world met at Boston, U.S.A., for their thirteenth triennial

Congress. Of the sixteen hundred scientists foregathered there, about one in three had a communication to make, and the Proceedings of this Meeting (a book of three hundred pages) constitute an up-to-date record of recent progress in Biochemistry. True, the meeting was primarily of physiologists, but the distinction is to-day a vague one. Indeed, it is said that a physiologist can be distinguished from a biochemist only by the higher proportion of brass in his instruments.

Insulin.—The internal secretion of the pancreas has been the subject of work in many countries. Chemically there has been the problem of its purification which has been solved to the extent of preparing it in crystalline form. In three different laboratories at least, crystals have been isolated, from ox pancreas and from the pancreas of certain fish (Jensen, Wintersteiner, and Geiling), and its physiological activity is in all cases about 25 clinical units per milligram. It appears to be a polypeptide containing sulphur; and certain amino acids, including valine, have been identified among its hydrolysis products. It has long been known that the protease trypsin can destroy insulin; indeed, this fact made the isolation of insulin so difficult in early days, for pancreas tissue is rich in trypsin. It is, therefore, not surprising that pure insulin appears to be of a protein nature. Physiologically, there is still mystery surrounding this hormone. It undoubtedly accelerates the disposal of sugar in the blood, both in normal and diabetic animals, whether the sugar is there already or is injected along with the insulin; but the fate of the sugar thus disposed of seems to depend on other factors not yet clearly known. Cruickshank records that whilst a normal mammalian heart which is beating in a circulation consisting of heart and lungs only, responds to insulin injections by converting glucose to glycogen, a diabetic heart does not, unless the concentration of glucose in the blood is kept very high by frequent additions of sugar. The diabetic heart, fed with diabetic blood, utilises no sugar, and, if given insulin, proceeds to utilise sugar as well as a normal heart, but only by oxidation—not by storage. Bürger reports that injection of insulin into the portal vein (which carries away the insulin formed in the pancreas of a normal animal) produces a general rise in blood sugar before producing the fall usually associated with insulin injection, and simultaneously the glycogen content of the liver falls. This is the reverse of its effect on the animal as a whole. Clearly insulin affects different organs in different ways, and even the same organ in different ways, according to circumstances.

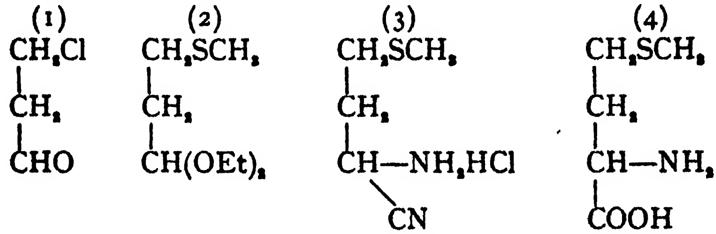
Glutathione.—The structure of glutathione, the respiratory substance found in living tissues of many kinds, was thought

until recently to be a settled question. Stewart and Tunnicliffe (*Biochemical Journal*, 1925, **19**, 207) had synthesised the dipeptide glutaminyl cystine, and found it to be identical in physical and chemical properties with the glutathione isolated from muscle tissue. Hunter and Eagles, who repeated much of the work of the Cambridge Laboratory, found their preparations to contain much less sulphur than was to be expected, and it has transpired that the glutathione molecule contains glycine in addition to the two amino acids already identified. The substance originally isolated by Hopkins must have been a partial breakdown product. Not only has Prof. Hopkins confirmed this discovery, but he has recently (*Journal of Biological Chemistry*, 1929, **84**, 269) worked out an extremely simple method of isolation, based on the insolubility of the cuprous salt of the tripeptide. If the tissue extract, cleared of proteins, is made half-normal in acidity with sulphuric acid, and cuprous oxide is added, this insoluble compound separates in crystalline form, practically analytically pure.

Kendall and Mason reported at the Congress that the estimation of glutathione in tissues by a form of direct iodimetry, such as has been used much of late, is not very specific, and may in some circumstances give misleading values. They oxidise the reduced SH groups with ferricyanide, measuring the ferrocyanide produced by the blue colour of the ferri-ferrocyanide salt (prussian blue). If this technique is repeated on another sample to which formaldehyde has been added, all SH groups present other than those of glutathione behave as before, but glutathione is no longer estimated. The difference between the two values, therefore, measures the true content of reduced glutathione. With the chemistry of glutathione put on a satisfactory basis we may soon hope for some reliable information as to its physiological functions.

Synthesis of Methionine.—Prof. Barger has added one more to the list of isolated and synthesised amino acids. The number of amino acids found in nature is curiously limited: only α-amino acids, and of these only a mere score out of the hundreds of possibilities, can be found. Prof. Mueller discovered some years ago a sulphur-containing amino acid in caseinogen which differed markedly from the only other known sulphur-containing amino acid, cystine. He was able to isolate enough for analysis, and showed it to be $C_5H_{11}O_2NS$. Barger and Coyne (*Biochemical Journal*, 1928, **22**, 1417–25) found the sulphur to be present as a methylthiol group and from a general knowledge of natural amino acids the rough formula $(CH_3S)\ (C_2H_4)\ [C(NH_2)COOH)]$ seemed obvious. They decided to synthesise γ-methylthiol α-amino butyric acid, the simplest

of the four possibilities. By condensing methyl mercaptan with the acetal of β-chloropropaldehyde (1), a good yield of β-methylthiolpropaldehyde acetal (2) was obtained. Gentle acid hydrolysis removed the acetal groups, and the free aldehyde was heated in ether solution with saturated aqueous ammonium chloride and potassium cyanide (one mol of the latter). After six hours of shaking, the ether layer was taken off and dried over sodium sulphate. Dry hydrochloric acid was led in and after removing the excess of this and the ether, a crude crystalline mass of the amino-nitrile was obtained in the form of its hydrochloride (3). Hydrolysis of this with strong hydrochloric acid converted the CN group to COOH, and the product (4) after recrystallisation proved to be the racemic form of Mueller's amino acid. In consultation with Mueller they decided to give the name methionine to the new acid.



The yield at the last stage is poor, but the earlier reactions have high efficiency.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., Ph.D., University, Glasgow.

Igneous Rocks.—Prof. W. Wahl's important paper, entitled "Contributions to the Chemistry of Igneous Rocks: Chemical Equilibria in Cooling Rock Magmas as Depending on the Constitution of the Silicates" (*Fennia*, **50**, No. 29, 1928, 32 pp.), goes far to establish the theoretical basis of Bowen's reaction series, and Niggli's stages of silicification in igneous magmas. He finds that at the highest magmatic temperatures aluminosilicates of comparatively simple constitution are stable together with simple orthosilicates, aluminates, iron oxides, and silica. As the temperature falls metasilicates are formed, and higher aluminosilicates become stable, addition reactions taking place whereby amphiboles, micas, or sodalite minerals appear. Final stages (in alkaline magmas) result in crystallisation of melilite, garnet, or analcite. After the bulk crystallisation has ceased addition reactions may still take place in the presence of water and other volatiles, giving rise to epidotisation, chloritisation, scapolitisation, and finally to the formation of zeolites and calcite in steam cavities and fissures.

Dr. E. S. Larsen has assembled the evidence for the actual

temperatures of magmas (*Amer. Mineralogist*, xiv, 1829, pp. 81–94). He shows that rhyolite magmas have lower temperatures than basaltic magmas. Some basaltic magmas have temperatures below 870°, many are below 1,000°, very few are as high as 1,260°, and probably the majority are not far from 800°–900°. Rhyolitic magmas run between 573° and 870°, as they are known to be below the decomposition temperatures of biotite (850°) and hornblende (750°), and above the inversion temperature of silica (573°). Direct measurements of temperature at volcanoes are extremely variable, even at the same vent, and therefore give unsatisfactory data.

The detailed memoir by Prof. J. J. Sederholm on "Orbicular Granites, Spotted and Nodular Granites, etc., and the Rapakiwi Texture" (*Bull. Comm. Géol. Finlande*, No. 83, 1928, 105 pp.) describes several new occurrences of these interesting rocks, and attempts an explanation of the phenomena in terms of modern petrogenic theory. Sederholm regards orbicular granites as due to a rhythm of fractional crystallisation under conditions of high magmatic viscosity. As regards the rapakiwi texture, Sederholm points out that the ovaloid felspars which characterise this texture are the only ones with investments of oligoclase on orthoclase. He suggests that crystallisation of the potash felspars caused enrichment of the magma in oligoclase substance, which did not diffuse readily owing to its high viscosity, and therefore gradually blanketed the orthoclase, stopping its crystallisation. A word is due to the magnificent illustrations of orbicular and related textures adorning this memoir.

By the study of fifty analysed siliceous lavas, H. A. Powers (*Journ. Geol.*, **37**, 1929, pp. 268–71) has shown that the amount of excess silica in a rock determines the type of its groundmass. Rocks with more than 26 per cent. of quartz in the norm showed rhyolitic textures, while those with less than this amount had trachytic textures. Most of the rocks showed a reasonable approach to the composition of Vogt's granitic anchi-eutectic. Nevertheless the phenocrysts present were not of the kinds expected from a melt approaching this composition. A critical consideration of the anchi-eutectic theory is needed with the aid of more accurate data.

Prof. S. J. Shand shows that it is possible to classify practically all varieties of glassy igneous rocks by simple methods of specific gravity, refractive index, flame reaction, and microchemical tests, not involving quantitative chemical analysis (*Geol. Mag.*, lxvi, 1929, pp. 116–20). He illustrates with a full description of the rhyodacite glass of Wormit, Fifeshire.

Prof. P. Niggli has performed a valuable task in summarising the principal ideas in the uncompleted Part II of the late

Hommel's work, "Systematische Petrographie auf genetischer Grundlage" (*Schweiz. Min. u. Petr. Mitth.*, Bd. vii, 1927, pp. 54–97). Hommel was a chemist turned petrographer; but in his first volume valuable and penetrating petrological ideas were unfortunately obscured by a most unnecessarily elaborate mode of formulation of igneous rock analyses. The MSS. he left incomplete on his death show that he intended to deal with processes in magma, and the consolidation, differentiation, origin, nomenclature, and classification of igneous rocks, which views are ably expounded by Prof. Niggli.

A further paper by Niggli deals with the significance of igneous rock analyses on the basis of the molecular values of the constituents treated as oxides (*Schweiz. Min. u. Petr. Mitth.*, vii, 1927, pp. 116–33). In this work he outlines a unique form of triangular diagram in which the distribution of silica as between the normative leucocratic and melanocratic constituents is shown. In the writer's opinion this mode of representation of an igneous rock analysis bids fair to be of great value in discussions of classification and distribution.

Niggli's methods of representing rock analyses by manipulation of the molecular values of the oxide constituents, which is admittedly based on the American Norm calculation, and the variation diagrams based thereon, have obtained a great vogue on the Continent, as is shown by the following works: C. Burri, "Chemismus und provinziale Verhältnisse der jungeruptiven Gesteine des pazifischen Ozeans und seiner Umrandung," *Schweiz. Min. u. Petr. Mitth.*, vi, 1926, pp. 115–99. "Kritische Zusammenfassung unserer Kenntnisse über die Differentiationstypen postmesozöischer Vulkangebiete," *ibid.*, vii, 1927, pp. 254–310. H. Jung, "Der chemischen und provinzialen Verhältnisse der jungen Eruptivgesteine Deutschlands und Nord Böhmens," *Chem. d. Erde*, **3**, 1927, pp. 137–340. "Die provinziale Stellung der permischen Eruptivgesteine des Thüringer Walde," *Beitr. z. Geol. v. Thüringen*, **2**, 1928, pp. 49–66.

Much of the work of Niggli and his disciples is directed to the elucidation of the geographical distribution and classification of igneous rocks. Niggli maintains three chief distributional and chemical categories, the Atlantic and Pacific with their old significations, and the Mediterranean of more or less intermediate characters. The artificiality of this classification is shown by the frequency with which Niggli and his followers have to admit the existence of "Mischprovinzen," and it is therefore regrettable that the author of the article "Petrology" in the new *Encyclopædia Britannica* should have given countenance to it.

According to the comprehensive investigation of Dr. P. Esenwein ("Zur Petrographie der Azoren," *Inaug. Diss.*

Zürich, 1928; *Zeitsch. f. Vulk.*, Erg.-H., 1928, 127 pp.), the lavas of the Azores consist of trachytes, trachyandesites, basalts, trachybasalts (some of which carry essexitic inclusions), and olivine-rich basalts (= oceanites), an assemblage of feebly " Atlantic " characters. We should prefer to record it as a typical " oceanic island " assemblage. A large number of new chemical analyses is given.

In his essay on " Ore Deposits of Magmatic Origin : Their Genesis and Natural Classification," which has now been translated from the German by Dr. H. C. Boydell (T. Murby & Co., 1929, 93 pp.), Prof. Niggli extends his views on distribution to ore deposits which have originated directly from igneous action. He shows the possibility and value of classifying such ore-deposits according to petrogenetic provinces. So far as ore deposition is connected with magmatic activity, it is a problem in the physical chemistry of magmatic solutions; and is, therefore, as are the parent igneous rocks, related to the tectonic conditions that prevailed during the period of formation.

Dr. R. Pilz and M. Donath have published an excellent summary and correlation of the igneous rocks and ore deposits of the little-known Bolivian Andes (*Zeitschr. f. Prakt. Geol.*, **37**, 1929, pp. 125–38). Magmatic activity of Mesozoic-Kainozoic times began with diabase-porphyrites. It continued with granular and porphyritic rocks of intermediate characters, and ended with outpourings of andesitic and basaltic lavas. Numberless ore deposits have been formed from the components of the magmatic residual solutions by reactions on the igneous and sedimentary rocks with which they have come into contact.

Some years ago Dr. H. S. Washington determined the average density and average chemical composition of the igneous rocks of various regions, and found that the average density varied inversely as the average altitude of the region, thus indicating an isostatic relation. He also showed that the average European igneous rock is heavier than the average North American rock. By a statistical method Prof. A. Johannsen has now shown (*Journ. Geol.*, xxxvi, 1928, pp. 283-6) that each *kind* of igneous rock in America is lighter, both in colour and weight, than the corresponding rock in Europe.

Prof. A. Lacroix has published an extremely valuable summary of the petrography of the volcanic islands of Southern Polynesia (*Mem. Acad. Sci., Paris*, lix, 1927, 82 pp.). He shows that all the volcanic rocks of this region have an alkaline cast, soda usually dominating over potash, although nepheline only rarely becomes an actual constituent. The common rock association of oceanic islands, trachyte, trachyandesite,

phonolite, trachybasalt, basalt, ankaramite, and oceanite, is everywhere dominant. Tahiti is rich in nepheline-bearing rocks, and has, moreover, a plutonic core of nepheline-syenite and theralite. On the whole, however, there is a great predominance of basaltic types. The work is enriched by numerous new chemical analyses, and further papers supplement petrographic information on this interesting region (A. Lacroix, "Nouvelles observations sur les laves des les Marquises et de l'île Tubuai," *C. R. Acad. Paris,* **187**, 1928, pp. 365–9. "Nouvelles observations sur les laves des îles Sous-le-Vent de l'archipel de la Société," *ibid.*, pp. 397–401).

Two further papers on the petrography of oceanic islands have been published by Dr. H. S. Washington and Miss M. G. Keyes. These authors have continued their survey of Hawaiian petrography with a study of the island of Maui (*Amer. Journ. Sci.*, xv, 1928, pp. 199–200). Maui is constructed of the remains of two volcanoes, the lavas of which are predominantly andesitic (in Washington's sense) and basaltic, with a distinct alkaline tendency. There are also some trachytic and nephelinic rocks on the island, as also many flows of basalts extremely rich in olivine (" picrite-basalt " or oceanite).

The same authors describe andesine-basalt and palagonite-tuff from the Galapagos Islands, with two new analyses (*Journ. Wash. Acad. Sci.*, **17**, 1927, pp. 538–43). They discuss observations which indicate that trachyandesites and trachytes also occur in these volcanic islands. Hence both the Galapagos and the Hawaiian islands fall into line with the majority of oceanic islands in regard to their lithological constitution.

British igneous rocks have received a considerable amount of attention lately. Mr. P. J. Robinson has described a remarkable dyke of pitchstone-porphyry at Penrioch, Arran, which contains numerous xenoliths of tholeiite (*Trans. Geol. Soc. Glasgow*, xviii, 1928, pp. 295–9). This occurrence provides one more example of the close association of pitchstone and tholeiite that characterises the Arran volcanic focus.

Dr. F. Walker has supplemented his earlier paper by a further description of the small plutonic masses which pierce the Lower Palæozoic strata of South-eastern Scotland (*Geol. Mag.*, lxv, 1928, pp. 153–62). These intrusions are probably of Old Red Sandstone age. Four new analyses of granodiorite and quartz-diorite from Priestlaw, and granodiorites from the Spango Water, are recorded.

The igneous intrusions between St. Andrews and Loch Leven, described by Dr. F. Walker and Dr. J. Irving (*Trans. Roy. Soc. Edin.*, lvi, 1928, pp. 1–17), are mainly sills, and comprise olivine-dolerites, teschenites, nepheline-basanites, monchiquites, and quartz-dolerites. The first four of these are probably

consanguineous; but the quartz-dolerites, which are very abundant, also occasionally contain analcite, the primary or deuteric origin of which is left in doubt. The most important geological result which has accrued from the recent work is the inclusion of the twin Lomond Hills amongst the volcanic necks of the district. All the igneous rocks described are probably of Carboniferous ages.

Dr. S. I. Tomkeieff has shown that the Calton Hill (Derbyshire) consists of a volcanic complex exhibiting two phases of eruption (*Quart. Journ. Geol. Soc.*, lxxxiv, 1928, pp. 703–18); an effusive phase to which is assigned the volcanic tuffs and agglomerate, and also a vesicular lava, and an intrusive phase represented by a mass of analcite-basalt. Numerous inclusions of peridotite are found within the latter, and these are ascribed to the shattering of a deep-seated peridotite plug. The history of this vent appears to be identical with that of many of the Carboniferous and Permian vents of Scotland.

A band of basaltic lavas near Cockermouth (Cumberland), formerly ascribed to the Ordovician, now turn out to be of Carboniferous Limestone age (T. Eastwood, *Summ. Prog. Geol. Surv. for* 1927, 1928, Part II, pp. 15–22). They consist of olivine-basalts of Dalmeny type, along with some fine-grained tholeiitic facies, which closely resemble the Carboniferous lavas of the Midland Valley of Scotland.

A critical revision of the classification of Scottish Carboniferous olivine-basalts and mugearites has been undertaken by A. G. MacGregor of the Geological Survey of Scotland (*Trans. Geol. Soc. Glasgow*, xviii, Pt. 2, 1928, pp. 324–60), with a view to clearing up ambiguities that have arisen in the course of recent work, and of establishing definitions of the various types which would be most useful for field work. It is strongly emphasised that these basalts form a continuous series; and although the majority of the rocks may be grouped around a number of arbitrary types, there are naturally many transitional varieties. Mr. MacGregor has collected the analyses of these rocks, and has provided an excellent tabular summary of the adopted classification.

Mr. J. E. Richey's paper on the "North Ayrshire Sequence of Calciferous Sandstone Volcanic Rocks" (*Trans. Geol. Soc. Glasgow*, xviii, Pt. 2, 1928, pp. 247–58) illustrates the value of the classification developed by the Scottish Survey (see preceding paragraph), since he has been enabled to establish the succession and structure of the complex North Ayrshire lava plateau. A lower group of mainly macro-porphyritic basalts is separated from an upper group of micro-porphyritic basalts, which locally contain two intercalations of rhyolite, trachyte, and trachyandesite lavas.

In a paper entitled "A Further Contribution to the Petrography of the Late-Palæozoic Igneous Suite of the West of Scotland" (*Trans. Geol. Soc. Glasgow*, xviii, Pt. 2, 1928, pp. 259–94), the writer gives a detailed description of the lavas and tuffs, the volcanic vents and their intrusions, of the Central Ayrshire region. Four new chemical analyses of olivine-basalt (Dalmeny type), analcite-basanite, nepheline-basalt, and bekinkinite, are published. A systematic treatment of the fragmental infillings of the volcanic vents is provided, and the petrography of the Permian volcanic vents of Ayrshire is described on this basis.

The analcite-syenite of Ayrshire occurs as stratiform bands, schlieren, and veins, within differentiated sills of crinanite or olivine-analcite-dolerite (G. W. Tyrrell, *Quart. Journ. Geol. Soc.*, lxxxiv, 1928, pp. 540–69). The variations within the intrusions are ascribed to simple crystallisation-differentiation aided by gravitative settling of titanaugite-ilmenite intergrowths. Several reaction-series, continuous and discontinuous, are traced. It is shown that a certain amount of lime must have been stored up in the residual magmatic liquor along with the usual soda, potash, silica, water, and volatiles, leading to the final crystallisation of analcite, soda-lime zeolites, and prehnite. An hypothesis for the development of schlieren and veins in sills, by the effects of the varying incidence of pressure due to the superincumbent column of rock upon a crystal-mesh filled with interstitial liquid, is framed.

An important paper by Prof. A. Holmes and Dr. H. F. Harwood deals with the age and composition of the Whin Sill and related dykes of the North of England (*Min. Mag.*, xxi, 1928, pp. 493–542). It provides a very full survey of the geological and petrological information that is available on that classic intrusion, along with much new work, and a number of excellent chemical analyses by Dr. Harwood. The mineralogy of quartz-dolerites of Whin Sill Type is fully treated. As regards petrogeny, the most significant conclusion that the authors have come to is that, under appropriate continental conditions, basaltic magma differentiates towards the Whin Sill magma-type, and thereafter remains almost unchanged until the final residuum is squeezed out as the comparatively rare leucocratic veins and patches. This implies deep-seated cooling of the parental magma before its injection into the crust. Other evidence leads to the view of a slow eastward movement of the magma, which might result from convection currents in the substratum beneath the sial layer.

Dr. S. I. Tomkeieff's "Contribution to the Petrology of the Whin Sill" (*Min. Mag.*, xxii, 1929, pp. 100–20) aims at describing unusual developments such as the coarse-grained

bands and acid segregations within the sill. He shows that the coarse gabbroidal variety occurs in the forms of bands, segregation veins, and spheroids, always sharply separated from the normal rock. The coarse variety is of the same chemical composition as the normal type, but is richer in micropegmatite and quartz. Hence the two rock types are heteromorphs in Lacroix's sense. Dr. Tomkeieff supposes a sort of "liquation differentiation" within the magma prior to its injection into the strata, whereby patches of volatile-rich magma were formed, within which crystallisation took a different course than within the drier normal magma.

In their comprehensive paper on "The Tholeiite Dykes of the North of England," Prof. A. Holmes and Dr. H. F. Harwood (*Min. Mag.*, xxii, 1929, pp. 1–52) put forward much new petrographic work and apply the results of eight new chemical analyses, along with those already published, to a thorough discussion of the classification of tholeiites. The tholeiite suite, as a whole, is found to be composed of four distinct classes of material: (*a*) anorthite crystals and aggregates in extremely variable amounts; (*b*) a framework of basaltic minerals; (*c*) iron-rich mesostasis; and (*d*) quartz—alkali-felspar mesostasis. The authors show that crystallisation-differentiation fails to explain the variations of these rocks, which behave as basaltic magmas that, in some cases, have been merely diluted with a quartz—alkali-felspar mixture. They conclude that tholeiites are the products of the partial mixing of basaltic and granitic magmas generated at their respective "sima" and "sial" levels in the crust, thus returning to Bunsen's conception that two fundamental magmas, basic and acid, are the parents of many groups of igneous rocks, notably those of Iceland and the Hebrides.

Dr. H. P. T. Rohleder's examination of the Mourne Mountains granite massif (*N. J. f. Min.*, *Beil.-Bd.*, lix, Abt. A. 1929, pp. 297–314), recently described by Mr. J. E. Richey (see SCIENCE PROGRESS, July 1928, p. 45), has led him to the view that it is a structural unit, and that the slight differences which caused Mr. Richey to distinguish four phases of intrusion are due to differentiation processes operating during the cooling of the mass. He correlates the older, pre-granitic, and younger, post-granitic, series of basic dykes with the older and younger basaltic lava series respectively of Antrim.

The basaltic lavas of the Faroes, according to J. B. Simpson (*Geol. Mag.*, lxv, 1928, pp. 510–17) are conveniently divisible into a non-porphyritic lower group and a porphyritic upper group, separated by a thin sedimentary zone which contains a coal seam. The chemical analysis of a member of the lower group agrees well with the Mull plateau-magma type, whereas

the analysis of a basalt from the porphyritic group corresponds to that of the porphyritic central basalt type of Mull.

In a paper on the major intrusions of South-eastern Iceland, Dr. L. Hawkes, H. K. Cargill, and J. A. Ledeboer (*Quart. Journ. Geol. Soc.*, lxxxiv, 1928, pp. 505–39) describe the three great intrusive masses of the Vestur Horn, Oster Horn, and Slaufrudal, with eight new chemical analyses. The first two are gabbro-granophyre complexes; the Slaufrudal stock, which is the largest known intrusion in Iceland, is a layered mass of granites, granophyres, and granite-porphyries. All these masses are replacive stocks with steep sides, domed roofs, and no visible floors. It is suggested that " the lighter supporting substratum which isostasy demands for Iceland is built up of acid rocks, and the intrusion of acid magma beneath it has saved it from the general collapse of the North Atlantic plateau [of basaltic composition] which occurred towards the close of the Tertiary Era."

From the material left behind by the late Prof. J. Romberg, Dr. C. Burri has compiled a short résumé of his latest geological observations, mineralogical results, and a table showing the age-sequence and kind of differentiation of the Triassic igneous rocks of Predazzo and Monzoni (Tyrol). Most welcome of all is the publication of nineteen new analyses of the rocks of this classic region. In the same paper Dr. Burri offers a review of the present stage of knowledge concerning differentiation in these igneous masses, which have caused some perplexity to petrographers because of their intermediate characters. They are characteristic examples of Niggli's " mischprovinzen " (*N. J. f. Min.*, *Beil.-Bd.*, lviii, Abt. A., 1928, pp. 109–40).

Dr. C. Burri describes the interesting series of alkaline igneous rocks at Alter Pedroso in Portugal, which has similarities to that of Evisa (Corsica), and Rockall (North Atlantic). The petrographic range, from syenite, through lusitanite (mesocratic) to pedrosite (melanocratic), is produced by quantity variations between two groups of minerals, alkali-felspar and analcite on the one hand, aegirine and osannite (soda-hornblende) on the other. The differentiation is clearly due to gravitative subsidence of osannite, and, to a less extent, of ægirine. All the rocks show protoclastic deformation. The age of the series may be Kainozoic, but there is also a petrographical resemblance to the well-known nepheline-bearing gneisses of Cevedaes, which are of a much greater age (*Schweiz. Min. u. Petr. Mitth.*, viii, 1928, pp. 374–436).

ENTOMOLOGY. By H. F. Barnes, B.A., Ph.D., Rothamsted Experimental Station, Harpenden.

General Entomology.—A very excellent little book entitled *Insects and Industry*, by J. W. Munro (80 pp., 1929, *Benn's*

Sixpenny Library, No. 95) deals, in a series of well-written essays, with introductory matter, damage and loss caused by insects, origins and causes of insect outbreaks, the biological basis of insect control, the control of insects by chemical and physical means and legislation as a control measure. A. Gibson and C. R. Twinn have produced (*Dominion of Canada, Dept. Agric.*, Bull. 112 (new series), 1929, 84 pp., 90 figs.) a very useful popular summary of our knowledge of household insects and their control. This bulletin is well illustrated. In *Principles of Forest Entomology*, by S. A. Graham (*McGraw Hill Book Company, Inc., New York*, 1929, 339 pp., 149 figs., and frontispiece) we have, apart from a few mistakes, a good textbook that could with advantage be used by economic entomologists and ecologists as well as students and teachers of forest entomology. The author, after dealing in successive chapters with introductory remarks and an historical review, has three very interesting chapters on biotic potential, environmental resistance, and insect abundance. Chapters on control, direct and indirect, follow. It is this half of the book that is the more valuable. Then there are chapters on ecological groups of insects such as the leaf-eating insects, meristem insects of the terminal parts, etc. The book concludes with chapters on insectivorous parasites and predators. *Pests and Diseases of Queensland Fruit and Vegetables* is the title of a handbook by R. Veitch and J. H. Simmonds (Government Printer, Brisbane, 1929, 198 pp., with 64 plates).

Further fascicles of the British Museum (Nat. Hist.) monograph on the *Insects of Samoa* have recently been issued, making up to date 20 fascicles since 1927 when publication commenced. The monograph does not restrict itself to dealing with insects, but also includes other arthropoda.

A new series of monographs on plant protection (*Monographie zum Pflanzenschutz*, Berlin, Verlag der Julius Springer) has made its appearance this year under the editorship of Dr. H. Morstatt. Three volumes are to hand so far. Number 1, *Der Apfelblattsauger, Psylla mali Schmidberger*, by W. Speyer (1929, vii + 127 pp., 59 figs., 1 coloured plate); number 2, *Die Rübensblattwanze, Piesma quadrata Fieb.* by J. Wille (1929, iii + 116 pp., 39 figs.); number 3, *Die Forleule, Panolis flammea Schiff.* by H. Sachtleben (1929, iii + 160 pp., 35 figs., 1 coloured plate). These volumes are excellently printed and the coloured plates are good. Each volume considers the insect in question from many points of view, especially the monograph on *Panolis*, which may be considered a complete ecological study.

The mind of an insect is discussed in a very interesting manner under four headings—the sensory basis of the mind,

reactions of insects to environmental stimuli, instincts and intelligence—by R. E. Snodgrass (*Smithsonian Rept. for* 1927, 1928, 387–416). The same author deals with the morphology and evolution of the insect head and its appendages (*Smithsonian Misc. Coll.*, **81**, 1928, 158 pp., with 57 figs.).

J. G. H. Frew, in an attempt to correlate the processes of insect metamorphosis with certain of the more obvious metabolic changes characteristic of larval and pupal life, finds that there is no reliable evidence which indicates that the metabolic processes of the two sexes are different from each other (*British Jl. Expt. Biol.*, **6**, 1929, 205–18). M. O. Lee reviews the current knowledge of respiration in insects (*Qt. Rev. Biol.*, **4**, 1929, 213–32). T. J. Headlee and R. C. Burdette (*Jl. N.Y. Ent. Soc.*, **37**, 1929, 59–64) have shown that insects are killed when exposed to high-frequency waves and that the lethal effect is due to the development of an internal heat of lethal degree. Our knowledge of the chemical senses of insects is discussed in an article by D. E. Minnich (*Qt. Rev. Biol.*, **4**, 1929, 100–12).

It has been the current idea ever since Van Gehuchten's paper in 1890 to consider the granular protrusions seen on the epithelial cells in the mid-gut of insects as sécretion vesicles. Recently Shinoda (1926 and 1927) accepted this view once again. Now H. Henson (*Q.J.M.S.*, **73**, 1929, 87–105), in a study of the structure and development of the mid-gut in five instars of *Vanessa urticæ*, shows there is an alternative view—namely, that the formation of the "secretion" vesicles is really a process of cell disintegration due to wear and tear or to the incidence of metamorphosis.

Some underlying principles of natural control, with special reference to insects, are discussed in an important article by W. R. Thompson (*Parasitology*, **21**, 1929, 269–81). The same writer (*loc. cit.*, 180–8), in a further paper on the theoretical effect of random oviposition on the action of entomophagous parasites as agents of natural control, deals with the hypothesis that when several parasites enter a single host one and one only will develop successfully. It is shown that, provided the reproductive rate is decidedly greater than that of the host, the effect on the time required for the extermination of the host will be very little if at all greater than when superparasitism causes merely a loss in efficiency. Multiple parasitism and its relation to the biological control of insect pests is discussed by H. S. Smith (*Jl. Ent. Res.*, **20**, 1929, 141–9).

Following up H. M. Morris's recent work on the insect fauna of permanent pasture and arable land at Rothamsted, E. E. Edwards (*Ann. App. Biol.*, **16**, 1929, 299–323) has made a survey of the insect and other invertebrate fauna of permanent pasture and arable land of certain soil types at Aberystwyth.

He finds that while qualitative differences may occur between the fauna of the several areas, the main differences concern rather the proportions in which the various organisms occurred; also that there is decidedly less difference both qualitatively and quantitatively between the animal associations than there is between the plant associations.

E. Percival and H. Whitehead (*Jl. Ecology*, **17**, 1929, 287–314) have made a thorough quantitative study of the fauna of some types of stream-bed. It is interesting that the authors in discussing the food relations of some of the aquatic invertebrates came to the conclusion that those animals which form the greater part of the fauna derive their nutriment chiefly from the Algæ, especially the Diatoms and Desmids. The numbers of species existing on other material is small. Further notes on insect inhabitants of bird houses are given by W. L. McAtee (*Proc. Ent. Soc. Washington*, **31**, 1929, 105–11).

Some useful methods of technique applicable to entomology are described by A. D. Imms (*Bull. Ent. Res.*, **20**, 1929, 165–71). C. A. Thomas (*Ent. News*, **40**, 1929, 222–5) deals with a method of rearing mushroom insects in bottles containing mushroom spawn. The use of cellophane for insect cages is briefly mentioned by L. M. Smith (*Jl. Econ. Ent.*, **22**, 1929, 705). This substance is used commercially to cover chocolate-boxes, etc., and is permeable to atmospheric moisture. Its use will therefore get rid of the difficulty of glass "sweating." I. Trägårdh (*Bull. Ent. Res.*, **20**, 1929, 245–50) has described some methods of investigating the fauna of tree-stumps.

Orthoptera.—The development of the genitalia and genital ducts in insects is being studied by R. I. Nel. In his first paper (*Q.J.M.S.*, **73**, 1929, 25–85) he has dealt with the female of Orthoptera and Dermaptera and has deduced an hypothetical ancestral condition of the oviducal system and accessory organs for insects in general.

Some cases of maternal care displayed by cockroaches and their significance are considered, both from the view that cockroaches are "solitary" insects and also from the view that they show an approach to a social mode of life, by H. Scott (*Ent. Mo. Mag.*, **65**, 1929, 218–22). As Dr. Scott states, if they are considered "solitary" insects the maternal care of young belongs to a class of rare phenomena; assuming the other view, this maternal care is of importance in connection with their close structural and phylogenetic relationships with the Isoptera.

An important résumé of all that is known concerning the genetics of the Tettigidæ by R. K. Nabours has recently been published (*Bibliographia Genetica*, **5**, 1929, 27–104) with 2 coloured plates).

Coleoptera.—The rostrum of *Leptopalpus rostratus* F. has been described as a new type by E. Handschin (*Zeits. f. Morph. u. Ökol. d. Tiere*, **14**, 1929, 513–21). W. E. H. Hodson (*Bull. Ent. Res.*, **20**, 1929, 5–14) has dealt with the bionomics of *Lema melanopa* L. Further valuable contributions to our knowledge of the biology of some flea-beetles have been made by H. C. F. Newton (*Jl. S.E. Agric. Coll., Wye*, **26**, 1929, 154–64). In this paper the following species received attention: *Epitrix atropæ* Foudras (the Belladonna Flea-beetle), *Plectroscelis* (*Chætocnema*) *concinna* Marsh, *Chætocnema aridella* Payk., *Psylliodes attenuata* Koch (the Hop Flea-beetle), *P. affinis* Payk. (the Potato Flea-beetle) and *P. chrysocephala* L. (the Cabbage Stem Flea-beetle).

No. 20 of *Faune de France* (1929, vii. + 167 pp., 71 figs.) by F. Picard deals with the Cerambycidæ. A useful feature of this volume is the list of Hymenopterous parasites.

Lepidoptera.—W. H. Thorpe (*Jl. Linn. Soc., Zool.*, **36**, 1929, 621–34) brings forward good evidence as a result of breeding experiments for supposing that the hawthorn and apple forms of *Hyponomeuta padella* L. are distinct biological races. D. F. Barnes and S. F. Potts (*Jl. Econ. Ent.*, **22**, 1929, 423) have announced the presence of a disease of the gipsy moth differing from the polyhedral disease.

Hemiptera.—In view of virus diseases it is important to know what insects with piercing or biting mouth-parts occur on plants which suffer from such diseases. W. Steer (*Entomologist*, **62**, 1929, 101–8) has made a list, together with notes, of the Capsidæ found on the genus *Rubus*. Observations on the insertions of the eggs of the Apple Capsid (*Plesiocoris rugicollis* Fall.) with particular attention being paid to the hardness of the wood of certain varieties of apple, have been made by M. D. Austin (*Jl. S.E. Agric. Coll., Wye*, **26**, 1929, 136–44). Apparently the hardness of the wood has little influence on the depth of the egg insertion. W. Steer (*Ent. Mo. Mag.*, **65**, 1929, 103–4) has made some observations on *Anthocoris nemorum* L. on *Rubus*, a bug that is often a very destructive predator in insectary breeding cages. F. G. Sarel-Whitfield gives the result of a morphological study of the Sudan Millet bug (*Agonoscelis versicolor* F.) and its relation to the bionomics of the insect (*Bull. Ent. Res.*, **20**, 1929, 209–24).

The presence of hæmoglobin is of rare occurrence in insects, up to the present only being found in certain larval Chironomidæ, *Gastrophilus* larvæ and in the Notonectid *Buenoa*. Recently Miss M. D. H. Brindley (*Trans. Ent. Soc. London*, **77**, 1929, 5–6) has discovered it in the male of the large water boatman, *Macrocorixa geoffroyei* Leach, in the accessory gland of the genital system. Its function is unknown.

A very remarkable type of aquatic Hemiptera has been described by T. Esaki (*Ann. & Mag. Nat. Hist.*, **4**, 1929, 341–6) and ecological notes have been added (*loc. cit.*, 346–9). This Mesoveliid bug (*Speovelia maritima* Esaki) was found on the moist walls of a littoral cave in Japan, and bears adaptation to this form of life in that the eyes and pigment in their peripheral area are reduced. This discovery is interesting among other things because Jeannel, in his *Faune cavernicole de France* (1926), denied the existence of cave-inhabiting Mesoveliid bugs. An unidentified cricket was also found, while the rest of the fauna comprised bats and their parasites, mosquitoes and other small Diptera. The particular " niches " these bugs occupy in the ecological food chains remain to be discovered.

An important paper on the taxonomy, phylogeny, and distribution of New Zealand Cicadas has been prepared by J. G. Myers (*Trans. Ent. Soc., London*, **77**, 1929, 29–60). The section on the ethological (ecological) distribution should be of especial value.

F. V. Theobald (*Jl. S.E. Agric. Coll., Wye*, **26**, 1929, 117–23, 1 coloured plate) gives a very useful diagnosis, together with a good coloured illustration, of the aphids of *Ribes* and Apple. The determination of types of individuals in aphids, as well as in rotifers and cladocera, is reviewed by A. F. Shull (*Biol. Rev.*, **4**, 1929, 218–48) in the light of recent research.

A further paper on the potato virus diseases by K. M. Smith (*Ann. App. Biol.*, **16**, 1929, 209–29) shows that *Myzus persicæ* Sulz. is an efficient carrier of the leaf-roll virus, while six other species of insects gave negative results. The virus can be disseminated by feeding the aphid either on the sprouts of the tuber, on the leaves and shoots of the growing plant, or on the stem alone. P. A. Murphy and R. McKay (*Sci. Proc. Roy. Dublin Soc.*, **19**, 1929, 341–53) have obtained transmission of the leaf-roll of potato with *M. persicæ*, *M. pseudosolani*, and *Macrosiphum solanifolii*, but with the latter two aphids only occasionally.

The possibility of a biological control of *Pseudococcus citri* in Palestine by indigenous predators has been studied by F. S. Bodenheimer and M. Guttfeld (*Zeits. f. ang. Ent.*, **5**, 1929, 67–136). While two Cecidomyidæ have been found to prey on the mealy bug, the Hemerobiid, *Sympherobius amicus* Nav., seems to offer most hope.

An important morphological study of the head and thorax of *Psylla mali* has been made by H. Weber (*Zeits. f. Morph. u. Ökol. d. Tiere*, **14**, 1929, 59–165).

Hymenoptera.—The Proctotrypidæ are a very neglected group of parasitic Hymenoptera, and an important paper describing the life-history of *Phænoserphus viator*, an endopara-

site of the larva of *Pterostichus niger*, a Carabid, by L. E. S. Eastham (*Parasitology*, **21**, 1929, 1–21) is very welcome.

C. P. Claussen (*Proc. Ent. Soc. Washington*, **31**, 1929, 67–79) has made some biological studies of *Poecilogonalos thwaitesii* Westw., which is parasitic in the cocoons of *Henicosphilus* (Hymen.: Trigonalidæ). Oviposition occurs on the leaves of a wide range of plants, but the manner of hatching and of the planidium gaining access to its primary host are still unknown.

J. Waterston (*Parasitology*, **21**, 1929, 103–6) has described a new Chalcidoid parasite of the *Miscogasteridæ* family which has been bred from a flea larva, *Ceratophyllus wickhami*, the flea of the introduced grey squirrel.

The results of five years' study on the biometry of the most important European races of the honey-bee, mostly by Russian investigators, are brought together and reviewed by W. W. Alpatov (*Qt. Rev. Biol.*, **4**, 1929, 1–58). E. F. Philipps (*Cornell Univ. Agric. Expt. Sta.*, Mem. 121, 1929, 52 pp., with 40 tables), working on the variation and correlation in the appendages of the honey-bee, finds that drones are considerably more variable than the worker bees and that irregularity in cell size has a direct effect by bringing about a lesser degree of homogeneity in the material; this may be interpreted as, the size of cell directly affects the sizes of various parts of the body. Abnormalities in venation support this great variability of the drones and suggest atavistic tendencies in some cases, while the abnormalities in others appear to be due to splitting of normal veins. The highest correlations are found in the plane of bilateral symmetry, less in homologous and analogous parts of consecutive segments, and less still between parts of appendages lying in a dorsoventral plane. A recording scale for bee-hives which is in use at Rothamsted is described and illustrated by D. M. T. Morland (*Ann. App. Biol.*, **16**, 1929, 294–8).

Diptera.—This year the British Museum (Nat. Hist.) has started issuing a monograph on what is known concerning the Diptera of Patagonia and Southern Chile. The final impetus to this work was a recent collecting trip by Mr. F. W. Edwards of the British Museum and Mr. R. C. Shannon representing the Bacteriological Institute of the National Department of Hygiene, Argentina. Part 1 consists entirely of *Craneflies* by C. P. Alexander (1929, xvi + 240 pp., 12 plates), who finds marked Australasian affinities. Two fascicules of the second part have also appeared: the first, *Psychodidæ*, by A. L. Tonnoir, the second, Blepharoceridæ, by F. W. Edwards. Mr. Tonnoir finds the greatest affinities with New Zealand fauna, while all except one of the species dealt with are described as new. Mr. Edwards takes the opportunity to discuss the affinities,

morphology, and classification of the Blepharoceridæ. An illustrated account of the larvæ and pupæ of the primitive genus *Edwardsia*, together with descriptions of new species, is also included.

During recent years some interesting facts about sex ratio have come to light in the investigations of C. W. Metz. He found that in two species of *Sciara* females sometimes produced all male families, at others all female families, and that selective segregation of the chromosomes took place. Recently (*Proc. Nat. Acad. Sci.*, **15**, 1929, 339–43) he has found that this occurs in a third species. C. W. Metz and M. S. Moses (*loc. cit.*, **14**, 1928, 928–32) showed that the female is responsible for the sex ratio and that female-producing females regularly give the two types of daughters in approximately equal numbers. The unisexual progenies which occur in these species are due to selective elimination of the gametes (sperm) (C. W. Metz, *Amer. Nat.*, **63**, 1929, 214–28). One very important deduction arising out of this work is that there appears to be evidence that there is a disposition in *Sciara*, at any rate, for an intermediate stage of sex chromosome mechanism, intermediate between Diptera, as exemplified by *Drosophila*, and Lepidoptera.

Pædogenesis has been known to occur in one sub-family of Cecidomyidæ, the Lestreminæ, for a long time. H. F. Barnes (*Ent. Mo. Mag.*, **65**, 1929, 138–9) now announces its appearance in a second subfamily, the Campylomyzariæ.

Following a study of the reaction of mosquito larvæ to pH., M. E. MacGregor's chief conclusion (*Parasitology*, **21**, 1929, 132–57) is that in most instances in various localities the larvæ of certain species do actually show a restriction to waters exhibiting a pH index within a definite short range, and that the pH index is consequently often a reliable index as to whether the chemical and biological group associations will favour or preclude the successful development of such larvæ. In an experiment on the ovulation requirements of *Culex pipiens* C. G. Huff (*Biol. Bull.*, **56**, 1929, 347–50) has been successful in getting a female to lay viable eggs within 18 hours after emerging and without having taken food of any kind. He also obtained viable eggs after feeding the insect on various substances, including potato, carrot, and apple juices. This is important in view of the numerous statements that mosquitoes require a meal of blood before laying viable eggs.

A. Cuthbertson (*Ent. Mo. Mag.*, **65**, 1929, 141–5) summarises data concerning the mating habits and oviposition of craneflies.

Following Mast's evidence that the sensitivity of the ommatidia in the compound eyes of insects increases as one

proceeds from the anterior to the posterior end, and that the difference in sensitivity of different regions of the eye is marked, W. L. Dolley and J. L. Wierda (*Jl. Exper. Zool.*, **53**, 1929, 129–39) find that in *Eristalis tenax* the ommatidia at the centre of the compound eye are more than 55 times as sensitive to light as those near the anterior end of the eye. They also find that difference in degree of light reaction is one of the main factors involved in the variability in the reactions of various specimens under identical conditions.

It has been known for a long time that there occur remarkable unicellular glands in *Eristalis tenax* larvæ, and as far back as 1886 Gazagnaire maintained that their function was to supply an oiling fluid for lubricating the breathing tube. Wahl in 1889 concluded, solely by examination of the secretion's optical properties, that it prevented water from adhering to or entering the end of the respiratory tube. Now W. L. Dolley and E. J. Farris (*Jl. N.Y. Ent. Soc.*, **37**, 1929, 127–33) in a detailed description of the glands show that the function probably is to furnish an oily secretion which prevents water from entering the end of the tail at the surface of the liquid as Wahl suggested.

V. B. Wigglesworth has made a study of structure and function in the digestion of the tsetse-fly (*Parasitology*, **21**, 1929, 288–321). The anatomy, histology, and digestive enzymes of the mid-intestine have been investigated and an attempt has been made to determine the functions of the various parts and to observe the changes to which they are subject during the digestion of blood.

Other Orders.—K. J. Morton (*Ent. Mo. Mag.*, **65**, 1929, 128–34) has made notes on the genera *Leuctra* and *Capria*, together with descriptions of three new species.

J. V. Pearmain (*Ent. Mo. Mag.*, **65**, 1929, 104–9) has described three new species of Psocoptera from warehouses, thus indicating that species may be introduced into this country.

E. Handschin (*Trans. Ent. Soc., London*, **77**, 1929, 15–28), after studying a small collection of Abyssinian Collembola, comes to the conclusion that the connection of the hitherto nearly isolated fauna of the Seychelles with that of the African continent becomes more firmly established. H. Scott, in a footnote, states that this is remarkable as in some groups of insects, particularly Coleoptera, the affinities of the endemic Seychelles fauna appear to be predominantly Oriental, African elements being almost entirely absent. E. Handschin deals with the Collembola (*Sch. d. Phys.-ökon. Ges. z. Konisberg i. Pr.*, **65**, 1928, 124–54) in a series of papers that are being brought out by A. Dampf and E. Skevarva entitled *Fauna des Zehlauhochmoores in Ostpreussen*. New species of Collembola from

Southern Rhodesia have been described by H. Womersley (*Ent. Mo. Mag.*, **65**, 1929, 152–8).

Surface caking of the soil seems to act inimically to soil-pupating species of thrips in a series of experiments by Miss E. I. MacGill (*Ann. App. Biol.*, **16**, 1929, 288–93). This is the fourth paper on the biology of the Thysanoptera with reference to the cotton plant that has appeared in this periodical in the past few years. R. S. Bagnall (*Marcellia*, **25** (1928), 1929, 184–204) has described some new genera and species of Australian gall-dwelling thrips (Thysanoptera : Tubulifera).

AGRICULTURE: ANIMAL NUTRITION.

By Herbert Ernest Woodman, M.A., Ph.D., D.Sc., School of Agriculture, Cambridge.

The Yield of Pastures.—Attention has been directed in earlier reviews to the new philosophy of grassland husbandry which is growing up on the basis of recent discoveries relating to the composition and nutritive value of grass in its different stages of growth. Work on this subject has now been going on at Cambridge since 1924, and the results of these investigations have been brought forward in a series of publications, the first two of which (H. E. Woodman, D. L. Blunt, and J. Stewart, *Jour. Agric. Sci.*, xvi, 205, 1926 and xvii, 209, 1927) have already been dealt with in these reviews. It will be recalled that the main conclusions from this early work were as follow : (1) Under a system of weekly cutting, pasture herbage, irrespective of its botanical composition, is a fodder whose dry substance partakes of the character of a protein concentrate of high digestibility and nutritive value. (2) Under such conditions, and especially when rainfall is well distributed over the season, the protein-concentrated character of the herbage is maintained substantially throughout the entire season. (3) Such herbage is capable of supplying the protein requirements of all types of farm animals. When supplementary food is necessary, therefore, this should be given in the form of carbohydrate-rich food and not, as has been customary in past years, in the form of oil-cakes.

A third investigation (H. E. Woodman, D. B. Norman, and J. W. Bee, *Jour. Agric. Sci.*, xviii, 266, 1928) showed that the differences in chemical composition, both organic and inorganic, between grass cut at weekly and at fortnightly intervals are inconsiderable. The dry matter of grass grown under a system of fortnightly cutting is a protein concentrate equal in digestibility and nutritive value to that obtained by weekly cutting. Moreover, by systematic cutting at fortnightly intervals, these characteristics are retained over the entire season.

During the season of 1928 (H. E. Woodman, D. B. Norman, and J. W. Bee, *Jour. Agric. Sci.*, xix, 236, 1929) the investigation

was carried a stage further l_ loption of a system of cutting at intervals of three weeks. Though the herbage obtained under this more lenient system of cutting was somewhat less rich in digestible protein, it was, nevertheless, equal in respect of digestibility and nutritive value to grass grown under weekly and fortnightly systems of cutting. At the end of three weeks' unchecked growth, pasture grass still consists of non-lignified, highly digestible tissue as at the end of a week's or of a fortnight's growth. Although the protein content of the grass shows a slight falling off during the third week of growth, this is unaccompanied by any corresponding diminution in digestibility. It was further demonstrated that the depressing influence of drought on the protein content and digestibility of pasture grass is much less marked under a system of three-weekly cutting than under the severer system of cutting every week.

During the carrying out of the 1927 investigation, it was found that if adjoining plots on a pasture be cut at weekly and fortnightly intervals respectively, then the plot cut fortnightly yields somewhat more heavily than the plot cut every week. Further, this disparity in productivity becomes most marked at those times of the season when the conditions for growth are most unfavourable, as, for instance, during a spell of droughty weather. Since the weather conditions of 1928 were, on the whole, unfavourable to the abundant growth of herbage on pastures (owing to droughty conditions which prevailed during April, July, and September), it would be anticipated that the yields from the plots submitted respectively to weekly, fortnightly, and three-weekly systems of cutting would display unusually striking differences. That this was actually the case is made clear by the accompanying table, in which a comparison is given of the total yields of herbage, in dry matter per acre, which were obtained in 1928 from the respective pasture plots. The yields from the same pasture under the conditions of the 1925 and 1927 investigations are also given.

Summary of Total Yields of Herbage from Pasture Plot in Three Different Seasons of Experiment.

April 13th, to beginning of October.	1925.	1927.	1928.
	lb. dry matter.	lb. dry matter.	lb. dry matter.
	Per acre.	*Per acre.*	*Per acre.*
Weekly cutting . . .	2,833	—	1,982
Fortnightly cutting . .	—	3,621	2,562
Three-weekly cutting . .	—	—	3,216

It will be noted that, under the weather conditions of the grazing season of 1928, cutting at fortnightly intervals produced

29·3 per cent. more dry matter than was obtained under a system of weekly cuts, whilst the yield obtained by cutting at three-weekly intervals was 62·3 per cent. greater than that obtained by weekly cutting, and 25·5 per cent greater than that grown under a fortnightly cutting system. Since there is little or no difference, from the standpoint of starch equivalent, between pasture herbage grown under systems of weekly, fortnightly, and three-weekly cuts, the *yield* differences under the three systems are possessed of great practical significance. A system of weekly cutting is comparable with the system of grazing where it is the custom of the grazier to regulate the stocking of his sheep-grazing land in such a manner as to keep the herbage uniformly grazed down throughout the grass season. If the herbage shows any tendency to grow beyond the very young stage, more sheep are introduced to hold it in check. This practice may be referred to, for convenience, as "non-rotational close grazing." On the other hand, a system of three-weekly cuts may be taken as conforming with the conditions of rotational grazing, where the interval between successive close-grazings of enclosures is of three weeks' duration. It will be convenient, for the purposes of the present discussion, to refer to such a system as a "three weeks' rotational close-grazing system."

The yield results bring to light an important advantage which "rotational close-grazing" possesses over a system of "non-rotational close-grazing." If the pasture, on which these investigations have been carried out, had been so stocked during 1928 that the herbage was kept closely and uniformly grazed down throughout the season, then it would have produced herbage, for the sustenance of the animals, at the rate of about 1,980 lb. of dry matter per acre over the season. If, on the other hand, the tract of grass had been divided up into smaller areas in such a way that each enclosure, after being closely grazed by stock, was permitted a three weeks' interval of unchecked growth before being grazed down again, the pasture would have produced herbage at the rate of 3,220 lb. of dry matter per acre over the season. In other words, the stock-carrying capacity of such *unfertilised* pasture would have been increased in the ratio of 198 : 322 (*i.e.* roughly 2 : 3). This measure of improvement naturally applies only to the pasture under investigation, under the weather conditions of 1928, and would probably not be so marked in a season more favourable to the abundant growth of herbage. Obviously, the question of the maximum yield of digestible food from a pasture is bound up with the investigation of the process of lignification in the herbage, since it is reasonable to assume that the conditions for such maximum yield will be realised when the

interval between successive cuttings, or grazings, is as long as possible. The length of this interval will naturally depend on the time required by the young shoots of grass to reach the stage of growth at which lignification, with consequent running-off of digestibility, sets in.

The data from these investigations also serve to demonstrate the *primary* importance of the influence of the general weather conditions of the season (in particular, those of rainfall) on the growth of pasturage. Within the limits of the systems of cutting which have so far been investigated, it is clear that unfavourable meteorological conditions in a particular season may lead not merely to a much smaller growth of grass than would be obtained, under the same system of cutting, in a more favourable year, but may actually cause the yield under a lenient system of cutting to be smaller, instead of larger, than under a less lenient system of cutting during a more favourable season.

Minerals in Pastures.—A notable contribution to our knowledge of the factors which influence the nutritive value of grass has come from the pen of Dr. J. B. Orr during the past year (*Minerals in Pastures*, J. B. Orr, 1929, H. K. Lewis & Co., Ltd., London). This treatise is primarily the outcome of the deliberations of a sub-committee appointed in 1926 by the Civil Research Committee of the Cabinet to consider and report on the relationship between the mineral content of pastures and their nutritive value. From the initial inquiries instituted by this sub-committee, it was evident that malnutrition in cattle and sheep arising from deficiency of minerals in grass was widespread in the pastoral areas of the Empire, and that the subject, which was of great economic significance, warranted close and systematic investigation. A report to this effect was duly forwarded to the Civil Research Committee, on whose further recommendation grants were made by the Empire Marketing Board in aid of a comprehensive scheme of investigations into the mineral aspects of pastures within the Empire. A twofold scheme of work was adopted, actual investigations in selected grassland areas being supplemented by a detailed search of the literature dealing with every phase of the subject. Major Elliot states in the preface to the volume: "At the request of the sub-committee, the information so far obtained has been brought together by Dr. Orr in the present review, with the object of having it circulated to various officials and research workers throughout the Empire who are interested in the subject."

Beginning with an explanation of the economic importance of grassland, the author passes on to trace the development of scientific methods for investigating the problems of pastures.

This is followed by a minute survey of the results of many investigations into the mineral composition of both good and poor pastures in the British Isles and in various parts of the world. The factors which affect the mineral content of pastures are also considered in the light of present knowledge. Dr. Orr then proceeds to deal with various forms of disease which are attributable to deficiency of minerals in pasture herbage. The deficiency diseases of grazing animals in Europe, Africa, Australasia, America, and Asia are described in separate chapters, and the results of investigations into the causes of such diseases are summarised and critically discussed. A further chapter treats of the prevention of deficiency diseases in pasturing stock, either by the direct administration to the animal of the deficient minerals or by the enrichment of the pastures through the application of mineral fertilisers to the soil. It is shown that the prevention of disease by these measures leads also to an increased rate of growth in animals and to an increased production in adult females.

In an appreciative review of this volume (T. B. Wood, *Nature*, cxxiv, 437, 1929), the question is raised as to whether the failure of animals to thrive on these deficient pastoral areas may not sometimes be due as much to the energy-deficient character of the herbage as to its deficiency in minerals. The starch value of such herbage may be so low that animals, even when consuming it to the limit of appetite, may not be able to secure sufficient net energy to permit of normal development. This aspect of the question is being studied at Cambridge at the present time.

Beef Production.—" Though it may still pay to fatten cattle on the fine pastures of the Midlands, winter feeding, as carried out in Great Britain, in many cases does not show a direct profit. It is still practised because it provides a valuable by-product, *i.e.* farmyard manure." This arresting statement is taken from the first paragraph of a publication which has appeared during the past year (*Beef Production in Great Britain*, T. B. Wood and L. F. Newman, 1928, R. Silcock & Sons, Ltd., Liverpool). The writers set out to discuss the causes which render uneconomic the feeding of cattle for slaughter and to indicate the means whereby it should be possible to place this important and essential industry on a paying basis. The directions in which improvement in this respect might be sought for are: (1) The production of the best class of animal, which commands a higher rate both from the butcher and in the open market. Attention should be directed to securing uniformity of quality in the beef, a factor of primary importance if the market for beef animals is to be held. (2) The elimination of ssary intermediate profits by setting up an organisation

for ensuring close co-operation between the store-breeders of Ireland and the western counties and the feeders of the eastern counties. (3) The production of " baby beef " by more intensive methods of feeding. The writers show, however, that " baby beef," per lb. of meat produced, requires about twice as much concentrated food as " three-year-old beef," and that the high price of concentrates, and their comparative shortage in this country, put a limit on the production of " baby beef " on an extensive scale. This difficulty might be met by utilising grass along the lines suggested by the Cambridge investigations, since the food material in pasture herbage, during its young stages of growth, is itself a concentrated, protein-rich food.

A valuable feature of this publication is the detailed information which is given of the composition of nineteen carcases of typical beef animals, the analyses having been carried out by the Staff of the Animal Nutrition Institute of the Cambridge School of Agriculture in the course of an investigation of the process of fattening.

Poultry Nutrition.—The results of an investigation into the influence of the size of a ration on the extent to which it is digested by poultry are discussed in a recent paper (E. T. Halnan, *Jour. Agric. Sci.*, xviii, 766, 1928). Four White Leghorn cockerels were fed in different periods on widely varying amounts of a Sussex ground oats and milk mixture. The daily rations varied from a sub-maintenance ration of 50 gm. to a " limit of appetite " ration of 150 gm. A slight depression in the digestion co-efficients of the organic matter, the crude protein and the N.-free extractives, and a slight increase in the digestion co-efficient of the ether extract, were noted with increasing bulk of ration, but the differences were attributable to individual variation among the experimental birds rather than to the effect of altering the size of the ration. It is concluded that in experiments with fowls, digestion of the food nutrients is not materially affected by variations in the amounts of food fed.

Digestion of Cellulose by Ruminants.—In connection with studies which are being made of the mechanism of cellulose digestion in the ruminant organism, experiments have been carried out (H. E. Woodman and J. Stewart, *Jour. Agric. Sci.*, xviii, 714, 1928) to demonstrate the possibility of obtaining glucose from cellulose by controlling the activity of cellulose-splitting organisms. An active culture of these organisms is added to pulped filter paper (or other forms of cellulose) in the usual culture medium and the mixture is kept at 65 C°. When the fermentation reaches its height (2 to 3 days), toluene is added and the mixture is then incubated at 37° C. Glucose, in small yield, is formed during this second stage of incubation. The

result is taken as supplying evidence of the formation of glucose as an intermediate phase in the destructive fermentation of cellulose by bacterial agency. The addition of the antiseptic inhibits the further activity of living organisms, and it is presumed that the glucose which arises in the second stage of the fermentation is the result of the activity of enzymes which have been elaborated and secreted by the organisms during the first stage.

High Protein Diet and Renal Disturbance in Sheep.—(J. Stewart, *Vet. Jour.*, May 1929.) Metabolism experiments on sheep and lambs have demonstrated that diets, in which the protein content is 400–600 per cent. above the highest any farmer would be likely to feed, are not productive of any definite disorganisation of the kidneys or other organs of these animals. It appears justifiable to conclude that excess of protein in the diet cannot any longer be regarded as the primary factor in the causation of "Pulpy Kidney Disease" in young lambs.

Tapioca (Manioc) Meal and Pig-feeding.—During recent years this carbohydrate-rich food has been imported into this country in increasing amounts. Feeding trials with swine carried out at the Harper Adams Agricultural College (J. Fullerton, *Jour. Min. Agric.*, xxxvi, 130, 1929) have shown that high-grade tapioca meal is a satisfactory food as regards palatability and other dietetic properties. It may be regarded as suitable to replace maize or barley meal up to at least 25 per cent. of the total ration. The bacon and hams resulting from tapioca-fed animals are of distinctly better quality than those from maize-fed pigs. At current prices, the use of tapioca meal in place of maize or barley effects a sensible reduction in the cost of feeding.

Sugar-beet Pulp and Pig-feeding.—It has been shown (H. E. Woodman, A. N. Duckham, and M. H. French, *Jour. Agric. Sci.*, xix, 656, 1929) that pigs are able to digest dried sugar-beet pulp and molasses-sugar beet pulp to an extent very little inferior to that to which ruminant animals are able to digest these feeding stuffs. Despite this finding, however, it is clear that sugar-beet pulp, although a good source of carbohydrate for sheep, bullocks, and dairy cows, is by no means an entirely suitable food for pigs. Its inclusion in pig rations, even to the moderate extent of the equivalent of one-sixth of the ration, causes the mixed food to be very bulky after the usual soaking in water. This leads to difficulties in securing satisfactory consumption of the food, and it is found that pigs, in consequence of the well-known difficulty they experience in dealing with bulky foods, are unable to consume as big a ration as is possible when sugar-beet pulp is omitted.

For this reason, the inclusion of sugar-beet pulp in the rations of pigs depresses the rate of live-weight increase, although the effect on the economy of food conversion is of much smaller account. Although sugar-beet pulp is highly digested by pigs, its bulk prevents its being a suitable food for use in more than small quantities when it is desired to make pigs into bacon as quickly as possible. There seems no reason, however, why it should not be used in moderate quantities as a food for breeding stock, or for pigs which are not being fed to their maximum appetite with a view to obtaining early maturity.

Whole-sugar Beet and Pig-feeding.—Though it is neither customary nor perhaps desirable to utilise sugar beet for feeding purposes, it is conceivable that circumstances might arise occasionally when a farmer would be desirous of feeding the whole or part of his beet crop. Recent investigations at Cambridge (H. E. Woodman, A. N. Duckham, and M. H. French, *Jour. Agric. Sci.*, xix, 669, 1929) have demonstrated that whole sugar beet, suitably grated, may be used to replace barley meal up to 25 per cent. of the total ration in the production of bacon pigs. The substitution should be effected at the rate of 3½ lb. of sugar beet to 1 lb. of barley meal.

Sugar Beet Pulp as a Source of Pectin.—In a recent communication (A. J. Codling and H. E. Woodman, *Jour. Agric. Sci.* xix, 701, 1929) it has been shown that dried sugar-beet pulp contains a high percentage of pectose. A number of successive digestions of 1 hour each with 0·5 per cent. ammonium oxalate extracts an amount of pectin equal to 34·5 per cent.of the weight of dried beet pulp, basing the determination on the weight of crude pectin precipitated when the extracts are run into 95 per cent. alcohol. A single prolonged digestion gives a yield of crude pectin equal to 32·2 per cent. of the dried beet pulp.

Digestion with acidic reagents, such as 0·5 per cent. oxalic acid, 0·6 per cent. tartaric acid, N/20 hydrochloric acid, etc., leads to a quicker extraction of pectin, owing to a speeding up of the pectose to pectin hydrolysis. The yield of pectin, however, is not necessarily enhanced thereby, since under such conditions the pectin undergoes a slow secondary hydrolysis during the extraction with the formation of reducing substances not precipitated by alcohol. Prolonged digestion at 100° C. of dried sugar-beet pulp with water alone also leads to a satisfactory extraction of pectin.

Although beet pulp pectin resembles the pectins from other sources in its general chemical and physical characteristics, it is sharply differentiated from apple pectin in its inability to impart a jelly condition to syrups containing suitable amounts of cane sugar and free acid. The reason for this absence of jellying power is not clear, though apparently it is not con-

nected with the nature and amount of the mineral impurities in the pectin preparations. Nor is it to be ascribed to any changes in the character of the pectose of sugar-beet pulp during the factory process of drying the wet material.

The investigations do not preclude the possibility of discovering a set of conditions under which the pectin extracted from sugar-beet pulp will display jellying properties similar to those of apple pectin. It is clear, however, that the extraction of pectin from beet pulp by methods similar to those employed for its preparation from apple pulp yields a product which, unlike apple pectin, has no technical significance in the processes of making jams and fruit jellies. It is to be concluded, therefore, that the proposals for utilising sugar-beet pulp in these industrial processes must, for the time being at any rate, be set aside.

TORIC ARCHÆOLOGY. By L. J. P. Gaskin, Librarian to the Royal Anthropological Institute, London.

Antiquity, September 1929.—Mr. J. H. Hutton contributes an interesting article on "Assam Megaliths." The author points out that Assam is one of the few remaining areas in which rude megalithic monuments are still erected. Mr. Hutton draws some interesting comparisons between the form of these rude megaliths and the megalithic monuments of Europe. The chessman pillars of Dimapur, for instance, bear, among other patterns, a leaf-shaped weapon very suggestive of the bronze Hallstatt sword; the monolithic erections have also their wooden counterparts, which is interesting in view of the discovery of Woodhenge; alignments of menhirs are frequent and of great size, the dolmens of Dimapur weighing some twenty tons.

The author finds no evidence for a sun-cult in the position of the monuments, and asserts mortuary, phallic, and fertility associations for the menhirs and dolmens of Assam. He suggests a similar association for the megalithic monuments of Europe. The article is accompanied by a series of excellent plates.

Dr. L. Franz writes on Austrian Lake-dwellings. He points out that in the three lakes of Upper Austria, "Mondsee," "Atter-see," and "Traun-see," there are remains of prehistoric pile-dwellings of the Copper Age, and that recent investigation shows that there is no close connection between the Austrian and Swiss Lake-dwellings.

In Upper Austria the villages were situated quite near the shore and the sites carefully chosen with a view to trade routes; the reason for the traffic being trade in copper. Numerous copper-mines in the mountains of Salzburg and the Tyrol were mined in prehistoric times, and there is definite

evidence of a trade route to the Danube via the "Mond-see," "Atter-see," and the rivers "Ager" and "Traun."

Nature, October 19th, contains an account by Miss Caton-Thompson of her recent expedition to Zimbabwe.

Miss Thompson surveys in turn her work on "The Maund Ruins," "The Acropolis Sites," "The Elliptical Temple," and "Sites in the Sabi Reserve."

The plan of work was to test the stratification over a wide, continuous area, and to check the results by means of excavations in the deepest undisturbed sections available in other areas. For the dating the following were found: Pottery of undecorated native ware, fragments of bronze wire bangles, six pottery phalli, and imported beads. The beads are of South Indian and Malayan types, certainly not earlier than A.D. 900 for the former, and A.D. 600–A.D. 1300 for the latter.

Miss Thompson's conclusions as to the nature and age of the ruins are a remarkable vindication of those of Dr. MacIver, who, twenty-four years ago, gave it as his opinion that the ruins were of mediæval date and of native manufacture. Miss Thompson states that "Every detail in plan, building, and contents seems African Bantu. Further, the construction is such that, apart from repairs, not one stone would be standing on another in a period reckoned in millennia and not in centuries." A full account of the work of excavation will be published later in book form.

Early Man in East Africa.—In an article in *Nature*, September 14th, there is a report of the work of the East African Archæological Expedition for the seasons 1926–7 and 1928–9.

The work of the Expedition has centred mainly round the lakes which lie on the floor of the eastern branch of the Great Rift Valley; the central area round Lakes Nakuru, Elmenteita, and Naivasha being worked out in greatest detail. These lakes show evidence of a very great antiquity.

An association of cultures and pluvial sequences has been worked out as follows:

Pluvials.	*Cultures.*
Nakuran . . .	Kenyan Wilton. Nakuran.
Makalian . . .	Elmenteitan.
Late Gamblian . .	Red-bed culture. (Mousterian influenced by Aurignacian.)
Gamblian . . .	Upper Kenyan Mousterian. Upper Kenyan Aurignacian.
Enderian . . .	Lower Kenyan Mousterian. Lower Kenyan Aurignacian.
Eburrian . . .	Kenyan Acheulean.

It is to be noted that while a European nomenclature is adopted for the Kenya cultures this is based on resemblance and no implication of contemporaneity is made thereby.

Suggestions have been put forward for a correlation of the pluvial phases with the glaciations of Europe, and if this can be proved a direct time relationship between the European and Central African Stone Age Cultures will be established.

Proceedings of the Spelæological Society, University of Bristol, 1928, contains an article by Mr. E. K. Tratman on excavations in Ireland.

The excavations were practically confined to a cave site at Kilgreany. The evidence proves that the cave was occupied for a short period at the end of the Neolithic era, and for a long period at the end of the Bronze Age and the beginning of the Iron Age. There seems to have been a considerable period between the early and late Bronze Ages when the cave was only used by animals. The finds include a bronze socketed knife, a small tanged iron knife, bronze pin, bone spindle whorls, bone needle, pottery and artefacts. There are illustrations of the finds.

Revue anthropologique, July–September 1929.—M. Zamiatnine reports on a Mousterian site at Ilskaia, province of Kuban, Caucasus. The industry was represented by some three hundred artefacts. The author describes the artefacts in great detail (with illustrations) and correlates the culture with three other Mousterian sites in South Russia : that of (1) " La grotte du Loup " ; (2) " La grotte de Kiik-Khoba," in which two levels were distinguished, one containing a lower Mousterian industry and the bones of Neanderthal man, the other comparable to the culture of Ilskaia ; (3) Derkoul, the latest of the Mousterian sites in South Russia, where artefacts were found which exactly resemble those of the Mousterian of Western Europe.

Prof. E. Pittard contributes an extremely interesting article on the fact that the first discovery of prehistoric art was made by Dr. François Mayor at Veyrier (Haute-Savoie) in 1833. The discoveries consisted of an engraved *Baton de Commandement* in reindeer and a rod four inches long barbed with thorns. M. Pittard points out that this latter piece has been wrongly classified as a Magdalenian harpoon, and is in reality a carving of similar date of a branch with its buds.

To Dr. Mayor belongs, therefore, the discovery of the first sculptured and engraved objects of Palæolithic art. A much fuller account of Dr. Mayor's work (by the same author) will be found in *Genava*, VII, 1929.

L'Anthropologie, **39**, 1929.—M. A. Vayson de Pradenne reports on the Palæolithic station of " Mont-Dol " (Brittany).

The author gives a summary of the history of the excavation of the site and then proceeds to outline his own work.

Of particular interest is the photograph of a mammoth-tusk found *in situ* but unfortunately entirely decomposed. The industry is Mousterian, and there are some magnificent plates representing the artefacts excavated.

L'Homme Préhistorique, June–August 1928.—M. Harmois contributes a catalogue of the large stone celts of France. Brittany and Normandy predominate in numbers found, and the longest mentioned is from La Mayenne. The catalogue loses much of its value from lack of illustration; but will be found to be useful as an inventory.

Bulletin de la société prehistorique française, July–August, 1929.—MM. Louis and Bruguière describe the excavation of a tumulus at La Rouvière. There seem to have been two tumuli, the one superimposed upon the other and of different epochs. The first was an undoubted funerary mound containing a cinerary burial of the Iron Age; the second, while showing some evidence for a funerary erection, has yet the aspect of a megalithic monument; it belongs to the Gallo-Romanic period. There are excellent plans and drawings of the mounds and pottery.

Forvännen, 1929 (2).—S. Lindqvist answers Prof. Rydbeck on the changes of level of the Stone Age Sea and the earliest human settlement of Scandinavia. He points out that the sea invaded the coast of South Scania farther in the Copper Age than in the Shell-mound period, and that the inhabitants made full use of this for fishing purposes. It follows, then, that the corded ware found in the upper levels of the Shell-mounds need not be earlier than similar pottery farther south.

The Antiquaries Journal, October 1929.—Mr. Woolley contributes a long, well-illustrated article on excavations at Ur, 1928–9. The author reviews the results of the season's work under four headings: (1) The Cemetery. (2) The buildings and rubbish-heaps of the pre-cemetery town and the evidence for the Flood. (3) The Nannar temple. (4) The town walls.

Most of the finds from the above have been described in SCIENCE PROGRESS as accounts of them have appeared in *The Times*. Mention of this article is made here for those who wish to consult a comprehensive survey of the whole work of the season.

Illustrated London News, July 20th–October 12th, contains the second to fifth series of the articles on the new treasures from the annexe of Tutankhamen's tomb.

The second series (July 20th) describes the Boy-king's fire-lighter, play-box, nursery-chair, and other finds. Of

particular interest is the fire-lighter, which is of the bow-drill variety. The third series (August 3rd) contains an account of the King's wine-jars, dalmatics and unguent vases; of special notice are the "lion gardant" unguent vase, the bleating-ibex vase, and Tutankhamen's dalmatic, a vestment-like gala robe.

The fourth series (October 5th) deals with Tutankhamen's gaming-boards, feather-fans, and sceptre. The largest of the gaming-boards is an oblong box (inscribed along the side and divided into squares on top) resting on a black ebony stand of sledge-like form with the cushions and claws of the feet embellished in gold. In the fifth series (October 12th) the King's weapons are described. These comprise single-sticks, falchions, bows and arrows, boomerangs and shields.

It is perhaps needless to add that the series is accompanied by the most magnificent illustrations.

ARTICLES

NOTES ON RESOLVING POWER

By ALFRED W. PORTER, D.Sc., F.R.S., F.Inst.P.,
Emeritus Professor of Physics in the University of London

The acutest and most definite of our organs of sense for giving us a detailed knowledge of the external world is the eye. Without it we might, no doubt, have felt our way about and obtained a knowledge of near things ; our knowledge of far things would have been limited to the information detectable by the organs of hearing. Blind men we would have been, with all the limitations that the phrase proverbially implies.

But eyes themselves are limited in their power of differentiation ; to remedy this, in the last three centuries telescopes and microscopes have been invented. Their design until the early part of the nineteenth century was based entirely upon the laws of geometric optics. By means of these instruments a picture or image of the object is produced larger than can be obtained without them. In the case of the microscope the image is really larger than the object ; in that of a telescope it merely looks larger (though it is in reality much smaller) than the object, *i.e.* it subtends a larger angle at the eye.

The need for extraneous aids can be illustrated by stating that most eyes cannot see the divisions on a scale when the interval between them subtends less than 1 or 2 minutes of arc. This must be taken together with the facts that an average eye cannot focus an object at a less distance than 10 inches, and the angle subtended by a scale-interval is increased if the object is brought nearer the eye.

If, when placed at the least distance for clear vision (about 10 inches for the average eye), the angle subtended by a scale-interval is less than the above limit, resort must be had to a microscope (or even a simple magnifier) that will produce an image sufficiently large. In all early investigations it was this magnification that was sought for, and systems of lenses of high and still higher magnifying power were produced. These systems were all calculated by applying the rules of geometric optics : the kinds of glass and the radii and positions of the

surfaces being selected so as to give the best approximation to a perfect image—that is, a picture as much like the object as possible, except in regard to its greater size. The realisation of a *perfect* image can be shown to be impossible—at least when spherically curved surfaces are employed; nevertheless, lens systems of very high approach to perfection are, in fact, manufactured.

These problems of geometric optics must always be kept in sight, but they do not constitute the chief subject of this article.

At the beginning of the nineteenth century Young re-introduced the theory that light is propagated as waves and not merely as rays. Since a ray may be regarded as the normal to a wave there *might* not be much difference in the two theories. But the implication is that, while a ray according to geometric optics acts only at points along itself, *each portion of space acted upon by a wave in turn acts as a source from which energy travels out in all directions.* Young adopted the wave theory (originally due to Huygens) in order to explain phenomena of interference of light in which this spreading became manifest. Light from a single slit passes subsequently through two narrow slits close to one another; maxima and minima of illumination (" fringes ") are observed on a screen receiving the light. If each of the two slits is considered as a source emitting light in the same phase, their combined effect on the screen depends upon the difference of lengths of path traversed. If a " crest " of one wave meets a crest of the other, there will be great illumination; if it meets a " trough " the two tend to cancel one another; hence the maxima and minima.

Twenty years later Fresnel, starting independently, applied the same type of reasoning to numerous cases in a masterly series of papers which laid the foundation of the modern wave theory of light. So thoroughly was the wave-theory treated in these papers that in 1831 G. B. Airy (the Astronomer Royal) wrote: " I believe there is no physical theory so firmly established as the theory in question." Experimental and theoretical inquiry (Schwerd, Stokes, Rayleigh, and others) has established it still more firmly at the present day, so far as it concerns the present subject; although, when interactions of light with matter are involved, mysterious transmutations of the interacting agents take place, which are at present in course of investigation and interpretation.

No discussion of the action of a lens can hence be complete which is restricted to geometric optics alone. The general nature of the facts observed can be most simply illustrated by considering a telescope objective. The image of a star (which itself can be regarded as a point) is never a point. Geometric

optics shows, however, that it should be more nearly so the smaller the aperture of the lens; but experimentally the *smaller* the aperture the *larger* is the image, and this consists of a central bright disc surrounded by luminous rings. The wave theory explains this fact. The geometric image is at the centre of the disc. The images of two stars are two discs with their attendant rings. To see them as two (*i.e.* to *resolve* them) the discs evidently must not overlap too much. To investigate when overlapping will begin it is necessary therefore to calculate the diameter of the disc.

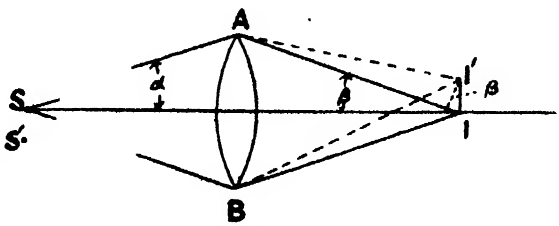


FIG. 1.

Let S be a very distant star (Fig. 1). The wave sends disturbances not only to the geometric image I, but to neighbouring points such as I′ as well. At I they meet in the same phase, and the resultant intensity is great. At I′ however, the extreme ray BI′ is longer than the other extreme AI′ by the amount $2\,\overline{II'}\sin\beta$; there is, therefore, at I′ a discordance of phase. If this amounts to a whole wave-length, half the disturbance reaching I′ will cancel the other half and darkness will result. This determines the position of the edge of the disc, and therefore its radius, which is given by $2\,\overline{II'}\sin\beta =$ wave-length $= \lambda_0$

$$\text{or } \overline{II'} = \lambda_0/(2\sin\beta) = \lambda_0 F/\overline{AB}$$

or radius of disc × diameter of lens = focal length × wave length in air.

Hence to produce a small disc-image the wave-length of light should be small, and the diameter of lens large.

The above calculation is exact only for a rectangular opening; to obtain an exact value for other cases all the waves that pass through the lens and reach I′ must be taken into account. For a circular opening of the same maximum width the disc is wider in the ratio 1·22 (Airy).

It still remains to find how the intensity will be distributed when it comes from two stars the disc images of which overlap. To examine this point appeal may be made to the laws of ordinary photometry. When two candles simultaneously

illuminate a screen, the illumination at any point is precisely the arithmetic sum of the illuminations due to the individual candles. There is no question of interference of light here, because we cannot think of the existence of any permanent phase relationship between the different sources—they are quite independent of one another. If we can calculate the distribution of light in each disc the resultant illumination at any one point is obtained in like manner by simple summation. This is illustrated in Fig. 2.

The lights in the two discs (the dotted curves, Fig. 2) are shown overlapping to such an extent that the centre of one coincides with the boundary of the other. The sum of the ordinates at any position gives the curve A. This degree of overlapping has been taken generally as the limiting case for which optical separation of the images can be detected, and is known as the Rayleigh limit. In reality observation with telescopes shows that the performance of the telescope is about 25 per cent. better than is given by the Rayleigh value for circular openings (Dawes). The possibility of this is indicated also in the diagram; for an acute eye may be able to detect the small deficiency of illumination in the centre of the conjoined discs at A—indicating that a greater degree of closeness may still give resolution. (See *Journal R. Micros. Soc.*, 1908, p. 3.)

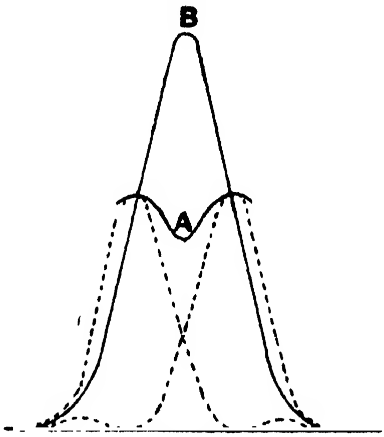


FIG. 2.

There is no serious question that the wave-theory (alternatively known as the diffraction-theory) gives an adequate account of this problem.

When, however, we come to deal with microscopes several complications exist, which have given rise to very considerable debate. No doubt is raised as to the validity of the diffraction theory; the debate has been as to the best way of employing it—especially as different investigators, using different methods, have come to different conclusions.

The chief circumstances which give rise to complications which for a microscope require distinctive treatment are (*a*) the fact that the object is almost never self-luminous, and (*b*) in many cases the lenses often subtend very wide angles at the object.

Consideration (*a*) also applies to telescopes, since most

terrestrial objects and also the planets are not self-luminous, and not without preliminary examination can it be decided in which category they should be classed. It is best to take consideration (*b*) first.

The type of calculation is the same as for telescopes, but approximate values of sines are no longer sufficient. As before (Fig. 1) the extreme difference of path at the edge of the disc-image of a point is of the order λ (the wave-length of the light employed; but it is also $2\,\overline{II'} \sin \beta$ so that $2\,\overline{II'} \sin \beta = \lambda$ nearly. Now the image always lies in air (index $= \mu_0$) but the object-space may be a medium of refractive index μ. It was shown by Hockin that one of the conditions for a perfect lens system is

$$\mu\,\overline{SS'} \sin \alpha = \mu_0\,\overline{II'} \sin \beta$$

where $\overline{II'}$ is the geometric image of $\overline{SS'}$. Hence we can write putting n for μ/μ_0, $2\,\overline{SS'}\,n \sin \alpha = \lambda_0$. Here it must be emphasised that, as before, $\overline{II'}$ is being taken not only as the semi-width of the disc image of S but also as the geometric image of SS′, where $\overline{SS'}$ is the smallest value which can be resolved. Hence this smallest value is $\lambda_0/(2\,n \sin \alpha)$ and the resolving power is taken as the reciprocal of this, viz. $2\,n \sin \alpha/\lambda_0$. The quantity $n \sin \alpha$ is called the numerical aperture (N.A.) of the objective. For circular openings this resolving power so defined is less in the ratio of 1 to 1·22, due to the disc images being somewhat larger in their case. (The formula previously obtained for the telescope can be deduced from this by inserting an approximate value of $\sin \alpha$ and putting $\mu = \mu_0$ because the media on both sides of the object glass are alike.) The result obtained corresponds to taking S and S′ as independent point-sources and the distribution of the light in the combined discs of two sources is represented by curve A on Fig. 2. Observations with microscopes by E. M. Nelson show that this degree of resolving power can be surpassed in about the same ratio as in the case of telescopes.

If, however, the object is not self-luminous, there may be more or less connection between the phases of the lights arriving from S and S′. To find the result of this dependence consider next the extreme case for which S and S′ are permanently in phase with one another. Each gives rise to a luminous disc (and rings) exactly as before, but there will be a phase-relation between the lights falling on any one point. If the amplitudes at a point are a_1 and a_2 and the phases (at the point) are e_1 and e_2 the resulting disturbance is

$$\begin{aligned} y &= a_1 \cos (pt - e_1) + a_2 \cos (pt - e_2) \\ &= \sqrt{a_1^2 + a_2^2 + 2\,a_1\,a_2 \cos (e_1 - e_2)} \cos (pt - \eta) \end{aligned}$$

and the intensity is $a_1^2 + a_2^2 + 2a_1 a_2 \cos(e_1 - e_2)$. It is necessary, therefore, to know the phase difference $(e_1 - e_2)$ of the superposed lights. If it is zero the intensity becomes $(a_1 + a_2)^2$; if it is π the intensity is $(a_1 - a_2)^2$. In the latter case for equal amplitudes the intensity is zero. Where the phase-difference is $\pi/2$ the intensity is $a_1^2 + a_2^2$, *i.e.* the same as for independent sources. For example, if the two dotted curves on Fig. 2 represented two illuminations in the same phase (instead of being independent) the resultant would be as shown by Curve B. There is now no dip of intensity in the middle and in consequence, although the object is double, the image would be a single one. It is clear, therefore, that the two sources need to be further separated in order that their duplicity may be detected; how much further will be shown in connection with Abbe's method.

These are two extreme cases, and the moral might at once be drawn that to enable fine detail to be seen, the illumination of the object should be such as to produce as little phase-relation as possible between the lights from neighbouring points. This statement, of course, requires detailed proof. The problem is a complicated one to deal with in any general way; the following principles will assist in making it clear.

1. If any part of the light from S has permanent identity of phase with that from S′ then the resultant intensity for that part is given by $I = (a_1 + a_2)^2$. It is represented by such a curve as B on Fig. 2.

2. If the remainder of light from S has no permanent phase relation with that from S′ the intensity is $I_r = a_3^2 + a_4^2$ and is such as Fig. 2, curve A.

3. The resultant intensity in the plane II′ is $I + I_r$. The addition of I and I_r tends to diminish the depression at A in I_r; that is, it diminishes the resolution.

There are thus two definite values for the resolving power corresponding to extreme cases, and it will be shown later that when the sources are independent the resolving power is about twice as great as that which relates to sources in the same phase.

We may turn aside for a moment and consider the question from the point of view of history. Herschel (1805) and Fraunhofer (1823) examined star images and showed that their diameters varied inversely as the diameters of the object-glasses used. Sir George Airy (1834) was probably the first to apply the diffraction theory; he was followed by Schwerd and Knochenhauer, who subjected the theory to experimental control. The microscope does not appear to have received the same attention in the early period. It is noteworthy, however (as the late Lord Rayleigh has pointed out), that even Fraun-

hofer was alive to the general principles to apply. He quotes: " Ein mikroskopischer Gegenstand z. B. dessen Durchmesser = ω ist, und der aus zwei Theilen besteht, kann nicht mehr als zwei Theilen bestehend erkannt werden. Diese zeigt uns eine Gränze des Sehevermögens durch Mikroskope. [ω = wave-length.] Fraunhofer's views, however, did not commend themselves to Herschel, who regarded the " alleged limit to the powers of microscopes " as " not following from the premises " (1830). Lord Rayleigh was convinced of the existence of such a limit in 1870. Probably the first to deal with resolving power in any detailed way, so far as it concerns a microscope, was Prof. Abbe of Jena (1873), closely followed by Helmholtz

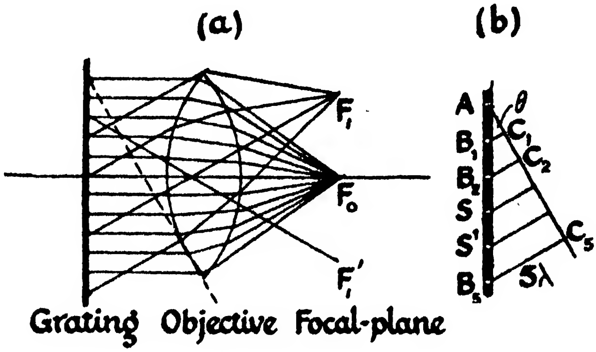


FIG. 3.

(1874). Unfortunately Abbe's method of attack is limited to cases in which the object is a diffraction grating (*i.e.* a series of regular rulings) illuminated by a narrow beam from a distant point source or from a distant image (of a point source) produced by a condenser lens. Further, it was intermingled with an erroneous theory, according to which the mode of formation of microscopic images was essentially different from that of macroscopic ones; since, however, this supposition was afterwards given up, it does not further concern us.

The specification of the illumination shows that the light passing through the " object " when the source is on the axis is all in one phase: the case selected is therefore the opposite extreme to that for telescopes. With the given disposition we can make use of the fact that the light diffracted by the object is first gathered together into bundles (Fraunhofer " spectra ") near the back focal plane of the objective at points such as F_1, F_0, F_1'. Each of these spectra, when monochromatic light is used, is a monochromatic image of the ultimate source of light; we confine ourselves, throughout, to the case of monochromatism.

Each of these "spectra" spreads out in diverging cones, which overlap and give rise to interference bands (Fresnel "fringes") in the plane of the ultimate image. These Fresnel bands are practically equally spaced and to that extent are a magnified image of the grating object. But Abbe showed that if the diffraction spectra are so widely separated that only one (*e.g.* F_0) transmits light into the field of view there can be no Fresnel bands formed, so that what is seen is merely uniform illumination in that field; there is then no indication of a ruled structure in the image seen, or in other words there is no resolution. To find when this will occur we require to learn how diffraction fringes are formed.

A direct parallel beam consisting of the light that passes through all the openings of the grating (Fig. 3) is focussed at the principal focus, F_0, and it is a property of a perfect lens that it introduces no difference of phase between the disturbances that reach F_0. Hence if the phases of the separate contributions are alike at the grating, they will be alike at F_0, which will therefore be strongly illuminated. But if we take an inclined parallel beam, since all the waves take the same time to go from a plane such as AC to the new focus at F_1, the differences of phase at F_1 will be the same as those in the plane AC, which is called the standard plane of reference. But to reach AC (and therefore F_1) at the same time as one another they must have passed through the grating at different times, because, as seen from the diagram, they have different distances to go to reach that plane. Suppose these distances are λ, 2λ, 3λ, etc., then there will be whole periods difference between their phases at F_1, and there will be a maximum intensity there. On the other hand, if the distances were only half these values, each would be half a period behind the next, they would cancel in pairs at F_1, and that point would be dark. Intermediate values of the distances would in general give intermediate intensities. That is, for brightest illumination in the focal plane we want SS′ sin θ (Fig. 3*b*, which is an enlarged view of the grating) to be equal to λ or 2λ or 3λ, etc., or say, $p\lambda$ where p is an integer either positive or negative. Hence a number of bright images of the source, viz., F_0, F_1, F_1', etc., will be formed in the back focal plane. They correspond to directions of rays making angles θ_0, θ_1, θ_2, etc., where $\sin\theta_0 = 0$, $\sin\theta_1 = \lambda/\overline{SS'}$, $\sin\theta_2 = 2\lambda/\overline{SS'}$, etc.

It is the light from these out of which the ultimate image is formed and Abbe argued that if the aperture of the lens is only big enough to let one spectrum form there will be no interference bands formed. Now the condition that only the central spectrum can form is that θ_1 must be greater than α, the semi-angular aperture of the objective; the limiting case

is when $\theta_1 = \alpha$ or $SS' = \frac{\lambda}{\sin \alpha}$. If the lens is an immersion lens λ must be the wave-length in the immersion medium, and as wave-lengths vary inversely as the refractive indices

$$SS' = \frac{\lambda_0}{n \sin \alpha} \text{ for resolution,}$$

where λ_0 is the wave-length in air.

Abbe further argued that the image becomes more perfect the larger the number of spectra that contribute to its formation.[1] He proceeded to prove the validity of his method of attack by introducing diaphragms at the back focal plane, with slits cut in them, so as to let one or more spectra through: the image changed character or disappeared according to the number of spectra transmitted. He showed further that, if the spectra of order 0, 2, 4, etc. were allowed through, the ruling on the image became twice as close as given by the ordinary law of magnification; he explained this by pointing out that the spectra so transmitted were in the positions of the 0, 1, 2 orders of spectra for an object ruled twice as closely.[2]

If we compare Abbe's expression with the limiting value of SS' obtained for independent sources, it will be found to be double. Hence, introducing the similarity of phase for the light passing through the several openings of the object has halved the resolving power—in accordance with the general rule that has already been given.

Of the method of Abbe the late Lord Rayleigh wrote in 1896: "The publication of the paper marks a great advance, and has contributed powerfully to the modern development of the microscope" (*Phil. Mag.*, xlii, 167, 1896).

Much discussion has been held over Abbe's method of attack, and serious attempts have been made to discredit it. Perhaps it is evidence of genius that in the presentation of his method he ignored all complications in the desire to get at

[1] Rayleigh (1896) remarks: "From some of the expositions that have been given it might be inferred that if all the spectra emitted from the grating were utilised the image would be a complete representation of the original. By considering the case of a very fine grating, which might afford no lateral spectra at all, it is easy to see that this conclusion is incorrect; but the matter stands in need of further elucidation." Probably, a consideration of the secondary spectra (see later) would remove some of the difficulty that Rayleigh felt. Attention needs also to be paid to Stokes's obliquity factor—which is usually omitted in the consideration of gratings. Further, the waves would need to be brought together in appropriate *phases*, and it is not clear that a single objective is capable of doing this.

[2] Apparatus for demonstrating these changes is on the market for use with the Zeiss microscope. It can also be made on a large scale for projecting the effects on a screen. (U. Behn u. Heuse, *Verh. d. Deutsch Physik Ges.*, 8, 283 (1906).)

some root principle, which would be a guide in microscopic practice; and in a great measure he succeeded. But his method has many imperfections. He took no account of the aperture being usually circular—his results correspond more nearly to rectangular openings. He took no account of the *total number* of lines on his grating. He took no account of the diffraction spectra failing to be narrow lines or points (for each colour); yet even when the grating lines are very narrow the spectra have a breadth equal to about $(2/N) \times$ distance between two adjacent spectra, where N is the number of "lines" on the grating itself (or rather those which are illuminated). Moreover, he took no account of the fact that, besides the principal spectra that Fraunhofer was acquainted with, there are $(N - 2)$ secondary spectra between each pair of principal ones. These are more feeble—but the brightest have an amplitude of more than one-fifth of a principal spectrum, and they must contribute something to the character of the ultimate image. Indeed, Johnstone Stoney has shown experimentally that resolution of a feeble kind can be obtained when only one principal and one secondary spectrum are operative—a result which violates Abbe's conclusions if the term "limit of resolution" is taken in its strict sense. This experiment Stoney showed the present writer, with all the precision in the optical arrangement of which he was capable, some twenty or more years ago, employing the line-markings on *Amphipleura pellucida*, and excluding all but the two desired spectra by means of diaphragms in Abbe's manner. Attention to all these details, however, would have prevented the wood being seen on account of the trees. It was left to the late Lord Rayleigh in the 1896 paper to develop the consequences of some (at least) of these initially neglected complications; and no one who is endeavouring to reconcile the somewhat diverse experimental results which are obtained by different practical microscopists should fail to consult this paper.

One of the most misleading of the attempts made to belittle Abbe's work, was made by Altmann (1880) and Mandelstam (1911). Interference bands in the image plane are obtained also when the object is a grating of glowing fibres (a piece of an Auer mantle) and photographs were made showing the bands. But it must be observed that in all such experiments, though no diffraction spectra could be formed (owing to the waves being completely independent) except those due to the *individual* fibres, *slits were placed where spectra would have been in Abbe's case*: the interference bands are therefore necessarily very similar in both cases, and the objections made, on the strength of them, against Abbe's treatment, are not valid.

At the same time it must be admitted that Abbe's treatment

is very limited in its application. It is exceedingly seldom that the object consists of regular rulings, and we want to know what we may expect from a lens in all the multifarious cases that can arise. Further, if we extend the use of the method by calculating the diffraction spectra and the interference figures which arise from them, for other objects—such as regular systems of dots, two (or more) lines or two (or more) dots, lines of different widths and dots of different diameters, it must throughout be borne in mind that the calculation—as may be inferred from what has been said—does not apply to the best possible resolving power attainable for the given object. By altering the illumination it should be possible approximately to double this power. Abbe's method does not enable us to decide that this should be so: it simply gives no answer at all to the question.

This leads one to discuss what was probably the most erroneous deduction that Abbe made from his experiments. He concluded that it was essential, for successful resolution, to interpose a slit in front of the condenser, so as to limit the incident beam. If this was placed close under the object it would limit the amount of the object illuminated, but this alone would not matter. He further limited the source so that only a very *narrow beam* was utilised.

In this case if the condenser focussed the source on the object, the objective would receive only a very narrow cone of light, except for the adventitious scattering which takes place at the object itself. Since Abbe's own method correctly indicated the utility of objectives of large aperture, it is difficult to see how he can have come to the conclusion that only a small part of the aperture must be used, even when care and expense have been taken to provide it. It is quite possible he may have been misled experimentally by "glare" effects, which sometimes limit the aperture utilisable; this, however, is only a conjecture. In any case, he found correctly, that to obtain well-defined *diffraction spectra* near the back focal plane, such a slit was a necessity, and he appears to have assumed that with a wide-angle incident beam, thecon fusion of spectra (from the different points of the source) would lead to confusion in the image. He did not work out, what is best worked out by other methods than his own, that if the wide-angle beam is focussed on the object so as to produce (as nearly as possible) independent illumination the confusion in the ultimate image is not increased, but is reduced even below that obtained when a narrow beam is employed: we approach, in fact, the twofold resolving power of independent sources.

It is difficult to discuss this question in a short article, because so many cases can arise. As another example, suppose

that a slit is used close to the condenser, and the object and source are in conjugate planes. Then the image of a narrow source formed at the grating-object will itself consist of a series of diffraction fringes of the source, the brightest being at its centre. The diffraction disc *may* cover all the object that can be seen at one time through the microscope. In this case the diffraction beams emitted by the object have different characteristics from those corresponding to any other disposition of the optical elements. Abbe's treatment gives no information as to what to expect with this type of illumination. General theory shows, however, that each single bright line on a grating *may* appear double in the final image due to the axial illumination being stronger than the oblique.[1]

This leads one to discuss oblique illumination in general;

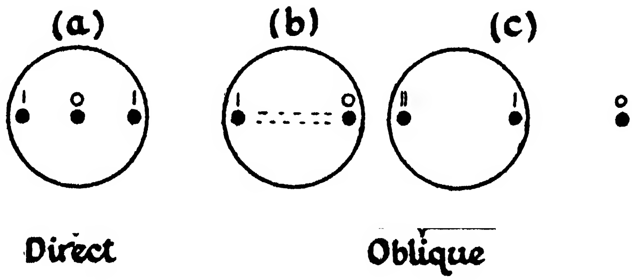


FIG. 4.

a special type of it is dark-ground illumination. Examined by Abbe's method for a grating object illumined by direct light the spectra at the limit of resolution are as in Fig. 4 *a*; whereas for oblique illumination they are as in Fig. 4 *b*. The numbers above the spectra refer to their orders—zero indicating the central spectrum.

In the latter case the spectra are twice as far apart when near the edges of the field, and the rulings that give rise to them must therefore be twice as close; the resolving power is, therefore, doubled by the obliquity. The extra resolution, however, is somewhat precariously obtained, because a very little increase in the obliquity would move the zero spectrum out of the field, and, since only one would remain, the resolution would have disappeared altogether. (The disappearance would not be absolutely abrupt—it would be sharper—the greater the total number of grating-lines under observation.) In order that resolution may be restored the obliquity would need to be

[1] Consideration of a single opening will serve to illustrate how this may come about. The usual expression is $\int \cos(pt-2u)\,du$; but if the amplitude of the incident light varies as $\cos ku$ it becomes $\int \cos ku \cos(pt-2u)\,du$, which can be expressed as two integrals $\frac{1}{2}\int \cos(pt-2u+ku)\,du + \frac{1}{2}\int \cos(pt-2u-ku)\,du$.

increased, until the first and second orders are just within the field (Fig. 4 *c*). In such a case the field surrounding the object will be dark.

Since objects may have many shapes, and are likely to be oriented in various azimuths, it is best for the illuminating beam to be direct, and a central "stop" used to block out the direct beam. The symmetrical illumination treats every azimuth alike. If a parallel incident beam, as recommended by Abbe, is employed the obliquity must correspond to *c* (Fig. 4). The obliquity is brought about by the use of a "paraboloid" or "cardioid" condenser. The figure shows that case *c* requires the use of a condenser of numerical aperture three times that of the objective, and therefore cannot be obtained for objectives of greater numerical aperture than 0·5. This serious restriction, however, only applies when the illumination and object are strictly of the Abbe kind. But even with the Abbe illumination resolving power equivalent to Fig. 4 *a* should be obtained by the use of a dark-ground condenser, with N.A. only slightly greater than that of the objective.

This is about as far as Abbe's method leads us. If we depart from the object and conditions of illumination which he premises we may more conveniently adopt other methods which may lead to different conclusions. We have already seen that a self-luminous object gives rise to double the resolving power with any particular aperture of lens, and that we can only hope to get this in perfection by focussing the source with a condenser of equal aperture upon the object. During a recent discussion held at the Royal Microscopical Society, Dr. H. Moore has endeavoured to extend Abbe's method to broad distant sources and to show that the method fails. But the case he takes is an extended distant source, the points of which send practically parallel beams of various obliquities. (This, however, is not the arrangement which makes the grating practically a self-luminous body.) In Dr. Moore's case he deduces that the illumination in the image plane will be uniform. This may be the case, but the problem would appear to require much more detailed treatment and attention to phases than that which he has given it. Abbe's method must surely give the right intensity-distribution in this case. Each point on the source admittedly gives rise to distinct spectra in the back focal plane, and all the light to be used is gathered into these spectra; the image arises from a redistribution of the light according to Fresnel's method. This holds for each point on the source, and a correct summation of the effects from the separate sources must give the total effect of all. Abbe's method can surely not be lightly swept away on the evidence supplied.

On the other hand, there is no obvious way of applying his method to the cases when the source of light is first focussed on the object.

Turn back to the question of dark-ground illumination. If a wide-angle direct beam is focussed on the object by a dark-ground illuminator, and if each point on the object is thus illuminated by a cone of wide angle, so that each point behaves as nearly as possible as a self-luminous point, the resolving power should be given by at least the Rayleigh value· and, since there will be excess of illumination at the boundaries of the lens, the Rayleigh value may be exceeded.

This seems to agree with the best practice. Mr. Conrad Beck, in the same discussion, gives the following as an example of the resolution obtainable in dark-ground illumination:

Amphipleura pellucida—mounted in realgar—illumination a complete hollow cone of light, the minimum angle slightly more than the N.A. of the object-glass used and accurately centred—*transverse* cross-lines shown very faint with an aperture of ·95 N.A., very clearly with an aperture 1·2 N.A., and with an aperture of 1·4 N.A. the whole structure is shown as distinct dots. With *transmitted* light and crossed Nicols (to eliminate glare ?) approximately the same results are obtained and in both cases are on the theoretic limit of resolution. Sir Herbert Jackson brought forward similar evidence, *e.g.* convincing resolution of *P. angulatum*, using lens of 0·7 N.A. with a dry front paraboloid condenser or with a high-angle dry achromatic condenser with a central dark stop and an aperture transmitting a hollow cone ranging from 0·8 to nearly 1·0 (dots 1/45000 inch apart).

Deductions that have been made from Abbe's method are based on the supposition that Fig. 4 *c* represents the state of things for resolution, *i.e.* an aperture three times the aperture of the objective would be needed. J. Rheinberg, in attempting to reconcile Abbe with practice, brings refraction (in the object) to his aid. He says: "Not only does it seem reasonable to assume, therefore, that in the case of diatoms the direction of the light which impinges on them from the condenser annulus of light may be refracted or shifted, and that the central maximum enters the objective, but it would almost appear unreasonable to assume the reverse, *i.e.* that *only* diffracted light enters the objective in such cases." It seems to the writer quite true that complicated events occur at the separate particles of the object the precise nature of which can only feebly be conjectured. It may be mentioned that isolated particles, small compared with the wave-length, scatter light in a quite definite manner (Rayleigh, 1871, 1899) and this is probably the process by which the effective self-luminosity

is brought about. Abbe's method becomes unutilisable, of course, when this scattering becomes important. The prevailing opinion is that his method is not practical politics. The conditions laid down are very restricted, almost never occur, and do not in any case represent the most successful conditions for correct reproduction of structure.

The influence of wave-length has been brought to the fore, in recent years, in connection with bacteriological investigations. By the use of ultra-violet light and photographic examination with optical systems transparent to the rays used it is possible to double the resolving power, compared with that of visual observations. Fused quartz is used for the objectives, the immersion fluid being glycerine and water (or glycerine and cane-sugar solution as being more stable). The foregoing considerations apply to the possibility of the discrimination of structure inside an individual bacterium. When this is beyond the microscopic limit it can be dealt with only as a single object. "There are two directions in which progress is essential. One is to obtain greater visibility, and this is in process of achievement by a dark-ground ultra-violet method of illumination. The other is to devise methods of illumination by which short exposures, down to one-tenth of a second, may be made" (Barnard, *Roy. Soc. Proc. B.*, May 1929). One of the chief difficulties is the focussing in the case of moving organisms; the adjustment must first be made visually or with the aid of fluorescent screens, allowance is then made for chromatic difference of focus for the U.V. light and the photograph-plate is inserted. *Meanwhile the organism moves.*

Complicated though the conditions of working with a microscope are, yet are there any rules which may be followed advantageously by workers? The following seem to be important prescriptions.

1. Every point of the part of the object in view should be independently illuminated. This result can only be imperfectly realised, but it should be approximated to by focussing a broad source on the object.

Theory shows that a short wave-length is desirable. Therefore green should be chosen in preference to red; blue is usually too weak for ordinary use. Ultra-violet should be employed if photographic methods are available. "A fused quartz monochromatic object-glass of numerical aperture 0·35 computed for and used with radiation of $\lambda = 0{\cdot}275\ \mu$ gives nearly twice the resolving power of a lens of similar aperture computed for and used with light of $\lambda = 0{\cdot}51\ \mu$; while the fused quartz monochromatic lens of numerical aperture 1·2 has a resolving power 70 per cent. higher than that of a well-corrected object-glass of the same numerical aperture when used with

light of $\lambda = 0{\cdot}51\ \mu$" (B. K. Johnson, *Phys. Soc.*, Lond., Nov. 1929).

2. Care should be taken that the convergence of the condenser beam on the object is such that the whole of the aperture of the objective is fully utilised. If this is not arranged for, scattering which inevitably takes place in passing through the object will tend in the right direction to prevent failure; but it should not be relied upon. If the objective is not uniformly illuminated theory indicates that the excess of illumination should be near the rim, not near the centre (Rayleigh, 1879–80).

As little as possible of the object should be illuminated so as to diminish the amount of glare arising from to-and-fro reflections. The presence of glare sometimes makes it impossible to employ the full aperture of the lens (Beck, 1922).

3. Every detail on the image corresponding to less than one wave-length on the object is suspect. Calculations made for two-point objects equally illuminated do not apply rigorously to two lines (especially if unequally illuminated).

4. Owing to dioptric aberrations the full resolving power corresponding to the aperture must not be expected. Due to the same cause resolution may be effected in the middle of the field, but may only be realised in the peripheral part of the field by readjusting the focus.

Single Objects

The above considerations give no information in regard to the visibility of small objects, but only of the possibility of seeing true detail in their images. An object of any shape, but very small, compared with the wave-length used, scatters light in a regular (but not uniform) manner to an amount proportional to the inverse of the fourth power of the wave-length and directly as the square of the volume. The blue of the sky as seen through a dust-free air such as on Mount Wilson is due to the preferential reflection of blue, almost entirely by molecules of air. (Ultra-violet of shorter wave-length than $\lambda = 290\ \mu$ is all *absorbed* before reaching the earth.) To see minute isolated objects like the particles in a suspension (*e.g.* of gamboge) it is necessary to illuminate them by a powerful beam at right angles to the line of observation. The visibility of the particles is simply a question of the intensity of the source; for this reason sunlight concentrated by means of an appropriate lens system is the best. With this source gold particles as small as $5\ \mu\mu$ have been revealed.

With this arrangement, only the scattered light reaches the eye. The image seen is a diffraction image; neither the size

nor the appearance of this image bears any explicit correspondence with the object.

The question of the visibility of certain larger objects also comes under this heading. The late Lord Rayleigh has worked out several cases. For example, a luminous line on a black ground gives an image, when examined by an objective, much wider than its geometrical image. Again, for a black line on a ground consisting of isophasal light, he concludes that the bar might well remain visible when the width of the bar is only one thirty-second part of the minimum distance between *two* such lines which would allow resolution. The slightly darkened image of the bar has then a width equal to about sixteen times that of its geometrical image, and its apparent width is therefore quite illusory. In the case of a self-luminous background (*i.e.* with phases completely independent) a bar of the same width has only half the visibility of the previous case, but it should be easily recognisable when its width is one-third of the minimum width for resolution. In these cases the visibility is a question of contrast.

Apparent Interference of Independent Illuminations

The question of the impossibility of independent lights interfering can be illustrated by means of an experiment due to Johnstone Stoney, which however, was devised by him to illustrate the contrary.

As ordinarily used to produce spectra a diffraction grating is illuminated by light issuing from a narrow slit, and made into a parallel beam by a collimator. The whole of the grating is then illuminated by light which is in one phase at incidence (when this incidence is normal). The diffracted light is then collected by a telescope objective in the focal plane of which the spectrum is formed ; it can, of course, be magnified afterwards by means of an eye-piece. Usual diffraction theory shows that the distribution of illumination with angular position θ in the spectrum is given by an expression

$$I = r^2 \sin^2 u \ \sin^2 Nu$$

where, if r = grating space, s = grating bar, λ = wave-length,

$$u = \frac{\pi}{\lambda} r \sin\theta \qquad v = \frac{\pi}{\lambda}(r+s)\sin\theta$$

and N = total number of openings (see Preston, *Theory of Light*). This represents a series of narrow maxima whose angular

width is $\frac{2\pi}{N}$; these are the spectra. If, however, each opening is illuminated by a different source, so that no phase relation exists between their contributions, the same theory shows that

$$I = N r^2 \frac{\sin^2 u}{u^2}$$

and the above spectra are no longer present. This is due to each opening acting independently and the total light at any point in the focal plane being N times the light due to each opening.

Johnstone Stoney's experiment consists in first opening the slit as wide as possible, and focussing the source (*i.e.* the sun or a sodium flame) very accurately by means of a good telescope lens upon the grating itself instead of upon the slit. Each point on the grating is now illuminated by light which is almost completely independent of the rest. On examining through the telescope the fine spectral maxima have disappeared. Next, *leaving the light still accurately focussed on the grating*, narrow the slit down to its usual width; it is now found that the spectrum has reappeared with its lines approximately as finely defined as with the usual arrangement. *Stoney argued* (*British Assoc. Report* (Glasgow, 1901) *that the experiment showed that independent sources can interfere.* Since, however, this apparent interference occurs only after the slit is narrowed, it is proper to inquire what change this narrowing brings about. To answer this, it must be recognised that the slit is itself a diffraction opening, and the light from it, falling on the grating, is given by an expression of the form

$$y \quad r\frac{\sin u}{u}\cos(pt - u).$$

When r and therefore u are very small, as they are for a very narrow slit, the light from end to end of the grating is practically of uniform amplitude and uniform phase. This is more nearly the case the narrower the width of the slit. In the limit, the resolving power should become that obtained with the usual method of illumination and with *the same width of slit.*

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DARWINISM *VERSUS* LAMARCKISM

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We seem to be threatened with a recrudescence of the controversy over the Darwinian Theory. But now the conflict is not to be between the learned Professors of Biology on the one side, and the Church and the people on the other, but an internecine warfare, that is to say, between the Professors themselves. It has taken something like fifty years to secure what we might call orthodoxy among the elect; now all is to be thrown into the melting-pot again. Some of those who have broken away have returned, or are apparently returning, to the faith of Lamarck, some find solace in Mendelism, while others, like Prof. Watson, have abandoned all hope. They bid us turn to physiological experiments to redeem the parlous state in which the evolutionary theory now finds itself.

But we have not lost *everything*. All the protagonists are agreed as to the reality of evolution. They question only the sufficiency of the Darwinian theory of Natural Selection to account for it. This they now regard as a soothing syrup which has lost its virtue.

The malcontents are a formidable body, but their position is being undermined. While, however, the reformers are at variance deplorable results may follow, and there seems little hope of stabler reconstruction until the folly is recognised of seeking for some common factor of evolution: there are many; and Natural Selection is *one* of them.

For long years the peculiarities which distinguished the shape and coloration, habits or habitat, of the various species, and larger groups of animals—and of plants too, for that matter—were supposed to be due to the action of Natural Selection. Observation has shown that, in spite of the apparent stability which any given species displays in the matter of its form, size, and coloration, yet no two individuals of that species are ever *exactly* alike: they display a tendency to "vary," now in one direction, now in another. Here, it is contended, is the material on which Natural Selection works. Variations which prove useful in the "struggle for existence" give such individuals an ascendancy over those which do not possess these

helpful characters. And, it is contended, since these variations are blastogenic, or congenital, they are transmissible to their offspring, and sooner or later, the "haves" crowd out the "have-nots." This has been the standpoint of the Darwinians.

The evolution of the long neck of the giraffe came into being, according to Lamarck, by the constant straining of the ancestral giraffe to reach leaves almost but not quite out of reach. As a consequence of this persistent stimulus the neck increased in length in every succeeding generation. Why it ever stopped this process of growth did not, apparently, occur to him to ask. Nevertheless, as I hope to show, his theory demands our very serious attention.

Darwin again and again showed that he regarded the Lamarckian interpretation of Use and Disuse with sympathy: though he held that Natural Selection afforded a better interpretation. But the Darwinians of to-day regard Lamarckism as utterly discredited.

Darwin's preference for the agency of Natural Selection rather than Lamarck's factors of Use and Disuse are clearly apparent when he discusses the matter of the evolution of the giraffe. He held that the development of this elongated neck could be more easily explained on the assumption that it followed as a consequence of prolonged periods of drought, during which all the members of the tribe were competing one with another for food, when only those with the longest necks would survive. In other words, they would be able to avail themselves of food which was out of the reach of those with shorter necks, which would consequently perish. This interpretation was based on his recognition of the variability just referred to presented by plants and animals of all kinds, whereby Natural Selection finds plenty of material to work upon under the conditions just postulated, more especially since he believed that variation was affected, in some inexplicable way, by external conditions. Those only with the longest legs and necks would survive this ordeal of drought. Even if such periods of stress occurred only once in fifty or a hundred years there would be ample time, in the course of ten thousand years or so, to bring about the evolution of the giraffe as we know it. For after each of such crises the surviving breeding-stock would show a higher average stature than before. The very striking differences in coloration which the several species of giraffe display, as well as the prehensile tongue, can, in like manner, be explained on the theory of Natural Selection, and *are* so explained to-day.

The sweet reasonableness of this interpretation is illusory. The variations which we observe between different individuals of any given species, though easily demonstrable, are not, it

has forcefully been objected, sufficiently large to have any survival value. That is to say, when we substitute critical analysis for hypothetical cases we find that they are not sufficiently large to enable competing individuals to derive any benefit therefrom. Some of these variations may, indeed, be due to nurture rather than to nature ; in other words, they are not congenital, only these last being transmissible to offspring. These heritable characters, I ventured some years ago to suggest, are free to go on increasing in magnitude until at last they become either definitely harmful or definitely beneficial in the struggle for existence. They come finally, in short, within the ambit of Natural Selection. And there seems good reason to regard such critical cases as furnishing one of the modes of Natural Selection, which, however, in spite of its more ardent champions, cannot be regarded as the sole cause of evolution.

All animals, at any given moment, are in harmony with their environment, and all furnish ample evidence of adjustments to meet the conditions of that environment, as well as the conditions necessary to secure the co-ordination of the internal functions of the body. But the nature and the degree of adjustment to the external environment shows varying degrees of intensity, so that it is by no means always possible to divine the full range of the activities of which this or that animal is capable, and this fact seems to have proved a stumbling-block even to zoologists of the first rank. Prof. D. M. S. Watson, for example, in his Presidential Address to the Zoological Section of the South African meeting of the British Association, contended that, save perhaps in the position of the eyes, there is no indication of the aquatic habits of the hippopotamus. Yet, surely, this is an overstatement. It is not only the position of the eyes, the whole head proclaims a habit of submergence, since eyes, nostrils, and ears are all placed on the same level so that all can be kept above water while the rest of the body is submerged. But quite apart from the head, when one comes to survey the great bulk of the body, the naked skin, and short, splay-footed legs, where else, but in or near water, would one, seeing such a creature for the first time, suppose its haunts to be—not on a sandy desert, or on the ledges of precipitous cliffs, or scrambling about in trees ! It might, one might venture to surmise, haunt the thickets of steaming forests, or lush meadows, but the head would finally assure a correct solution. The feet are certainly not those of a swimming animal, nor of a diver. The hippopotamus is both. Why, then, is this fact not proclaimed in the appropriate structural modifications ?

At first sight this question might seem disconcerting. But there are some instructive parallels, and some of the

most striking of these are to be found among the burrowing animals. The sand-martin, the kingfisher, and the bee-eater are all expert burrowers. Yet it would seem incredible, if we were shown one of these birds for the first time, and were told it was a burrower, for all have the feeblest of feet, and shortest of legs, and beaks fitted apparently for any purpose but burrowing. Who, again, would attribute the ability to burrow to a rabbit? especially having regard to the fact that our standard of burrowing mammals is the mole. In like manner, we should not attribute swimming habits to the tiger, yet we know that it will swim long distances.

Why, then, since the hippopotamus is so thoroughly aquatic, and since the martin, the kingfisher, and the bee-eater are such expert burrowers, do they show no structural modifications to subserve these ends?

A little reflection will show that structural modifications or "adaptations" for particular ends are found only where the organ concerned is used intensively. Burrowing, in the case of the rabbit, and the birds just referred to, is but an occasional and transitory activity.

The conditions of life imposed on the hippopotamus effectively prevent the transformation of the legs into swimming organs since long distances have to be covered on land in search of fresh feeding-grounds. But the head has become profoundly modified in two directions in response to the conditions of existence. The shifting of the nostrils, eyes, and ears to the same plane is a response to an urgent need to be able to remain submerged for long periods, while the enormous mouth and the singular form of the teeth are, as obviously, adjustments for feeding. These changes of form are responses to continuous and persistent needs: they are adjustments due to intensive use. Burrowing, in the case of the rabbit, and the birds just referred to, makes no more than casual demands on the structures used in the work of excavation; their main functions are of a very different character, and it will be noted that their size and shape are directly related to their *use*, dynamically. Thus the beak of the sand-martin is exceedingly feeble, and extremely short, while the mouth is wide. The beak is not used for seizing and tearing up prey; it is merely a framework for the mouth. The legs are never used for walking, but merely for the support of the body when at rest; hence their feeble character. This is surely a more reasonable interpretation than that which attributes the origin of these characters to the action of Natural Selection acting on small variations.

The Lamarckian theory of Evolution by "use" has been misinterpreted: and we shall neither discredit it, nor find the solution we seek in broad generalisation derived from morphology,

or in physiological experiments; but in a more intensive study of living animals, and the nature of the adjustments they have undergone in relation to their mode of life.

Turn for a moment to such adjustments in the case of aquatic animals, taking, as a standard of comparison, the amphibious otter. Here we have a long, lithe body, short legs, and webbed feet. In the sea-lion, more intensively aquatic, the fore-limbs have become transformed into "flippers," but still capable of serving the function of legs, though to a very limited extent. The hind-legs have become still less efficient for locomotion on land, and take little or no part in swimming; the tail has practically vanished. In the seals, locomotion on land has been reduced to the smallest possible limits; it can, at best, do no more than shuffle along. The "flippers" serve merely to perform these shuffling movements, or for steering and balancing when in the water. The hind-limbs are no longer of the slightest use on land, being now turned permanently backwards, with their soles facing one another. In the water they are used as an aid to swimming.

Passing now to the Cetacea, we find what should indeed be convincing evidence of the effect of *use* and *disuse*, and their bearing on evolution; and, in proportion as this is appreciated by the apparent importance of the concept, the "selection of small variations" will diminish.

In all the Cetacea the body is more or less fusiform in shape, and hairless, a peculiarity which they share with the Sirenia. In both types the fore-limbs have become transformed into flippers, while the hind-limbs, and the hip-girdle, are reduced to the merest vestiges, as a consequence of their lack of use.

But more than this, they have developed special structures—dorsal and caudal fins—in response to stimuli consequent on their mode of life. These differ from the fins of fishes in having no bony skeleton, and furthermore the caudal fin is horizontal, instead of vertical as in the fishes. The size and development of these fins varies, we may assume, with the speed of movement through the water. In the Killer-whale the dorsal fin may attain to a length of 6ft., it is large in most of the dolphins and porpoises, and in the rorquals, but it is wanting or feebly developed in the white whale (*Beluga*) and narwhal (*Monodon monoceros*), in *Neomeris*, and the fresh-water *Lipotes*, as well as in the right-whales and the Sirenia. It is significant that it occurs on the summit of the curve formed by the bending of the body in diving, as if in a response to an incessantly recurrent stimulus formed by the swirl of the water at this spot. The horizontal position of the caudal-fin we must again refer to the reaction of the skin to the resistance of the water incurred by the up-and-down movements of the tail in swimming, and it

is significant to note that only in the marine Sirenia do these tail-flukes agree with those of the Cetacea in being triangular; in the fluviatile species they take the form of rounded flabella. It seems incredible that the position, form, and size of these fins can have come into being solely as the result of the selection of small favourable variations. Such a theory demands the assumption that there must have been a tendency to produce one or more incipient dorsal-fins, either simultaneously or at different times, all along the back, and that but one of these gained a chance of survival through the accident of its position, just where we find it. Furthermore, what useful part could the incipient stages of such a fin have conferred in the "struggle for existence"? On the postulate that such a fin arose in the first place on the site of, and in response to, a persistent stimulus, we are not called on to account for its "use" during these early stages. Indeed it may still have no "use" in any save where it is conspicuously large, where it serves to keep the body from rolling over during movement at speed. The increase in size began with, and kept pace with, increased speed.

This reciprocity in the development of organs we find everywhere. We may sometimes trace it to changed habits; while in other cases we can, at present, only attribute it to "idiosyncrasies of growth."

When the hoatzin took to eating the leaves and astringent seeds of the Duri-pimpler (*Drepanocarpus lunatus*) the crop not only responded by greatly thickening its walls, but it assumed also the function of the gizzard, which, from lack of use, has degenerated. As a further consequence, the pressure of this large and solid crop gradually forced back the keel of the sternum, declining from lack of use, until to-day no more remains than a small triangular ridge at the posterior end of the sternal plate. But for the fact that this bird has no need for long flights, the keel of the sternum, instead of retreating, would have opened out to lodge the crop, as it has done to lodge the wind-pipe in the case of some swans and cranes, where flight is essential. Here, indeed, we have, as Gadow long since contended, a case of the transmission of "Acquired Characters," though a thousand years or so may have been taken in the process.

This interpretation is easier to accept than that this singular modification of the crop has come about by the selection of small—and purposeful—variations which must, moreover, have been accompanied throughout by equally small—and purposeful—variations affecting a totally different structure, to wit, the sternum. The physiological experiments for which Prof. Watson pleads, if endowed to ensure their unbroken continuance for a thousand years or so, may prove that a like

change in the form of the crop, correlated with a like change in the form of the sternum, may be induced by some other, and as yet unsuspected, agency; but for practical purposes we must take our evidence as we find it.

Let us return to this matter of reciprocity in development. In the swans and cranes just referred to the trachea displays a remarkable hypertrophy of growth, far exceeding the length of the neck. As a consequence, in the course of the development of the individual there is formed a loop which impinges against the anterior border of the keel of the sternum. As the pressure increases—and at any given moment this must be infinitesimal—this keel, normally a mere blade of bone, opens out to form a cavity which gradually lengthens with the "thrust" of the trachea, and eventually extends to the extreme posterior end of the keel. In the black guinea-fowl (*Guttera cristata*) the trachea impinges on the hypocleidum, the disc-like expansion formed at the junction of the clavicles. As a consequence this disc becomes transformed into a cup for the reception of the sternal loop.

We can hardly regard such singular structures as the result of Natural Selection and the struggle for existence, or, to put it another way, of the selections of "favourable variations." But they become intelligible as idiosyncrasies of growth which have survived because of this "reciprocity" between even unrelated structures, enabling them to adapt themselves to persistent stimuli.

In this connection we have to reckon also with the phenomena, and the mode of disuse, the evidence of which is furnished in its most palpable form by "vestiges": and less obviously in cases where we find, on the other hand, an excessive development of some particular organ, or set of organs.

The common mole may well be selected from among a bewildering number of illustrations of the more emphatic effects of "use" in changing the form of animals. The profound modifications of structure displayed in its skeleton, the loss of sight, and the peculiar character of its fur are all so many adjustments to that changed mode of life which are associated with its burrowing habits.

The mere mention of "use" revives the old controversy over the blacksmith's arm, and its non-transmissibility; as well as the contention that if the theory were true the arm would in time come to dominate the rest of the body, like the tail that wagged the dog. That this hefty arm had not the slightest chance of becoming a heritable character must be obvious, from the fact that its size depended entirely on its continuous performance of the work demanded of it. A brief period of rest is attended by a decline in its power. The black-

smith's offspring, therefore, could hardly be expected to inherit so fleeting an accretion, and especially so since this is developed only by the male parent. In the matter of its size, again, this arm is no bigger than the work demanded of it.

The peculiar structural features of the mole—the short arm, the enormous hand, the long claws, the feeble hind-limbs, and the still feebler eyes are all directly, or indirectly, the effects of "use." And they are heritable characters because the energies of both sexes, throughout their whole life-time, are expended in digging. They must dig or die, for only thus can they find their food, and they eat ravenously.

A ready and highly probable interpretation of the effect of these activities almost forces itself to the front. Animals eat, we say, when they are hungry. The food ingested goes to the repair of exhausted tissues, skeletal, muscular, and nervous. Those which have been used most will need, and absorb, most of this food. Those organs which have been comparatively little used will require least, and will absorb least, of this nourishment. As a consequence, then, of this relationship, sustained uniformly by every member of the species, and for countless generations, organs but little used will slowly be starved out, and ultimately reduced to mere vestiges, finally to disappear altogether.

Exactly how the effect of these activities, or their suspension, is transmitted to the offspring has, so far, presented an insoluble problem, since there seems to be no discoverable means of showing how the modifications undergone by this or that set of tissues could be transmitted to the germ-plasm, and thus to the offspring. But let us assume that each particular tissue is not a mere "substance" formed afresh during the antenatal life of each individual, but is an entity in itself, just as the whole complex of tissues makes up the entity which we call the individual. In this case the "experiences" of each of these "entities"—the stimuli to which they have been subjected, the responses in growth-form they have made, and the amount of nourishment they have absorbed in consequence of such responses—would become part of their ontogenetic recapitulation, a fixed habit of growth, and hence would appear in successive generations of themselves, which, *en masse*, have determined the shape of the body we know as, say, a mole, or a horse.

If this interpretation is even approximately correct, we are rid of the necessity, which some insist on, of regarding all characteristics whatever of living bodies as the outcome of Natural Selection. But it is always to be remembered that evolution is the expression of no one factor, but of many factors; and, finally, that Natural Selection is the final arbiter

of what shall survive of these processes of adjustment to the demands of the environment.

Thus there is evidence to show that some organs, like the tusks of the elephant and the antlers of the extinct Irish Deer, acquire a certain impetus of development which does not cease after these weapons have become no longer useful. The tusks of the elephant are used as weapons and for uprooting trees. When the mammoth and the American *Elephas columbi* came to live in areas where conifers only existed, the tusks, no longer needed to secure fruit by felling trees, instead of decreasing in size from lack of use, became hypertrophied, possibly as a result of the stimulus of their own weight on the pulp cavity of the tooth. Hence they not only grew to an inordinate size, but their points crossed, rendering them useless either as weapons or as lifting organs. In like manner the antlers of the Irish deer increased beyond their need, by reason of the stimuli set up by their great weight, till they became a burden too great to be borne in the flight from wolves. Hence, in each of these cases Natural Selection terminated their existence.

There are yet other aspects of evolution awaiting revision, and of these the most important are sexual-selection, the origin and development of secondary sexual characters, coloration, physiological selection, isolation, and changes of habit ; all are of first-rate importance. Any attempt, however, at an analysis thereof would tend to distract attention from the issue now raised. Suffice it to say that for these also some theory other than the selection of small variations is necessary to explain their origin and development, though an exception is perhaps to be made in the case of coloration—protective, and warning—when natural selection seems indeed to play an important part.

We can explain much on the assumption that the several tissues of the body are not mere " substances " formed afresh in the ontogeny: nor are they the equivalent of " genes," but rather, as I suggest, so many separate entities, each with a pedigree of its own, and each having been moulded in the course of its history by the conditions of existence to which it has been subjected.

The consequent effect of such conditions on these several entities determines the shape of the body they compose, governing also its responses to stimuli as well as its functions and emotions.

On this interpretation an explanation is forthcoming for the special structures peculiar to larval stages. Take, for example, the often remarkable modifications of free-swimming larval stages of Crustacea, Mollusca, and Echinoderms, as well as of many fishes and Amphibians. The often fantastic forms

to be found in larval Crustacea are, in the first place, the expression of the responses to stimuli of the tissues affected. The apparent contrariness of larvæ of nearly allied species—in the matter of their shape, development of spines, and so on—found in the same environment is explained by idiosyncrasies of growth: similar tissues reacting very differently to like stimuli because of subtle differences in the qualities of the somatoplasm. This interpretation is at least as likely to be correct as that which attributes these peculiarities to "the selection of small, favourable variations."

There seems, then, to be no escape from the introduction of a large leaven of Neo-Lamarckism into the Darwinian theory of Natural Selection. We cannot, in short, escape from the conclusion that the various organs of the body are moulded by the effects of the stimuli to which they have been intensively subjected, rather than that they have come into being solely by the "selection" of fortuitous variations in this or that direction. The close correlation between structure and function which we find everywhere is interpretable on the assumption of the effects of "use" and "disuse," and the recognition of the inheritance of "Acquired Characters" follows as a consequence. That correlation is not explainable on the theory of Evolution by the selection of "small, favourable variations."

If it be objected that the truth of my arguments cannot be demonstrated, they will yet stand in no worse case than those which insist, on the one hand, that we can explain everything on the theory of Natural Selection, and on the other that we are bankrupt and can explain nothing.

RIVER TERRACES AND RAISED BEACHES: THE WORK OF THE COMMISSION ON PLIOCENE AND PLEISTOCENE TERRACES AT THE INTERNATIONAL GEOGRAPHICAL CONGRESS, 1928

By B. R. M. SANER, B.A.

Part II

In the first part of this article[1] reference was made to the object of the Commission on Pliocene and Pleistocene terraces—namely, to determine the existence of constant levels, on the Atlantic and Mediterranean coasts of Europe and throughout the world; in other words, to prove or disprove the theory that changes of base-level in Quaternary time were due to Eustatic not Tectonic movements. The Eustatic theory, first suggested by Suess,[2] was based on the reported occurrence of elevated coral reefs, and the evidence of relative uplift which can be found on the coasts of the continents in all latitudes. Without consideration of any difference there might be in the age of the uplifted coast-lines, Suess postulated a recent worldwide lowering of sea-level to explain all the "raised beach" phenomena.

De Lamothe and Depéret, working on the river terraces and raised beaches of the Western Mediterranean and France, found that certain shore-lines traceable throughout the Western Basin of the Mediterranean could be correlated with the raised beaches and river terraces on the Atlantic coasts of France. Depéret[3] has attempted to disentangle the problems of Quaternary Chronology by making marine cycles of sedimentation his basis, and correlating the river terraces and glacial deposits with the marine stages.

Each of Depéret's stages represents a complete cycle of sedimentation—that is (*a*) regression of the sea (relative uplift of the land) with coast erosion; (*b*) transgression of the sea

[1] Science Progress, vol. xxiv, no. 94, October 1929.
[2] *Das Antlitz der Erde*, ii, pp. 399–41.
[3] "Essai de Coordination Chronologique Général des Temps Quaternaires," Charles Depéret, *Compt. Rend. Acad. Sci., Paris*, 1918–20.

(relative sinking of the land margins) with sedimentation. Thus the features actually preserved as evidence of any stage may be the beach deposits of the last shore-line, or off-shore sediments, or erosion features. There are four, or rather five, stages :

(5) *Post-Monastirian Stage.*
(4) *Monastirian Stage.*—Corresponding to shore-line 18–20 metres.
(3) *Tyrrhenian Stage.*—Corresponding to shore-line of 28–30 metres. (This stage is quoted as 30–35 metres for the Atlantic coasts of France.)
(2) *Milazzian Stage.*—Shore-line of 55–60 metres.
(1) *Sicilian Stage.*—Shore-line of 90–100 metres.

River terraces which correspond to the several stages occur at the same heights above present river level.

Depéret's five stages correspond to the four Glacial Periods, and the post-Glacial period, of Penck and Brückner, but he is the first to correlate the Alpine river terraces with the Mediterranean sea terraces, and to make these four outstanding terrace levels the basis for Quaternary chronology in Europe.[1]

Depéret's system is criticised, especially in America, on account of his assumption that the changes of base-level which caused the terrace features are due to Eustatic movements of sea-level, not to any movement of the land-mass.

The report on the Terraces of the Atlantic Region of France by M. E. Chaput and M. G. Dubois forms a concise summary of the knowledge of the French Quaternary. The authors, however, are in favour of Depéret's system, and rely on the evidence of levels. The first part of the report deals with the Raised Beaches (Marine Terraces) of the north-western and western coasts of France; these may be divided into three groups according to their altitude.

(1) Terraces above 20 metres; corresponding to the Tyrrhenian, Milazzian, and Sicilian of Depéret.
(2) Terraces 10–20 metres. Monastirian of Depéret.
(3) Terraces below 8 metres to 30 metres, the "Flandrien" of Dubois.

(1) In the North, de Lamothe has described abrasion platforms covered by pebble deposits at a height of 50–55 metres at Cap Griz-Nez, and at 100 metres south of Sangatte (near Calais), and M. Briquet recognises a shore-line at 140–50 metres near Noires-Mottes. There are also abrasion platforms

[1] "Old and New Standards of Pleistocene Division in Relation to the Prehistory of Man in Europe," H. F. Osborn and C. A. Reeds, *Bulletin of the Geological Society of America*, vol. 33, pp. 411–90, July 1922.

(wave-cut beaches) on the cliffs of Picardy at 55–60 and 100 metres, corresponding to Depéret's Milazzian and Sicilian stages. At St. Valéry-sur-Somme a deposit of sand and shell-fragments with rolled pebbles at 35 metres is thought to be a storm beach (*cordon littoral*) corresponding to the Tyrrhenian stage.

In Normandy there are planated surfaces at about 100 metres, which is below the level of the plateau; they may be surfaces of marine erosion or peneplains; Depéret has included them in his Sicilian stage. Wave-cut platforms and terraces are frequent in the Armorican massif; they may form "giant staircases," for example near Cherbourg and Vannes, or the slopes between the terraces may be gradual and drift-covered. MM. Barois, de Lamothe, Chaput, and Terronière, have described platforms at 30–40, 55–60, and 90–100 metres. Recently M. Baulig has done experimental work with hypsometric curves (graphs in which the area of the surface is plotted against the height of land) and obtained indications of planation at 128–90–65–51–42 metres, and at 25 and 10 metres. His work seems to indicate that there has been no warping of these plains.

Between La Vendée and the Landes there are indications of platforms at various heights between 10 and 40 metres; for instance, north of the Marais de Poitevin deposits with marine shells are found on the plateau at 35 metres. These deposits have been attributed to the Pliocene, owing to their position above the Quaternary valleys, but de Lamothe and Depéret correlate the deposits with the Tyrrhenian stage, thus making the valleys post-Tyrrhenian.

(2) Of the lower beaches (10–20 metres), the Sangatte raised beach near Calais is famous. This beach was first described by Prestwich in 1851, and later by Briquet, Depéret, and Dubois. The deposits are thought to have been formed below sea-level. The lowest beds are gravel, laid down on a sloping littoral platform; these are followed by sand-beds, and possibly the highest beds represent a storm-beach (*cordon littoral*). The sediments are now 11 metres above sea-level, sloping towards the sea and overlaid by scree and clay (*limon*). They yield a rather more arctic type of fauna than the present fauna of the English Channel.

Opinions differ as to the height of the shore-line at the end of the phase of deposition represented by Sangatte "beach." Briquet places it as low as 8 metres above present sea-level, while Depéret includes the beach in his Monastirian stage (18–20 metres). Dubois suggests 12 metres. Inland from Calais, one or two hills capped with sand and gravel stand out above the general level of the plain, for example, at Coulogne,

Dubois considers that these are beach deposits and correlates them with the Sangatte beach.

In Picardy there are several ancient storm-beaches (*cordons littoraux*). These sands and gravels yield marine shells (*Tellina balthica*), and attain a maximum height of 12 metres above present sea-level. Briquet considers that these banks of pebbles were thrown up by a sea which stood 5–6 metres above the present level. Dubois, however, notes that the gravels are decalcified and much degraded on the surface; he therefore concludes that their original altitude was at least 15 metres and accordingly correlates them with the Monastirian. Near Abbeville, in the Somme Valley, there are sands with a cold temperate fauna (*Tellina balthica, Cardium edule, Buccinum undatum*) at an altitude of 11 metres. Again in this case opinions differ as to height of the shore-line. Briquet is in favour of a 5-metre sea, while de Lamothe and Depéret claim that the sands were deposited when the sea stood at 15 metres, and therefore form further evidence of the Monastirian stage in the North of France.

The chalk cliffs of Normandy are evidence of the coast erosion which is still in progress. There are hanging valleys south of Havre, which have been beheaded by the retreating cliffs. Depéret claims further evidence of the Monastirian stage in the estuarine at the mouth of the Ste Addresse Valley in the Seine estuary. These beds are 10 metres above sea-level and consist of peat and sand with a marine fauna of existing species. North of Caen there is a deposit at 6 metres with a mixed fauna of existing species, and those belonging to a colder climate (*Buccinum grœnlandicum*), which Depéret attributes to a pause in the post-Monastirian retreat of the sea.

In the Armorican massif there are features which indicate ancient shore-lines. For instance, sands with *Cardium edule*, at a height of 14 metres at Mont Dol, correlated with the Monastirian by Depéret and Dubois. Also deposits of indeterminate age, 6–7 metres above sea-level, at St. Malo and Mesquer. Other unfossiliferous sands and gravels ranging from 5 to 20 metres are found on these coasts, some of which are included in the Monastirian by Depéret. At Croisic there is a reef shattered by waves above an abrasion platform at 12–15 metres.

Between La Vendée and the Pyrenees the only deposits which fall within this Monastirian group are the shell banks—Kitchen Middens—which attain a height of 12 metres. These banks contain oysters with both valves intact, and other molluscs; the fauna is not earlier than the Monastirian stage (Dubois) and Depéret considers that they may be shell banks thrown up in the Monastirian sea.

(3) The Flanders stage (Flandrien of Dubois) is a cycle of sedimentation initiated at the end of the Palæolithic, and still in progress, which has formed the extensive coast plains of Flanders and Picardy, and those which extend from the Loire to the Pyrenees. M. G. Dubois, in his memoir,[1] has collected the evidence of numerous borings in his type area between Calais and the river Scheldt. This plain is about 10 kms. wide and 3–4 metres above sea-level. The borings show that 20–30 metres of Quaternary sediments rest on the Tertiary or Cretaceous floor. The boring at Coquelles is the best example; here sediments 22½ metres thick are found above the Cretaceous.

v.	·6	metres	peat.
iv.	2·1	,,	Polder clay.
iii.	15·9	,,	Sands with *Cardium edule*, *Tellina balthica*, etc.
ii.	·6	,,	Peat.
i.	5·6	,,	Polder clay with sand bands and tooth of *Elephas primigenius*, var. Sibirica.

Dubois divides this Flanders cycle into four stages:

iv. *The Present Plain.*—A modern terrace, still subject to inundation if the dykes are neglected.

iii. *Upper Flandrien.*—Dunkerque beds—marine sands and peat—Gallo-Roman remains, and in some places relics of the Middle Ages.

ii. *Middle Flandrien.*—Calais beds—marine sands and peat, with remains of Neolithic industry.

i. *Lower Flandrien.*—Ostend beds—corresponding to the end of the Palæolithic. *Elephas primigenius*, *Corbicula fluminalis*.

Evidence of conditions similar to those described in the Flanders plain has been found by Dubois in the maritime plain of Picardy, also near Havre and Cancale, at the mouth of the Loire, and the estuaries of the Charente, Seudre, and Gironde. The deposits probably continue to the Spanish frontier under the dunes of the Landes. The sedimentation seems to have been continuous; Neolithic remains were also found 6 metres below low-water mark at St. Nazaire.

The Flanders stage thus shows remarkable continuity throughout the Atlantic coasts of France, and forms one readable paragraph in the history of the Quaternary. Since the end of the Palæolithic these coasts have sunk, relatively, about 25 metres, but sedimentation has kept pace with the sinking

[1] "Recherches sur les terrains quaternaires du Nord de la France," *Mémoires Soc. Géol. du Nord*, t. viii, Mem. No. 1, 1924.

and the water has always been shallow, as the peat-beds interstratified with sands have proved. M. Chaput considers that the raised beaches, banks, and wave-cut benches, which occur between 12 and 20 metres, indicate a period of stability when the sea stood at about 15 metres—Depéret's Monastirian stage. But other workers in France do not always agree with this grouping. The higher stages are rarely represented by sedimentary deposits, but remain as plains or peneplains which may be due to continental or marine erosion. The 90–100 stage appears to be the most marked (Sicilian) and later stages at 55 and 35 (Milazzian and Tyrrhenian) metres are also clearly traceable. Depéret seems to have a very fair case in favour of his system of classification—but there is no convincing evidence in favour of the Eustatic theory, except perhaps at the Flanders stage.

The second part of the French report summarises the results of an immense amount of work which has been done on the river terraces of France.

There appears to be a well-marked high-level terrace or peneplain, about 90–100 metres above sea-level, traceable throughout the area. This plain forms an important feature of the topography in the North of France, Flanders, Artois, and Picardy ; Briquet [1] suggests that it may be continuous with the peneplain of the Paris Basin, which is 150–200 metres above the present valley floors. This higher plain was thought to be of Oligocene age by de Lapparent, but Briquet regards both plains as late Pliocene, and suggests that they have been warped in such a way that the level varies from 100 to 200 metres.

In the Seine Valley there is a distinct terrace at 90 metres, which by its relation to the meanders is clearly a terrace and not the fragment of a peneplain. In the absence of conclusive evidence it would seem that the actual facts favour de Lapparent's original idea that the 150–200-metre plain is older than the 100-metre plain, whether it be Oligocene or Pliocene. The presence of the 90-metre terrace in the Seine Valley supports this interpretation rather than Briquet's suggestion.

On the south of the Armorican massif above the Loire plateaux which interrupt the hill slopes of Mauges and the "Bocage Vendéen," at 90–110 metres, are conspicuous. Chaput attributes these to Depéret's Sicilian stage. G. Denizot [2] suggests, however, that since Pliocene deposits (the Sables Rouges of Brittany) attain a height of 100 metres, the platforms are also Pliocene. Chaput argues that a peneplain of continental

[1] A. Briquet, *Ann. de Géog.*, t. xvii, 1908.

[2] G. Denizot, "Les sables de la Basse-Loire," *Bull. Soc. Géol. et Min. Bretagne*, 1926.

erosion could hardly terminate a phase of sedimentation; he therefore claims that the platforms are Sicilian, but have remanié Pliocene sediments scattered on their surface. Without more detailed knowledge of facts it is impossible to decide this point; but it is suggested that Chaput's argument is not infallible.

Very similar conditions are found in Aquitaine; there are sediments similar to the Sables Rouges of Brittany, occurring up to same height as platforms, at 120–30 metres. Chaput again suggests that the platforms are later than the sediments. Lignites with a Pliocene flora have been found south of Biarritz; they are covered by alluvium, which attains a height of 80 metres above sea-level; they may be correlated with the Sables Rouges and the Pliocene deposits of Aquitaine.

Of the lower river terraces only those near enough to the river mouths to be obviously connected with the marine "terraces" have been considered in the report.

The Somme is the only river north of the Paris basin whose terrace record is significant. The other rivers of Flanders, Artois, and Picardy have one or more terrace features which Chaput correlates with the several marine stages; some rivers, *e.g.* the Aa, show evidence of recent (Flandrien) adjustments.

There is difficulty in interpreting the evidence of Somme terraces owing to the well-developed incised meanders of the stream. Since such meanders are prominent features in the other great rivers of France, it may not be out of place to discuss their influence on terrace formation in rather more detail.

When the "thalweg" of a river approaches the theoretical curve of equilibrium and the valley is graded to the base-level of erosion, the river ceases to degrade its valley by vertical erosion, but lateral corrasion, causing the widening of the valley floor, is the active process. As long as the stream is straight corrasion of both banks is equal, but should the river begin to wind (meander) the current will impinge with greater force on the concave bank, eroding that bank, while deposition takes place in the slack water near the convex bank. By this process the meanders increase in amplitude and complexity, widening the valley floor, and at the same time each meander tends to migrate downstream as the current sets most strongly against the downstream side of the concave bank.

If the base-level is constant, and there is no change in the volume or load of the river, such meandering simply widens the valley floor till it becomes a flat alluvial plain, on which the deposits are constantly shifted and resorted and gradually moved downstream. If the load is decreased but the other factors remain constant, the river will begin to degrade the

valley floor and form the alluvial terraces mentioned in the first article,[1] and described by W. M. Davis.[2]

Any relative increase in the load or rise in the base-level will cause increased deposition and aggradation of the valley floor.

If the river is "rejuvenated" by relative uplift of the land, vertical corrasion will again exceed lateral corrasion, and the meanders will be preserved as the river lowers its bed, leaving the old valley floor as a terrace. But Chaput has noted that unless the down-cutting is very rapid, deposition will still occur on the convex side of the meander and a long drift-covered slope may be formed, inclined towards the stream and towards the mouth of the river; such features he has termed "terrasses polygénique"—polygenetic terraces—to distinguish them from the simple step terrace (*terrasse monogénique*), since the long slope is really a series of small terraces. The combination of such slopes with the more simple terrace features of a meandering river causes a complicated terrace sequence in which the evidence of major changes of base-level is not easy to find.

Opinions have differed widely as to the number of distinct stages which can be read from the terrace features in the Somme Valley, but de Lamothe has distinguished three groups of terraces which are more or less parallel to the present Somme (before canalisation); these may be summarised as follows (the figures quoted refer in all cases to heights above the present rivers):

(1) 50–52-metre terrace.

(2) 30-metre terrace. Warm fauna—*Elephas antiquus*—and *préchelléen* flint implements. Correlated with the Tyrrhenian 30-metre beach at St. Valéry.

(3) 12-metre terrace. Mixed fauna—late Chellean or Acheulian industry. Correlated with Monasterian beach deposits of Picardy as distinguished by Depéret.

The valley of the Seine between Paris and the Channel is remarkable for the well-developed incised meanders which show polygenetic as well as monogenetic terraces. M. Briquet was led to distinguish a great number of stages, but Chaput recognises only three terraces which correspond to phases of stability, the remainder being drift-covered slopes, corresponding to phases of gradual down-cutting.

(1) 50-metre terrace, on which the deposits show deeper weathering than those of later stages.

[1] SCIENCE PROGRESS, vol. xxiv, no. 94, Oct. 1929.

[2] *Geographical Essays*, by W. M. Davis, p. 374, "The River Terraces of New England."

(2) 32-metre terrace. Well preserved at the mouth of the Risle and in most of the meanders. Warm fauna and Chellean implements.

(3) 16–18-metre terrace. Seen in the meanders of Caudebec and Duclair, separated from the present flood-plain by a well-marked slope. " Mixed " or " cold " fauna, Acheulian-Mousterian industry.

The rivers flowing to the Atlantic between the Seine and the Loire have few well-developed terraces, and the deposits are thin (1 metre). The Orne has a 15-metre terrace which is older than clays (*limon*), yielding Acheulian implements. There is also a 70–metre terrace.

The Loire has been studied by Terronnière and Chaput independently, and they have both arrived at the same general conclusions :

(1) 55–60-metre terrace.
(2) 35–40-metre terrace.
(3) 15-metre terrace.

The two lower terraces extend into wide alluvial plains, and can be easily correlated with the well-developed marine features at corresponding heights.

In the valleys of the Seine, Loire, and Somme there are features which can be correlated with the Flanders stage. Towards their mouths the longitudinal profiles of the valleys on the solid rock, beneath the recent alluvium, have an abrupt change of slope indicating two distinct periods of erosion due to relative uplift of the land.

The lower of these two curves corresponds to the sea-level before the Flanders phase of sedimentation. The origin of the upper curve is not yet explained. Sediments belonging to the Flanders phase are represented by 20 metres of alluvial deposits near the river mouths, but these deposits decrease upstream to 12 metres, where the change of slope occurs in the valleys of the Seine and Loire. The present flood-plains of these rivers, which are extensions upstream of the coast plains, form a " modern terrace " graded to the present mean sea-level.

The terraces of the Garonne, Dordogne, and Adour have been studied by M. J. Blayac, who distinguishes two terraces in the lower courses of the rivers which correspond to terraces noted in the middle Garonne by Chaput :

(1) 50-metre terrace, which falls to 40 metres near Bordeaux.
(2) 15-metre terrace. *Elephas primigenius.*

Blayac thinks these two terraces converge towards the coast, but Chaput does not agree. A lower jaw of *Elephas*

meridionalis, var. Cromerensis, identical with forms from the "forest bed" of Cromer, and connected with the Sicilian by Depéret, has been found in some clays at sea-level at Gurp, near Soulac; but these clays have not been related to terrace sequence.

The terraces of the Nive, tributary of the Adour, have been studied by M. Passemard. These terraces are well preserved; the relative altitudes are:

(1) 142 metres.
(2) 80–95 metres.
(3) 40–57 metres.
(4) 26–34 metres.
(5) 15–17 metres, contemporary with the infilling of a rock-shelter containing Mousterian implements.

Chaput notes that these terraces have a steeper slope towards the sea than the present river, so that they correspond to sea-levels lower than the levels quoted, but owing to coast erosion no corresponding marine features can be traced.

The terraces of the Charente have not been studied since 1906; they may be summarised as follows:

(1) 47–60-metre terrace at Rochefort.
(2) 30–40-metre terrace, descending to 15 metres at Rochefort.
(3) 15-metre terrace, at Angoulême, falling towards the mouth. Mixed fauna (*E. antiquus* and *primigenius*) and Acheulian industry (at Tillaux).

This brief account of the river terraces of France indicates that their levels can be correlated with Depéret's marine stages, but there are certain points which suggest that the histories of all the rivers have not been the same. For instance, why is the Monastirian terrace of the Somme as low as 12 metres, though its fauna and implements are rather older than those of the 18–20-metre terrace of the Seine?

Somme	*Seine*
12-metre terrace.	16–18-metre terrace.
Mixed fauna.	Mixed, or cold fauna.
Late Chellean or Acheulian industry.	Acheulian—Mousterian industry.

Charente	*Nive*
Terrace of 15 metres or less.	Terrace of 15 metres or less.
Mixed fauna (*E. antiquus* and *primigenius*).	Mousterian industry.
Acheulian industry.	

If the histories of the rivers have been different the correlation of terraces and "beaches" on the basis of height alone is not a safe method.

A paper on the terraces of the middle Vistula was read at the meeting of the International Geographical Congress at Cambridge in 1928, by Prof. S. Lencewicz. His conclusions may be summarised as follows:

(1) 45–35-metre terrace, showing evidence of subsequent glaciation near Ploek. Correlated with 40–30-metre terrace of the French rivers.

(2) 22–15-metre terrace. Correlated with the 20–15-metre terrace of the French rivers.

The fauna of *Elephas antiquus, Rhinocerus merkii*, etc., has been found in these deposits, which are also correlated with the Würm glaciation.

(3) 5–10-metre terrace.	Ancylus.	Phases of the Baltic.
(4) low terrace.	Littorina.	

The correlation between the two older terraces and those of France is possible because until the Scandinavian ice left the Baltic the Vistula flowed westwards to the North Sea.

There are seven short reports on the Raised Beaches and River Terraces of the British Isles:

(1) *Evidences of Change of Relative Level between Land and Sea in Southern England since the Pliocene Period*, by Henry Dewey.

(2) *The Raised Beaches of the British Isles*, by W. B. Wright.

(3) *On the Climatic Equivalent of Raised Beach Mollusca*, by D. Baden-Powell.

(4) *Land Oscillations in England at the close of the Neolithic Depression*, by C. J. Gilbert.

(5) *Post-Pliocene Movements in the Vicinity of Cambridge*, by J. E. Marr.

(6) *Raised Beach Levels: Ayrshire Coast from Ardrossan to Girvan*, by V. A. Eyles.

(7) *A Raised Beach on the Outer Hebrides*, by J. W. Gregory.

The following short account of the terraces and raised beaches is based on these reports and other published material. In the London Basin at least such an account must begin in the Pliocene. The work of Dr. S. W. Wooldridge [1] has shown that there was an important marine transgression in the London Basin, which may be correlated with the Diestian of Belgium.

[1] "The Pliocene History of the London Basin," by S. W. Wooldridge, *Proc. Geol. Assoc.*, vol. xxxviii (1927), p. 49.

The deposits left by this transgression (Lenham beds) occur on a well-marked platform at about 550 feet (168 metres) in the Chiltern Hills and the North Downs. The platforms slope towards the centre of the basin, but there has been little or no warping since that time. There are indications that planation at the same height occurs in the Hampshire Basin. The Marine Pliocene is represented by the "Crags" of East Anglia, a series of shallow water sediments, laid down as the North Sea retreated after the Diestian transgression. The fauna shows an increasing number of arctic species, as the sea retreated. The last stage of this retreat is represented by the estuarine deposits of the Cromer Forest Bed.

In the main part of the London Basin the continental equivalents of the "crags" have not been clearly recognised, but between the Lenham beds and the proved glacial deposits there are two distinct platforms, at 400 feet (122 metres) and 200 feet (60 metres).

The 400-feet platform is well developed north of London, for instance, in the Tertiary plateau (often mentioned as the Tertiary Escarpment) between the Lea and the Colne, on the Epping Forest ridge and the Laindon Hills, and south of London in Chobham Ridges, and certain summit levels near London. The deposits connected with this platform are the "pebble gravels" of the Geological Survey. Dr. Wooldridge suggests [1] that the pebble gravels are fluviatile deposits only slightly younger than the Diestian Lenham beds. In both the Diestian and 400-foot platforms a slight gradient from west to east is discernible.[2] Dewey, in his report, notes the widespread occurrence of a 400-foot (122 metres) platform in Devon and Cornwall, Wales, and parts of Scotland and the Midlands. Clement Reid [3] attributed the platform in Cornwall to marine erosion on the evidence of the fossiliferous St. Erth beds, which yield an early Pliocene fauna. The fact noted by Dewey that glacial deposits are found on the platform here and in Wales, and in the London Basin, only proves that the feature is preglacial, not that it is glacial.

The 200-foot (60-metre) platform is well developed in the east of the London Basin—East Kent [4] and Essex.[5] In Essex the plateau formed by this feature is covered by the thick

[1] "The Pliocene History of the London Basin," by S. W. Wooldridge, *Proc. Geol. Assoc.*, vol. xxxviii, (1927), p. 49.

[2] *Ibid.*

[3] "The Geology of Land's End," *Mem. Geol. Surv.*

[4] "The Geology of the Canterbury District," by H. Dewey, S. W. Wooldridge, H. W. Cornes, and E. E. S. Brown, *Proc. Geol. Assoc.*, 1925.

[5] "The Pliocene Period in Western Essex and the Pre-Glacial Topography of the District," by S. W. Wooldridge, *Essex Naturalist*, vol. xxi (1927), p. 247.

deposit of Chalky Boulder Clay. The existence of such a plateau has been proved by boring records over a wide area.[1] The surface appears to slope towards the north and east; whether this slope is "natural" or caused by subsequent warping is difficult to distinguish from the borings alone. Also the relation between this feature and "Crags" of East Anglia, which are little above sea-level, has not been determined. There is, however, clear evidence that the 200-foot (60-metre) plateau was complete before the invasion of the Chalky Boulder Clay ice, indeed the valleys of the larger rivers were already carved below its surface. Besides these valleys there were important "subglacial depressions," which were probably over-deepened by the ice.

In East Kent the 200-foot platform is well developed in connection with the Stour drainage system, and has been traced farther west in the "gaps" by which the Wealden rivers traverse the chalk escarpment of the North Downs. But it should be noted that the existence of this 200-foot (60-metre) platform at 200 feet O.D. in the western part of the London Basin is not possible, but features of the same age may be looked for 200 feet above the present valleys.

Dewey, in his report, refers to plains at 200 and 300 feet O.D. (91 and 60 metres), which are structural rock benches[2]; his remarks can only be applied to features occurring in the Aldershot district. There are indications that marine platforms occur at this height in Cornwall and Devon.

With regard to the age of this platform, it is undoubtedly older than the Chalky Boulder Clay, and had been considerably dissected prior to the advance of the ice. Dr. Wooldridge himself suggests a tentative correlation with Depéret's 60-metre Milazzian beach, but concludes only that the feature is late Pliocene or early Pleistocene, and prefers to regard it as the last phase of the Pliocene.

In his report W. B. Wright mentions the 10-foot (3-metre) beach, which is found in S.W. England, Wales, and Ireland, and N.E. England. Its level is very constant, though it appears to drop towards the north, and there is clear evidence that it is preglacial, since boulder clay is found on top of the scree, above the beach deposits. In Yorkshire the scree descends below present sea-level. It is curious that this stage, which Wright considers represents a long

[1] "The Pliocene Period in Western Essex and the Pre-Glacial Topography of the District," by S. W. Wooldridge, *Essex Naturalist*, vol. xxi (1927), p. 247.

[2] SCIENCE PROGRESS, vol. xx, no. 94, Oct. 1929, "River Terraces and Raised Beaches," Part I.

halt at a constant level, cannot be traced in any of the fluviatile features, but it is probable that such evidence would be destroyed by subsequent events.

The platforms described above are important facts which will have to be fitted into any scheme to correlate the British and Continental Pliocene and Pleistocene histories. The correspondence between the Diestian (168-metre) and 400-foot (122-metre) plains and higher peneplains of Northern France may be significant. On the other hand, there is no feature which can be linked up with Depéret's Sicilian stage (100–90 metres). But Depéret correlates the Cromer Forest bed with the pre-Sicilian regression of the sea, and the Yoldia Myalis shales with the end of the transgression.[1] The corresponding shore-line has not been identified anywhere in the British Isles, if it is to be found at 328 feet (100 metres).

In this article only the river terraces and raised beaches of the British Pleistocene will be considered in detail; reference will only be made to the actual Boulder Clays, etc., when they have a bearing on the age of the terraces.

The Pleistocene history of South-eastern England differs in many important respects from that of Northern England and Scotland. The latter were glaciated by local ice—from adjacent highlands—while South-eastern England north of the Thames formed what might be termed the "Pedmont" region, glaciated by the "snout" of the Eastern ice. South of the Thames, though there was no large mass of ice, there were arctic climate conditions, which were reflected in the weathering of the landscape.

Much has been written, and many contradictory theories presented, concerning the raised beaches of the British Isles, especially those of Scotland.

W. B. Wright gives a short but clear account of these beaches. The 100-foot (30-metre) beach of Scotland is due to a late glacial submergence of the land centring round the Highlands of Scotland. The upper limit of the beach may have a steep gradient outwards, similar to certain of the Baltic beaches. Wright does not consider that there is sufficient evidence at present to distinguish a 50-foot beach as a separate stage from the 100-foot beach; indeed, he uses the term "100-foot" simply to indicate the general maximum height of the marine deposits. These deposits are generally unfossiliferous coarse shingle banks, though marine clays have yielded an arctic fauna. There is very little evidence of shore erosion or cliff cutting, indicating that the sea was not long at one level.

[1] "Old and New Standards of Pleistocene Division," by H. F. Osborn and C. A. Reeds, *ibid.*

The only other well-marked raised beach is the Early Neolithic, or 25-foot (7-metre) beach, which can be traced in Scotland, Northern England and North-eastern Ireland. The term "25-foot beach" is misleading, since there is no constant level; the beach attains a height of 75 feet at the head of Loch Linnhe, but declines fairly rapidly to sea-level east of the island of Lewis, and less rapidly to the east and south-east, north-north-east, and south-south-west, having an elevation of 5 feet (1½ metres) at Caithness, Donegal, and Dublin.

The age of the beach has been proved by the discovery of implements, and the marine shells indicate a postglacial climatic optimum. The Early Neolithic beach is separated from the older beaches by a period of emergence (regression of the sea), evidence of which is preserved in the widespread occurrence of submerged forests and peat beds. The post-Neolithic emergence appears to have been almost continuous down to the present day, though there are indications of smaller oscillations, such as the upper submerged forests of Lancashire and Cheshire.

W. B. Wright's interpretation of these northern beaches leaves no room for any eustatic theory as the cause of the changes of level, and his results are comparable with those obtained from the study of the Baltic shore-lines.

In South-eastern England the Terraces of the Thames form the most considerable body of evidence to be discussed. They may be summarised as follows:

Alluvium.—The present flood-plain of the Thames.

Lower Flood-plain Terrace.—Often buried under the alluvium.

Buried Channel.

Upper Flood-plain Terrace.—10–20 feet above the Thames.

Taplow Terrace.—50–30 feet above the Thames.

Boyn Hill Terrace.—Furze Platt Stage, 15 feet below the Boyn Hill Stage. Boyn Hill Stage, 90–70 feet above the Thames.

Winter Hill Terrace.—150–100 feet above the Thames.

Glacial Gravels.—Gravels lying between 200 and 300 feet above sea-level, yielding Bunter Quartzite and other "foreign" pebbles.

The glacial gravels present a problem to which a satisfactory solution has yet to be found. They range in height from nearly 400 feet to below 200 feet (122–60 metres), but may be easily distinguished from the gravels of the 400-foot platform by their composition. The latter are mainly derived from the Eocene beds with an admixture of older material from the Weald in certain areas; but the glacial gravels contain a

varying proportion of quartzites and grits and limestones derived from the Permian, Carboniferous, and Jurassic of the Midlands. The question of how this material was introduced is a problem in itself, but the "foreign" pebbles appear to have entered the London Basin through the gaps in the Chiltern escarpment at Goring and Stevenage. The term "glacial" is a misnomer,[1] as some of the bunter-bearing gravels are probably preglacial. These gravels form a broad belt across the Chiltern dipslope from Goring to Hertford; from Goring to Uxbridge the lower spreads (250–200 above sea-level) form a distinct terrace of the Thames (the Winter Hill Terrace), and yield a few palæoliths,[2] but the higher gravels (above 300 feet), though they simulate terrace form, have no longitudinal gradient, and continue at the same height to Hertford.

Between Rickmansworth and Hertford, in the Colne and Lea valleys (the Vale of St. Albans), the lower spreads (200–250 feet above sea-level) are definitely associated with the Chalky Boulder Clay, occurring above and below it, and evidently representing the outwash during both the advance and retreat of the ice. Similarly, the gravels which are found at 200 feet above sea-level in the Brent Valley represent outwash from the ice which left Boulder Clay at Finchley. The problems of these gravels in the Vale of St. Albans have yet to be solved, but it is suggested that the upper and lower spreads are distinct.

The Winter Hill Terrace[3] is well developed between Reading and Uxbridge; there is a deserted meander at this level (150 feet above the river, 250 feet above sea-level) north of the Thames between Reading and Henley. Palæoliths have been found in these gravels,[4] near Henley. The gravels on the plateaux of Richmond, Kingston, and Putney Heath, at about 190 feet above sea-level, though hitherto classed as Glacial, belong to this spread. Eastwards from London, and in the Lea Valley, it has not yet been possible to distinguish this terrace from the glacial gravels and the Boyn Hill Terrace gravels. This terrace can be but little later than the Chalky Boulder Clay, and may have been formed by the Thames while the ice was still in East Anglia. In the Lea Valley gravels at this height above the river are clearly glacial outwash, but in the west a well-marked slope of some 50 feet separates the Winter Hill Terrace from the older gravels.

The Boyn Hill Terrace is again separated by a distinct slope —often driftless—of 50 feet from the Winter Hill Terrace, where they occur together near Maidenhead. The Boyn Hill

[1] "River Development in Essex," by B. R. M. Saner and S. W. Wooldridge, *Essex Naturalist*, vol. xxii, 1929.

[2] *Ibid.* [3] *Ibid.*

[4] *Memoirs of the Geological Survey*, "Henley."

Terrace is about 90 feet (27 metres) above the river-level, and 15 feet below is a lower terrace—the Furze-Platt stage; these two stages are separated by a step of " solid " rock, and can be traced near Maidenhead and at Dartford Heath. The fauna of the Boyn Hill Terrace is a " warm " type, and the sequence of flint implements was clearly demonstrated at Swanscombe, Kent, where early Chellean types occurred low in the gravels, and were succeeded by late Chellean and early Acheulian types. At Dartford Heath this gravel is 50 feet thick, having its base at 88 feet above sea-level, and contains many " foreign " stones, including the Rhaxella chert (Corallian) derived from the Boulder Clay. This terrace is generally accepted as definitely postglacial, that is, later than the Chalky Boulder Clay.[1]

Another period of erosion followed, and the river-bed was lowered about 50 feet before deposition was renewed and the Taplow gravels laid down. These gravels yield late Acheulian and Mousterian implements, and a cooler type of mammalian fauna than the Boyn Hill (*Elephas primigenius* and *Cervus elephas*). It is generally thought that the climate became colder during this period, and that this cooling culminated during the following period. There is a certain amount of evidence indicating a phase of erosion with deposition at the end of Taplow time.[2]

The surface of the Taplow gravel shows certain features which were probably due to " soil creep " under arctic conditions, and are generally regarded as additional evidence in favour of the following cold period. The " brick earth," or loam usually found above these gravels, and often banked against the slope of the higher terrace, is considered by some to be ancient alluvium, by others as loess; its age is uncertain.

During the following period of erosion the river cut down about 30 feet to the base of the upper Flood-plain Terrace, which appears to be a bench in the London Clay at about the present river-level. Here Mousterian implement factories have been found near Crayford. Deposition followed and 15–20 feet of gravel were laid down, forming the upper Flood-plain Terrace. The next phase seems to have been one of severe—arctic—climate and erosion. Probably the " Combe " deposits—accumulations of various rock debris in valley bottoms—which buried the implement factories at Swanscombe belong to this period. The arctic beds found near Charing Cross and at Ponder's End in the Lea Valley are the most important evidence in favour of a phase of very cold climate at this time. The arctic beds are clays yielding arctic

[1] " The Geology of the London District," *Memoirs of the Geological Survey*.
[2] "On Some Features of the Taplow Terrace between Charing Cross and the Fleet Valley," by B. R. M. Saner, *Proc. Geol. Assoc.*, vol. xl, 1929.

plant remains, specially *Betula nana*. In both cases these beds occur about 30 feet below the level of the present flood-plains; at Ponder's End the bed appears to belong to the late Palæolithic.[1] At Charing Cross this bed occurs at the base of the Lower Flood-plain gravel at about 25 feet below sea-level. The relation between this gravel and the Buried Channel is not clear. At Westminster the boring records indicate that the base of the gravel is generally at this level (–25 feet), where it forms a considerable terrace below the alluvium. The Buried Channel is a deep trench cut down to 100 feet below sea-level (–30 metres), which has been filled up with deposits dating from the early Neolithic[2] gravels to the present alluvium. The later gravels and the alluvium certainly occur at the top of the Lower Flood-plain Terrace, but the lower gravels on this terrace are probably older than the Buried Channel. The fact that gravel can be traced continuously from the Taplow terrace to the bottom of the Buried Channel at Crayford,[3] probably indicates a slope which is really a polygenetic terrace.

The alluvium of the present flood-plain clays and silts, with peat and sand bands, is generally only 10 feet deep near London, but increases to 30 feet at Erith and 50 feet at Tilbury, which indicates that the phase of sedimentation which began in early Neolithic times is still in progress.

Raised beaches equivalent to the terraces of the Thames and its tributaries are not conspicuously well developed, but the 100-foot raised beach north of Portsmouth,[4] with a warm fauna and Acheulian implements, may be correlated with the Boyn Hill, whilst the 15-and 50-foot beaches of the same district, with a cold fauna, many foreign stones and Mousterian implements, are probably the equivalents of the Taplow and Upper Flood-plain Terraces. In the same area there is evidence of a sea-level 70 feet below the present level, which may correspond to the Buried Channel.[5] Certain important facts are indicated by this brief survey of the River Terraces and Raised Beaches in South-eastern England. The major glaciation represented by the Chalky Boulder Clay and the associated outwash gravels were spread out shortly after the completion of the 200-foot (60-metre) platform, and before it was maturely dissected. The river terraces of the Thames and its tributaries are younger than the Boulder Clay, and represent resorted outwash material. The first three terraces (Winter Hill, Boyn Hill, and Taplow) are simple " cut and built " terraces, due to

[1] " The Geology of the London District," *Memoirs of the Geological Survey*.
[2] *Ibid.* [3] *Ibid.*
[4] " The Pleistocene Deposits of the Portsmouth District," by L. S. Palmer and J. H. Cooke, *Proc. Geol. Assoc.*, vol. xxxiv, 1923. [5] *Ibid.*

periods of erosion followed by periods of deposition, but the Flood-plain Terraces are not so easy to interpret correctly. The late Palæolithic phase of erosion (or emergence) represented by the Buried Channel of the Thames has been followed by continuous deposition and submergence with emergence in contrast with the evidence of the early Neolithic beach of Scotland.

Considering all the evidence presented in the " Report on Pliocene and Pleistocene " is the adoption of any " base-level " as a *constant level* justified? The late Palæolithic phase of Marine Regression or Uplift of the Continent, represented by the Flanders stage, the Buried Channels of the Thames, the Seine and the Nile, which has been followed by a phase of Marine Transgression and continued deposition—or sinking of the Continent—is a significant fact. On the other hand, this stage is not represented in the Mediterranean Basin (except in Egypt), and does not occur in Scotland and Scandinavia. In the north, and the Mediterranean coasts of Italy and Greece, the study of the Raised Beaches indicates that there have certainly been movements of the land during Pleistocene and Recent time. In the Mediterranean such movements suggest tilting of the land blocks. If we accept 30 metres, the base-level of the late Palæolithic marine regression as a constant level, and suggest that Eustatic movements of the sea caused the changes of level, we also have to admit that there are certain areas where there have certainly been movements of land and the constant level cannot be traced. Similarly, the Pliocene platforms of England and France, as well as Spain and Egypt, are found at comparable levels, but in the other Mediterranean countries the Pliocene platforms are 850 metres higher. Thus even if the Eustatic theory were adopted, Tectonic movement would have to be admitted, and, once admitted, would have to be considered as a possible factor in every case brought forward.

Depéret's system of classification has much to recommend it, but the correlation of terrace features cannot be based on relative altitude alone, since differential land movement is a possible factor.

Pliocene and Pleistocene chronology is still far from straightforward, but correlation between the different kinds of evidence, presented by the different studies, such as Archæology and Geomorphology, is becoming more reliable,[1] and the Terrace Commission has done important work in presenting the evidence obtained from detailed work in the various countries.

[1] "Old and New Standards of Pleistocene Division in Relation to the Prehistory of Man in Europe," by H. F. Osborn and C. A. Reeds, *Bulletin of the Geological Society of America*, vol. 33, pp. 411–90, July 1922.

POPULAR SCIENCE

SCIENCE AND COSMETICS

By H. STANLEY REDGROVE, B.Sc., A.I.C.

Amongst the objects found in the tomb of Queen Shubad excavated at Ur, and which is estimated to date back nearly six thousand years, were a number of receptacles, shaped like shells, which contained cosmetics for the queen's use in the world of spirits. I do not know whether the contents of any of these have been analysed, but the green colour in one case suggests that the shell may have originally contained a preparation of copper oxide for blackening the eyelashes. Whether this supposition is accurate or not is, however, immaterial to the point I wish to emphasise, which is the extreme antiquity of the use of cosmetics. From the very dawn of human history, it would seem, mankind, or rather womankind, has made use of artificial, or, perhaps I should say, chemical, means for the enhancement of beauty.

In tracing the history of the subject, we find the use of cosmetics most in evidence during periods of luxury, and one is tempted to say that, if the history of cosmetics is not identical with the history of human culture, their evolution has at any rate followed along strictly parallel lines.

At the present moment, the consumption of cosmetics has reached unprecedented proportions in Western Europe and the United States of America—in other words, the most highly civilised parts of the world. According to a reliable authority, American women spent, during 1927, nearly five million dollars a day on cosmetics. The *per capita* expenditure is probably higher in the United States than in any other country; nevertheless, enormous sums are expended every year on cosmetics in England, France, Germany, and Austria.

There are, I think, two main reasons why cosmetics are so much more widely employed to-day than was the case, say, a generation or two ago. In the first place, there has been a marked change in public opinion concerning the propriety of using them, at any rate in England and the United States. The old "Victorian" idea that the use of cosmetics is somehow

immoral—that no "respectable" woman could possibly dye her hair or apply rouge to her cheeks—is rapidly dying if it is not already completely dead. In the second place—and this factor has probably played a part in bringing about the change just referred to—dermatology and chemical science between them have brought about great improvements in recent days, resulting not only in a greatly increased range of cosmetics, but in cosmetics of a character far superior, both from the point of view of hygiene and that of æsthetics, to those obtainable in the past.

I shall have many things to say concerning these advancements in the course of this paper, but at this point a summary statement of the present position may not be out of place. On the one hand, then, let it be said that the subject is, to-day, by no means free from a good deal of humbug and chicanery; that all modern cosmetics are by no means perfectly free from noxious ingredients; and that quite a number of women know so little how to use cosmetics correctly that they too frequently render themselves hideous rather than beautiful by their aid. Nevertheless, we are far from the days when finely ground chalk from the Dover cliffs, which must have made women look like clowns, was thought to be an ideal face-powder; when such a highly dangerous material as vermilion was the main substance used for tinting the cheeks red; and when, speaking generally, one might say that most cosmetics were either definitely noxious or devoid of æsthetic value. If, in certain directions, further research is urgently necessary, it is true to say that modern chemical science does offer, even if her gifts are not always accepted and utilised, a large range of innocuous cosmetics in harmony with our present stage of culture and æsthetic sensibility, and which, in some cases at any rate, are definitely beneficial from the medical point of view.

Whilst there is general agreement as to what constitutes a cosmetic, some divergence of opinion may arise when one comes down to details. A cosmetic is usually defined as a means for improving beauty, especially a preparation for beautifying the skin and hair. One might argue that, as good health is the best means to beauty, every preparation in the pharmacopœia is a cosmetical one, for all of these are designed as means to good health. Such an attempt to stretch the meaning of the word, however, would not merely serve no useful purpose but be quite absurd. On the other hand, it seems quite reasonable to class as cosmetics those medicaments containing drugs of relatively little potency which may be employed to cure slight blemishes of the skin, and also those which exercise a prophylactic action, serving to preserve the complexion against the inclemencies of the weather. It might

be said that these preparations belong rather to medicine than to cosmetics. But the suggested classification serves a useful purpose. Faced with so many complex problems of an urgent nature, medical practitioners and those engaged in medical research can hardly be expected to pay much attention to minor blemishes of the skin which appear in no way to be symptomatic of more deeply-seated ills.

Amongst preparations of the first type referred to above may be included a large array of face-creams containing such ingredients as glycerol, zinc oxide, benzoin, hamamelis, etc., whose therapeutic properties are well known, as well as a multitude of other simple toilet preparations. We might include face-powder, when this contains zinc oxide, a beneficial astringent used in small amounts. We might also include brilliantine and other oily preparations for the hair, the researches of Dr. Leftwich [1] having shown that oils increase the hair's tensile strength.

As concerns prophylactic preparations, one of the best examples is provided by so-called "vanishing" cream, a soapy emulsion of stearic acid, the discovery of which we owe to modern chemistry. A cream of this type, used sensibly in combination with a moderate application of face-powder, does very emphatically serve to preserve the beauty of the skin.

To continue with the question of the classification of cosmetics: we might, with some considerable justification, include under the term all toilet preparations used for cleansing purposes, such as soaps, shampoo preparations, and dentifrices. Soaps and dentifrices, however, seem to form distinct classes by themselves, which, if considered at all, would call for more detailed discussion than is possible in this paper and would take me too far from my original intention. I pass over these, therefore, contenting myself by drawing attention to the enormous advances which have been made in the manufacture of soaps and dentifrices due largely to modern chemistry.

One cleansing preparation, however, stands by itself, and is invariably regarded as a cosmetic. This is cold cream, the discovery of which we owe to the noted Roman physician, Claudius Galenus. This cream was once prepared from expressed almond-oil, beeswax, spermaceti and rose-water, the formation of the requisite emulsion being a most tedious process. Modern chemistry has changed all this. The discovery of emulsifying agents, borax being the one usually employed in making cold cream, though lanoline is also sometimes used, has made its preparation so simple that a child almost could carry it out. Moreover, the substitution of liquid paraffin for the almond-oil, and the omission of the spermaceti, has resulted in a cream much cheaper to prepare and possessing the additional advan-

tage of never going rancid, a source of much trouble with cold creams of the older type.

To continue: we might also not only say that the perfume is an important ingredient in practically every cosmetic, but claim as cosmetics all perfumes used for rendering the person more attractive. This again, however, would be an unjustifiable extension of the term; and whilst, so to speak, cosmetics and perfumery overlap, it is as well to draw a distinction between the two so far as may be possible.

Having cleared the ground, we are now in a position to consider the very important class of cosmetics—the class concerning which so may diverse opinions have been expressed—consisting of preparations employed to cover up blemishes and to give the hair or skin, in appearance at any rate, some factor which it lacks, especially hair-dyes and the cosmetics used for colouring the skin, for which there is no comprehensive term in English, called " fards " in French.

Let it be at once admitted that there is nothing like buoyant good health to bring roses into a woman's cheeks. Let it be admitted that any attempt to change in too great a degree the colour of the lips, cheeks, eyebrows, or hair may produce a disastrous disharmony in the colour scheme as a whole. Nevertheless, the fact remains that even artificial beauty is preferable to ugliness, provided the artifice is good. Here, after all, is the crux of the question, and it is here, I suggest, that modern chemistry has signally triumphed. Moreover, there is a psychological factor to be taken into account: if a touch of rouge or a lip-stick gives a woman the consciousness of presenting a better appearance to the world, it may exercise a very real and valuable medicinal effect.

Moreover, it must be borne in mind that much of our lives is spent in artificial light, and that what appears natural and beautiful in sunlight may seem very different in a room brilliantly lit by electricity. Those who deride women for using mauve or pale green face-powder for evening use merely betray their ignorance of the laws of optics.

In this connection, of course, the question of cosmetics for theatrical use arises, " make-up " on the stage being absolutely necessary if a natural appearance is to be presented to the audience. The cinema-stage also demands its artists to be made-up; and, here it may be noted, recent developments in the cinema industry have had some interesting consequences. In the early days, when films of the ordinary type were employed, make-up in seemingly grotesque colours, in which bright yellow and green predominated, was found to be quite necessary to obtain satisfactory results, owing to the marked differences in sensitiveness of the camera and the human eye

towards the colours of the visible spectrum. The use of panchromatic films and improved lighting conditions have wrought a marked change in the type of make-up required, this now following on much more " natural " lines, with *brown* as the predominating colour. The advent of the " talkies " has had a further effect, the necessary increase in the rate at which the scenes are " shot " necessitating the use of somewhat darker colours.

It will be realised that the preparation of cosmetics for colouring the skin, whether for ordinary, theatrical, or cinema use, involves far more scientific knowledge than the man in the street is wont to credit. The problems to be solved are, in the first place, problems for the chemist and the physicist. The correct colours must be determined and dyes or pigments which will give these colours discovered. But what may be called the physical and chemical solution must always be subject to the approval of the dermatologist : a question of prime importance is, Are the selected ingredients entirely free from any deleterious action on the skin ? It is here, undoubtedly, that more research is necessary, for there is, perhaps, too ready a tendency to accept a colour as satisfactory on æsthetic grounds alone without adequate assurance of its entire harmlessness.

In connection with the question of harmfulness and harmlessness, the subject of hair-dyes calls for particular consideration. Hair-dyes really form a quite distinct class of cosmetics and the art of dyeing the hair has quite peculiar problems of its own to solve. In a paper published some little time ago in *The Chemist and Druggist,* [2] I gave a brief résumé of our present knowledge concerning the composition of preparations used for dyeing the hair, and, in collaboration with Mr. G. A. Foan, I have dealt with the subject exhaustively in *Blonde or Brunette? A Complete Account of the Theory and Practice of Hair-dyeing in all its Branches* [3]. It seems necessary, however, very briefly to review the subject here.

Modern hair-dyes may, from the chemical point of view, be roughly grouped in five classes : (i) Dyes which deposit silver sulphide or a mixture of the sulphides of silver and other metals, or in which metallic silver is produced by the interaction of pyrogallol and a silver salt ; (ii) dyes of the rastick type which make use of the interaction of pyrogallol and compounds of such metals as copper, cobalt, and nickel, in the presence of an oxidising agent (the atmosphere), complex organo-metallic pigments of unknown constitution being formed, often used in conjunction with silver ; (iii) henna, which owes its efficacy to a hydroxy-naphthaquinone, christened " lawsone," which appears to combine with the keratine, usually employed in combination with dyes of the second type mentioned above ;

(iv) dyes containing para-phenylene-diamine, which deposits a pigment, "Bandrowsky's base," on oxidation (hydrogen peroxide being usually employed), and allied compounds; (v) lead dyes, usually called "hair-restorers."

Of these dyes, those of the para-phenylene-diamine type undoubtedly give the best results from the artistic point of view; but there is equally no doubt that para-phenylene-diamine is an extremely dangerous substance, presumably owing to the formation of quinone-di-imine during its oxidation, and has been the cause of some tragic accidents. On the other hand, whilst pyrogallol is certainly poisonous, dyes containing it would seem to be safe, provided due care is exercised in using them, and, especially in the case of those containing cobalt, some very good shades indeed can be produced. Henna, it may be added, is quite harmless.

A disadvantage attaching to the use of dyes containing metals is that hair dyed by their aid can only be "permanently" waved, if at all, with great difficulty. It would seem that here chemical science has not achieved all that might be expected of it, though certainly advances have been made. I once called hairdyeing the Cinderella of applied chemistry, and the designation seems just. Much more research is necessary before a hair-dye is produced which can really be labelled ideal.

Several years ago Dr. Rabuteau carried out some interesting experiments in which persons were immersed in a bath containing a considerable quantity of belladonna without any dilation of the pupils taking place. We must not, of course, infer from this result that any toxic substance can safely be applied in aqueous solution to the skin or hair provided the skin is unbroken and the substance is not of a caustic character, for much depends on the type of solution it forms. When, of course, the toxic substance is compounded with a greasy medium, the danger of absorption is much greater. The experiments, however, do illustrate the fact that the physiological results of taking a drug internally and applying it externally are not necessarily the same. It may be possible to make external application without harm of a substance which, taken internally, would have serious physiological consequences. Alternately, the external application of a substance, harmless if taken internally, may have serious results.

This latter possibility is not, I think, always envisaged. An interesting case in point is supplied by bismuth. Whilst the internal administration of basic salts of bismuth seems to be practically free from danger, cases of poisoning have occurred which have been attributed to the external absorbtion of bismuth salts; and, although the cause has been disputed, it is wise to err, if at all, on the side of safety. The

use of compounds of bismuth in cosmetics, therefore, is not to be commended.

The history of face-powder and the improvements made, not only in the method of its manufacture, but in its composition, thanks to modern science, is an interesting one. The time is not so far past when women thought their faces were improved by the application of a thick coating of an opaque, white powder. No wonder face-powder had innumerable critics amongst people of sense and æsthetic judgment. Nowadays face-powder of a more transparent type is preferred, a light coating of which will give a peachlike bloom to the complexion without hiding it. Moreover, modern face-powders are almost invariably tinted.

The demand for more or less transparent face-powders has led to the increasing employment of clay in their manufacture, the best form of this being osmo-kaolin, a very fine china-clay purified by electrolytic means. To this is added talc, to give the powder "slip"; magnesium carbonate, to reduce the density of the powder; magnesium stearate, a modern chemical discovery, to increase its adhesiveness; and zinc oxide, to whiten it and increase its opacity a little. Starch is also another very frequent ingredient; in some powders, the main ingredient. As Mr. C. Doubleday has recently pointed out in the columns of *The Chemist and Druggist*, [4] starch suffers from the objection that it is liable to get into the pores and there to ferment; but the objection does not hold if the face is first treated with "vanishing" cream, and cleansed every night with cold cream, which is the modern method, in theory at any rate, if it is not, as Mr. Doubleday thinks, always put into effect. When starch is used, this, in theory, is always rice starch, preferable on account of the fineness of its granules. In practice, maize starch, whose granules are considerably larger than those of rice starch, is very often employed.

Of course, for theatrical purposes, a very opaque white powder, or a heavy white grease-paint of good covering power is sometimes required, as, for example, in the *maquillage* of the classical clown of the circus. Bismuth sub-chloride is often used for this purpose. Titanium dioxide would seem to offer itself as a preferable material. This substance, not so many years ago an expensive scientific curiosity, is now a common article of commerce. When pure it is quite innocuous and very inert, and possesses quite remarkable opacity and covering powers. Its possibilities as a cosmetical pigment do not seem to have been at all fully explored [5].

Face-powders may be tinted by incorporating with them suitable quantities of an insoluble pigment, for which purpose some of the iron-containing earths, such as the ochres, sienna,

and Armenian bole, are especially useful; or, owing to their absorbent character, they can be stained by means of a solution of a water-soluble dye. The large range of shades now obtainable is due to the increasing employment of coal-tar dyes, amongst which the halogen-substituted fluorescein dyes, such as eosin, erythrosine, phloxine, and rose bengale, and certain of the azo-dyes, may be mentioned as particularly suitable for obtaining shades of rose, pink, and *naturelle*. Every year sees an increasing number of new coal-tar dyes placed on the market, of which, no doubt, many are suitable for cosmetical purposes. Unfortunately our knowledge of the physiological action of dyes does not keep pace with the increase in their number, and here more research is urgently necessary. It would be extremely valuable if, against every dye included in the *Colour Index*, a note could be added giving its physiological action or stating "physiologically inert." Unfortunately, in too many cases, present knowledge on the subject could only be represented by a question-mark; though the particular dyes mentioned above would seem to be quite innocuous when used in reasonably small amounts.

In the case of colouring-cosmetics with a greasy basis, dyes can, in many cases, only be successfully employed in the form of lakes, and here the problem of choosing materials satisfactory from both the dermatological and æsthetic standpoints becomes rather more difficult, owing to the fact that so many lakes contain toxic metals. In numerous cases barium sulphate is employed as a basis on which to deposit lake-colours, and whilst when *absolutely* pure this is innocuous owing to its entire insolubility, the necessary degree of purity can hardly be expected of the commercial article. Fortunately, there are some suitable lakes available, and these, with the iron- and manganese-containing earths, which are extremely valuable cosmetical pigments, and some few other innocuous pigments such as lampblack, ultramarine, etc., enable an enormous range of shades and colours to be obtained.

A word concerning ultramarine seems desirable, for the synthetic preparation of this pigment was one of the earliest triumphs of colour chemistry. At one time extracted from *lapis lazuli*, ultramarine cost several pounds an ounce. To-day, thanks to chemical science, this extremely useful pigment, made synthetically, can be purchased for a shilling or two per pound.

Before leaving the question of colouring matters, I want to emphasise the fact that, although chemical science makes possible the preparation of cosmetics to colour the skin any desired shade from perfectly innocuous ingredients, such ingredients are not always employed. Some little time ago, for

example, a firm specialising in colouring matters for cosmetical purposes offered me a range of pigments described as in every way suitable and quite harmless. One was zinc chromate !

Turning our attention from the consideration of cosmetical colouring matters to that of cosmetics as a whole, it must be confessed that, in spite of great advances and improvements and of all that might be achieved if full use was made of present-day scientific knowledge, the subject seems still to be in what might be called the "patent medicine" stage. There is too much secrecy, which is both unnecessary and unhealthy. One can quite understand a manufacturer of a fine perfume keeping its composition a close secret, for often it will owe its particular individual character to minute traces of synthetic materials, which cannot be detected by analytical methods. Too often the protection afforded by a patent has been found to be illusory. But similar remarks do not by any means apply to, for example, a hair-dye, or in the case of numerous other cosmetical preparations. Any trade rival can have these analysed and successfully imitate them if he so desires. The secrecy concerning their composition is a secrecy directed solely against the public, as a writer in a trade journal recently pointed out, and is too frequently adopted as a means to support untenable claims.

Any manufacturer who had the courage to place on the market a series of cosmetical preparations giving the formula on each pot or bottle would render cosmetics a signal service. It may be added that a similar policy adopted in the case of medicaments has proved highly successful commercially.

The position of cosmetics would also be much improved if a cosmetical formulary, having an authority equal to that of the *British Pharmacopœia*, or, at any rate, that of the *Codex*, could be published. Incidentally, it may be mentioned that both these volumes contain formulæ for the preparation of a few cosmetics, *e.g.* cold cream ; and the fact that they are few in number may be taken as proof of what I have already intimated, namely that medical science, as a whole, cannot be expected to concern itself very seriously with cosmetics. Meantime, we have for example, in French, Cerbelaud's monumental *Formulaire*, in which recipes are given for making a large number of preparations analogous to proprietary cosmetics. An English work, somewhat on the lines of *Secret Remedies* [6], and having like authority, containing analyses of the most widely used and advertised proprietary cosmetics, would be very welcome and would help to clear the air [7].

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2. "The Chemistry of Hair-Dyes," *The Chemist and Druggist*, vol. 109, pp. 760–2 (December 22, 1928).
3. William Heinemann (Medical Books), Ltd., 1929.
4. C. Doubleday, "Colours for Face-powders," *The Chemist and Druggist*, vol. 111, p. 372 (September 29, 1929).
5. These have recently been called attention to in the trade press by the present writer. See "Titanium Dioxide and its Cosmetical Uses," *The Perfumery and Essential Oil Record*, October 22, 1929.
6. Published in two volumes by the British Medical Association in 1909 and 1912. Vol. 2 contains analyses of some proprietary hair-dyes.
7. Lest I be thought to be ignoring it, I should perhaps point out that Mr. Poucher's very valuable work on *Perfumes, Cosmetics, and Soaps*, which contains so much scientific and practical information, is cast on different lines from the book suggested and serves, of course, a somewhat different purpose.

ESSAYS

THE CHEMISTRY OF DVI-MANGANESE ELEMENT OF ATOMIC NUMBER 75. [By J. G. F. Druce, M.Sc. (Lond.), R.Nat.Dr. (Prague), F.I.C.]

THE discovery of two new elements of atomic numbers 43 and 75 was announced in 1925 by W. Noddack and I. Tacke [1], who claimed to have detected them in platinum ores and in certain columbite minerals.

The presence of any unknown elements in platinum ores in appreciable amounts was subsequently denied by O. Zvjaginstsev [11], and Prof. Prandtl [12] cast doubt upon the presence of these new elements in columbites. Nevertheless, I. and W. Noddack have since been able to obtain enough material containing the element of atomic number 75 (which they have called Rhenium whilst assigning also the name Masurium to the element of atomic number 43 [1]) to establish its existence.

The occurrence of dvi-manganese in crude manganese compounds was discovered independently by Loring and Druce [2] and by Dolejšek and Heyrovsky [3]. Since the amount of the new element in commercial manganese compounds is small (it is estimated to be about one part in 20,000), its isolation from this source has proved most tedious, but sufficient evidence has accumulated to prove beyond dispute the existence of this element. Furthermore, the German workers (I. and W. Noddack) have found more promising sources, namely, molybdenum glance, alvite, and gadolinite, from which they have been able to extract the element in sufficient quantity to investigate its physical and chemical properties.

METHODS OF EXTRACTION

There is some reason to suppose that the sulphide of dvi-manganese would be soluble in ammonium sulphide, like that of chromium. Thus the earlier attempts at separating the new element from manganese compounds consisted in removing manganese as sulphide from alkaline solutions,

[1] Mendělejev's name, dvi-manganese, for the element is, however, retained in the present communication.

and then examining the filtrates for dvi-manganese [2]. In this way preparations estimated to contain 1 to 2 per cent. of dvi-manganese were obtained. When these are heated in a stream of hydrochloric acid gas they give a volatile higher chloride of dvi-manganese which, together with the excess of hydrochloric acid, is retained in a wash-bottle containing water. This solution, on electrolysis, gives a bright silvery-grey deposit on the cathode. The metallic deposit of dvi-manganese is readily soluble in hydrochloric acid, giving a very pale green solution which yields no precipitate with sodium or ammonium hydroxide solutions. The metal ignites readily in the air, emitting a white fume of the volatile higher oxide as it burns away.

Heyrovsky, who detected the presence of the element in manganese solutions by means of the dropping mercury cathode [3], prepared products rich in dvi-manganese by dipping strips of zinc or platinum into concentrated manganese solutions and by the addition of manganese amalgam to similar solutions. The deposits so obtained were all found, on X-ray examination, to contain the new element.

The methods used by the German investigators were at first rather involved, and it is to be supposed that they have since simplified the process as originally given. The procedure consisted in heating the powdered columbite with sodium hydroxide and nitrate so that iron and the earthy metals remained insoluble. After separating most of the niobium and tantalum, the manganese was removed with hydrogen peroxide. The filtrate, coloured yellow with sodium chromates, was repeatedly saturated with hydrogen sulphide and acidified in order to obtain the sulphide of the new elements together with those of about twenty other elements present in columbite. Treatment with nitric acid removed most of the remaining niobium, tantalum, and tungsten. Addition of alkali then separated out most of the iron, cobalt, nickel, manganese, tin, and copper, and left a product containing 1 to 5 per cent. of the new element. More recently [24], in treating alvite and gadolinite, these authors first removed molybdenum as ammonium phospho-molybdate and effected the final purification by fractional sublimation of the volatile oxide. This gave a product containing only minute traces of other metals (molybdenum, arsenic, antimony, and lead).

When the sulphide is heated in a stream of hydrogen, it leaves the metal in a form which readily burns to one of the oxides on heating in air. The atomic weight of the element has been estimated from the ratio of the metal to sulphur in the sulphide by this process. It is given by the Germans as 188·7.

When Mendělejev published his *Periodic Classification* of

the elements in 1869, he left a number of blank spaces for unknown elements whose existence he predicted. Among these were eka- and dvi-manganese in the seventh group, occupying the places directly under manganese. Mendělejev predicted very fully and, as has subsequently proved to be the case, accurately the properties of some of the then missing elements (*e.g.* scandium, gallium, and germanium), but of the congeners of manganese he only mentioned that they would resemble that element in their chemical properties. This has proved to be correct also, for several oxides of dvi-manganese have been obtained, the higher ones being acidic and the lower ones basic. More than one chloride exists and a sulphate is known.

The Oxides

The most exhaustive and complete work on the oxides of the new element has so far been carried out by I. and W. Noddack [24]. So far five oxides have been obtained and described:

- D_2O_8, a white amorphous volatile solid, m.p. 150–155° C.
- D_2O_7, a hygroscopic yellow solid, melting above 220° C. and boiling at temperatures over 450° C.
- DO_3, a red oxide obtained on oxidising lower oxides in solution.
- DO_2, a (blue-) black oxide.

There are also violet-blue oxides obtained by reducing the higher ones with sulphur dioxide or carbon monoxide whilst they are heated in a suitable tube. Their composition is said to correspond with the formula D_2O_5.

When impure dvi-manganese is heated in oxygen, the lower oxides are the main products. If the metal is heated in oxygen to 200° C., it glows and emits a thick white smoke which on analysis corresponds with the composition DO_4, but the formula D_2O_8 is suggested to accord with the maximum valency of seven which dvi-manganese should possess. When the element is heated more strongly in oxygen, the vapour condenses to yellow drops which solidify on cooling to crystals of D_2O_7. As these can be distilled unchanged, this oxide has been used for a molecular weight determination by Victor Meyer's method, and gave the value of 502 (at 520° C.); the theoretical value is 490 for this compound.

The black oxide is basic and will dissolve in sulphurous acid. The solution, on continued boiling, gives the sulphate of dvi-manganese. This oxide in nitric acid solution gives a reddish coloration owing to the formation of the red oxide, DO_3. Caustic soda turns it yellow and forms the dvi-manganate

from the solution of which lime water and baryta water precipitate the yellow insoluble dvi-manganates of calcium and barium.

When the yellow oxide, D_2O_7, dissolves in water it yields an acid like $HMnO_4$ and $HClO_4$, which is capable of dissolving the hydroxides of iron, aluminium, and zinc, and which will also react with iron and zinc, evolving hydrogen. The acid is reduced by means of zinc and sulphuric acid and by hypophosphorus acid, but is re-oxidised on shaking in the air. Among the salts that have been obtained are those of potassium, sodium, ammonium, and barium by neutralisation, and the silver salt which is obtained as a white precipitate on adding silver nitrate solution to the sodium salt solution.

In an earlier communication [22], W. Noddack had stated that the white oxide was the heptoxide to which he assigned a melting-point of 26–30° C. The yellow compound was regarded as the trioxide and was stated to melt at 160° C. In the same communication the author mentions that a heptasulphide, trisulphide, and a disulphide have been prepared.

The Chlorides

Dvi-manganese forms a volatile higher chloride, possibly DCl_7, although the Noddacks regard it as a hexachloride, and also a non-volatile lower salt, DCl_2.

According to Dolejšek and Heyrovsky [4], chlorine prepared by the action of hydrochloric acid upon potassium permanganate contains appreciable quantities of dvi-manganese as the volatile higher chloride. It is now found that the chlorine disengaged on adding a concentrated manganese chloride solution to solid commercial potassium permanganate also contains the new element in combination. When the gas is cooled, small green crystals appear, and when passed into barium hydroxide solution a precipitate is formed. The same occurs when the crude oxide is treated with hydrochloric acid gas. With aqueous solutions of hydrochloric acid a deep light green solution has been observed (Druce and Loring [2]; Dolejšek and Heyrovsky [3]). The green chloride was also obtained with an ethereal solution of hydrochloric acid. The chloride is, however, readily decomposed.

The lower chloride, analogous with manganese dichloride, results when the metal is dissolved in dilute hydrochloric acid.

The Sulphate

When chlorine containing the higher dvi-manganese chloride is absorbed in barium hydroxide solution, a dark precipitate

containing the dvi-manganese separates out. This is partly soluble in hot sulphurous acid forming dvi-manganese sulphate, and may be filtered from the barium sulphate simultaneously formed. On evaporation the filtrate leaves a white residue, difficult to crystallise and containing impure dvi-manganese sulphate, contaminated with some manganese. With caustic alkalis a dark precipitate of the hydrated oxide was formed, which redissolved in excess on exposure to the air by oxidation giving the dvi-manganate.

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THE LIFE AND WORK OF SIR HUMPHRY DAVY. (Joshua C. Gregory, B.Sc.)

"In order to love mankind, we must not expect too much from them." If this maxim of Helvétius sounds cynical, a simple revision will leave the wisdom, and remain generous. Accept from every man, or from every thing, what is worth accepting. The wisdom of life requires a criticism that observes the good and notes the unworthy without acrimony. Sir Humphry Davy became an easy victim for the belittling habit when success spoiled him into an egotistical, and sometimes rather petty, man. He was too greedy for fame, and too sensible of his own achievements, to be very generous, or even just, to his fellows. The jealousy that prompted him against the election of Faraday into the Royal Society revealed the ungracious man within, as frequent tactlessness and resentful irritation often exposed a central egotism. His close brotherly relations with Dr. John Davy were probably promoted, to say it without cynicism, by the unswerving admiration of the younger. He had his great moments, he had one when he refused to patent his safety lamp; but there must be candour enough in the record, as well as generosity in the estimate, to admit the deficiencies of the man. The admission is easily forgotten in the splendour of the achievements. Faraday never forgot that, though Davy the man was often little, Davy the Natural Philosopher was always great. No solemn warning is needed to take from Sir Humphry Davy what he is, or was, so manifestly able to give.

Like Pandora in the myth, Sir Humphry Davy received many gifts from the gods. Coleridge believed that he would have been a foremost English poet if he had not become a foremost Natural Philosopher. It is doubtful whether Davy the poet were ever a real rival to Davy the man of science: the poet was certainly rapidly outpaced by the chemist.

Sir Humphry seems to have had the supreme happiness and the great success that attend the exercise of great powers precisely where they are most able. If it is ever true that "each has his special talent," as Emerson said, it was true of Davy, and if "the mastery of successful men" ever "consists in adroitly keeping themselves where and when that turn shall be oftenest to be practised," as Emerson also said, it was so with the discoverer of laughing gas, the alkali metals, the elementary nature of chlorine, and the foundations of electro-chemistry. The "adroitly" is perhaps less pertinent than the "special talent" or than its persistent practice, for, though Davy was adroit, he was more palpably driven by impulse and sustained by a power of performance than maintained in his appropriate activity by adroitness.

Humphry Davy was born at Penzance on December 17, 1778, and he "was just entering on his nineteenth year" when Natural Philosophy, or chemistry, excited his interest in late 1797. He had been apprenticed to Bingham Borlase, a Penzance surgeon and apothecary, early in 1795. Professional interests, therefore, may have directed him to Lavoisier's *Elements of Chemistry*, probably in Kerr's translation, and Nicholson's *Dictionary of Chemistry*. He had begun to write his *Essays on Heat and Light, etc.*, a few months after his first studies in chemistry. They were published in 1799, less than two years after, to commend new versions of many phenomena to science. They contained crude speculations and imperfect experiments, but their obvious fervour and equally obvious power displayed the young Davy dropping into the very niche for which nature had intended him.

Davy had evidently found his genuine home in the laboratory: he could neither leave it nor refrain from discovering. Nothing could divert him from his experimental career of discovery. He was so socially sought from almost the very moment of his first lecture at the Royal Institution on April 25, 1801, that his friends feared for him. Social prestige and social pleasure did allure him, but there is little sign of that allurement in his experimental achievements. Medicine was unable to reclaim him for the fame and fortune of a great physician: he did meditate a return to his first profession, he even prepared to return in 1810, but he could not leave his laboratory. The Church could not persuade him to devote his eloquence to religion. His marriage to Mrs. Appreece in 1812, when he severed his close connection with the Royal Institution, did not interrupt his habits of research. A "commodious portable apparatus" accompanied his honeymoon, for if he could not go to his laboratory, it must come to him. It accompanied him to the Continent in 1813, for,

though foreign laboratories were to be open to him, research was *always* to be possible. He was discovering still, as Faraday wrote of him in 1815, as was his practice. That could have been written of him at almost any moment of his life, for, even during his last desperate search for health when he left England on March 29, 1828, to die at Geneva on May 29, 1829, he was inquiring still. His "real" existence, as he himself said, was "amongst the objects of scientific research." Even poetry seemed to him to amuse more than it instructed, but science instructed more than it amused. When Lavoisier and Nicholson had revealed his genuine interest and his special aptitude to him, nothing could deflect him from scientific research. He was born to be a Natural Philosopher, or chemist, a chemist he became, and from chemistry or natural philosophy nothing could turn him. He sought the laboratory as unswervingly, and as passionately, as a lover seeks his mistress.

Fortune was lavish to Davy, though, as no man is without breaks in his bliss, he also suffered. His marriage was unfortunate, and illness only allowed him nearly fifty-one years of life. His greed for fame may have sometimes stirred him to ungenerous fears: John Davy thought that he worked feverishly at one period to outpace rival investigators. Fortune, however, was too lavish to Davy for him justly to fear either a preliminary obscurity or a relapse into it. Sir Humphry Davy is almost unique in the annals of science for a combination of high contemporary scientific esteem with a fervid more popular favour. His scientific achievements were solid enough to ensure permanent scientific reputation; they were also dramatic enough, and his eloquence sufficiently great, to fix the public eye upon him.

Nature endowed Davy with great powers, and fortune provided him with a great opportunity for achievement. The French revolutionaries had guillotined Lavoisier in 1794, but they had not guillotined his concepts. A young man who was born in 1778, and attracted to natural philosophy in 1797, when the "new theory of chemistry" was steadily asserting itself, need not spend his first energies on disengaging his mind from the grip of the Phlogiston Theory. The power of Stahl's Phlogiston Theory, revised by the discovery of hydrogen, and momentarily augmented by the discovery of the composition of water, did disturb Davy's speculations before 1812, and perhaps even after that. His youthful eyes were, however, sharpened towards chemical phenomena by Lavoisier. The state of the science of chemistry, as his biographical brother, John Davy, noted, was very favourable to rapid advances when he was a young man. The "antiphlogistic" theory, as Lavoisier's chemistry was often named,

called for complete establishment, and presented ample opportunities for discerning experiment. The chemistry of gases presented possibilities of a still richer harvest than had been reaped by Priestley, Cavendish, and others. Chemistry had before it the obvious tasks of improving its analytical technique, and of applying its knowledge to nature and art. The science was still narrowly bounded, new knowledge was easily acquired, and enterprise was tempted in every direction. The young and brilliant Davy, with his quick mind, with his power of rapid experimental achievement, and with his natural instinct for the laboratory, could hardly fail to achieve much. His native brilliance was as adapted to secure popular fame as to secure scientific esteem, and fortune arranged to attach a dramatic quality, appropriate enough to Davy's brilliancy, to his early discoveries.

Pneumatic fervours quickly established Davy as a hero of a veritable laboratory drama. Men were looking hopefully, and often credulously, to gases for curative powers. Jean Ingenhousz warned practitioners that oxygen was no Elixir of Life, though it might be a valuable medical aid; other gases were also fervently applied with high hopes, and often, as records show, with credulous belief. Dr. Beddoes persuaded subscribers to maintain a Pneumatic Institution at Bristol to investigate the medicinal effects of gases. He also brought the young Humphry Davy to superintend it in the October of 1798, and installed him at Clifton early in 1799. Beddoes had been impressed by the young man's youthful *Essays*, which he published for him in the same year. Davy owed his first step in his career of discovery to these *Essays*, for they brought him to Bristol to discover. He also experienced a pain from this youthful sally into science, for many critics had a heyday of scoffing at the young author. He quickly repented, perhaps too bitterly, his impetuous publication, but his mortification at critical quips toned down his immature impetuosity into a swift effectiveness. He very rapidly substituted an enduring fame for a painful notoriety: Davy was always rapid, and he matured, as he did everything else, very quickly.

The name "laughing gas" commemorates the laughter and capers of many under Davy's nitrous oxide. Pluck, perseverance, insight, and experimental aptitude led him to the knowledge of this gas during 1799: chemical ability was required to handle it, and courage to inhale it. The gas seemed to the February issue of the *Philosophical Magazine* in 1800 to intoxicate through the lungs, as alcohol through the stomach. Dr. Beddoes continued to hope for cures of paralysis from the potent gas, though he advised caution, and verified the production of "fits" in "hysterical females"

by excessive doses. When Davy publicly summarised his researches and conclusions he had experimented too ruthlessly upon himself with gases to be too credulous towards pneumatic cures. Nitrous oxide seemed to him to be a suitable anæsthetic for minor surgical operations, and dentists still confirm his conclusion. The popular fancy fastened on one dramatic quality of the gas. The effects of nitrous oxide varied with individuals, and with the same individual at different times, but it could induce ecstasy, as in the paralytic who "felt like the sound of a harp." Many found Paradise in the "silk bag" that contained the gas, and thrilled subjects often refused to surrender it. Coleridge recorded his pleasures from nitrous oxide, and many visited the laboratory to inhale it. Dr. Thomas Thomson complained in 1830–1 that Davy's fortunate discovery of laughing gas probably made him more celebrated and more subsequently successful than all the sterling merit of his later researches. It did make him immediately famous; it also brought him rapidly to London, for neither the Paradise of nitrous oxide nor the brilliances of Davy were to be reserved for Bristol.

When Davy lectured on respiration at the Royal Institution on June 20, 1801, those who wished could breathe nitrous oxide after the lecture, and some did. The spectators were amused by the antics of the experimenters, and one subject at least enjoyed Paradise, for Mr. Underwood was so transported, and so reluctant to leave heaven for earth, that the "breathing-bag" had to be snatched forcibly from him. Laughing gas, however, though it effectively introduced Davy to London audiences, did not wholly secure the vogue that he gave to the Royal Institution. His eloquence rapidly converted an organisation that had been founded in 1799 to convey scientific knowledge and benefit to the poor into a haunt of fashion. His opening lecture on April 25, 1801, effectively removed lingering doubts from the mind of Count Rumford. A thronged lecture-theatre, compliments, letters from ladies, and public solicitude quickly made Davy and the Royal Institution a vogue of London. He was raised rapidly to a professorship on May 31, 1802, when he was nearly twenty-four years old. His popularity steadily increased as long as he lectured in the theatre of the Royal Institution till 1812. His audience grew steadily too, until, if his admiring brother wrote truly, it was at the last "scarcely less than 1000." Coleridge culled metaphors from Davy's lectures, others sought stir from his eloquence, social fervours brought many, fair ladies admired the brilliant lecturer. The inanition that threatened the Institution when Davy joined it was rapidly exchanged for a threatening flood of fashion.

Davy's unswerving devotion to research did not falter before this amazing success, nor did his indulgence in social pleasures wean him from the laboratory. The *Edinburgh Review* had not "made a point," in 1811, "of following this excellent chemist through his various and important discoveries" simply because he filled his lecture theatre with fashion. Savants attended as well as fashionables. Nor did they only relish an eloquent exposition: Davy made the laboratory of the Royal Institution as famous in the annals of scientific discovery as he made its theatre a brilliant society tryst. His experimental achievements constantly provided materials for exposition. Dramatic quality did not desert his discoveries, for the isolation of the alkali metals made almost as dramatic an appeal as nitrous oxide.

The Royal Institution had many duties for Davy. He studied tanning, and lectured on it. He collected scattered materials into a course on geology. He lectured on agricultural chemistry for the Board of Agriculture from 1803 to 1813. He had his general lecturing duties, and he experimented for these, and for other, purposes. The Royal Society was little behind public favour in its honours, for it made the young professor a Fellow in 1803, and handed him a Copley Medal. This election added another to Davy's many activities in 1807, when he became Secretary to the Royal Society, to remain so till 1812. He gave his first Bakerian Lecture to the Royal Society on November 20, 1806. Neither his multifarious duties nor his social pleasures had prevented him from making important discoveries when he read this paper "On Some Chemical Agencies of Electricity." Dr. Thomas Thomson considered this paper to be Davy's greatest production, and the most admirable inductive inquiry of his age. Though France and England were then at war, the Institute of France awarded a medal to Davy twelve months after the publication of the lecture.

The "pile of Volta" precipitated Davy into this research. Nicholson and Carlisle had startled chemists before June in 1800. They placed two separated platinum wires, with their points apart, in a tube of water. When one platinum wire was connected with one end of a voltaic pile, and the second platinum wire with the other end, gases fizzed off from the platinum wires. Further experiments revealed that oxygen gas bubbled off one wire, and about twice its volume of hydrogen off the other. The galvanic current seemed to decompose the water: the process was leisurely in the preliminary experiment, for about 1·17 cubic inches of total gas were collected in thirteen hours, but the decomposition apparently occurred.

Davy had already experimented with the pile constructed

for Dr. Beddoes before he left the Pneumatic Institution for London in 1801. The startling experiment of Nicholson and Carlisle diverted him from his other researches. Chemists stared with astonishment at oxygen and hydrogen bubbling off from *separated* wires. If water were decomposed at one wire, and oxygen came off there, the released hydrogen seemed to hurry through the liquid to the other. If hydrogen bubbled off where the water was decomposing at the wire, the oxygen, in its turn, seemed to scurry through the liquid to be in time at its own wire. These enigmatical travels of the gases could be lengthened. Davy himself lengthened them. He dipped each platinum wire into a *separate* vessel of water, and connected the two wires as usual with the respective ends of a pile. Then he connected the two separated vessels of water either by dipping one of his own fingers into each, or by a muscular fibre, or by a metallic conductor. The gases still bubbled off *separately* at either wire. A perplexed "correspondent" informed the *Philosophical Magazine* in 1801 that the experiment could be performed by connecting the two vessels of water with a syphon. He was incredulous: the hydrogen and oxygen could not scamper through the syphon to be in time at their own wires as they seemed to do. Davy was incredulous too: he could not credit opposite scampers of gases *through his own hand* to get into their own glasses and reach their own wires. The "correspondent" declared that the water was not decomposed: the two gases, oxygen and hydrogen, were oppositely *electrified waters*. Davy had experimented very systematically on these and other galvanic effects when he gave his first Bakerian Lecture.

Acids and alkalis also disconcertingly presented themselves at the wires during the galvanic decompositions. Davy, by successively eliminatory steps, showed that these were derived either from impurity in the water or from the material of the apparatus. The enigma of the scampering gases, however, was repeated by, for example, sulphate of soda. When the solution of the salt was galvanised, as in the previous experiments with water, alkaline soda collected round the negative wire and acid round the positive. The acid and the alkali seemed to travel through the water to their own wires, just as did the gases from galvanised water. They also seemed to travel through a syphon, or through a connecting fibre of "washed asbestus," and if a third vessel was interposed between the two vessels into which the wires were dipped, and duly connected with them, they seemed to travel through that too. If the interposed vessel contained litmus, the acid seemed to travel through it without reddening it, and the alkali to travel through it without making it blue. He did, however,

discover that when barytes was thus electrically transferred through a vessel containing sulphuric acid a precipitation occurred. There was also precipitation when muriatic acid (HCl) was passed similarly through an interposed vessel containing a solution of nitrate of silver. He could also relieve the incredulity of the " correspondent " and others by substituting a *train of decompositions and recompositions* through the liquid for enigmatical scampers through syphons or " washed asbestus " or simply through the water between two platinum wires. Davy's mind was not at rest, nor was he yet able to discard electrified-water versions with perfect certainty, but he had systematically disclosed the powers of the pile, and, even if dubieties did still linger in interpretation, he had discovered many facts of galvanic decomposition.

The dramatic sequel of potassium and sodium from potash and soda by galvanic decomposition was announced in Davy's second Bakerian Lecture on November 19, 1807. The exciting isolation of potassium had been effected in the October of the same year. When Davy exposed fused potash or soda to the galvanic current from the pile, as he had analogously exposed water or solutions of salts to it, the metal separated at the " negative surface." The electricity both fused the alkali and decomposed it.

The *Edinburgh Review*, delivering its pontifical judgment in 1808, admitted that Davy had made more discoveries than any man since Newton. Yet he was no genius, for, while Newton created his own tools, Davy owed his success to the powerful instrument placed in his hands. The play of *Hamlet* is often said to act itself; the pile, according to the literary pontiff, produced its inevitable results in a similarly pseudo-automatic way. Volta did proffer the pile, Nicholson and Carlisle did give the cue by decomposing water, substances did cry for a pile, a powerful pile if need be, to decompose them, and the Royal Institution did supply the powerful instrument. Davy, however, supplied the rapid brilliancy: he had to study out the powers of the pile, to construct the pile or battery appropriately, and to apply it effectively.

Davy " bounded about the room in extatic delight " as " the minute globules of potassium burst through the crust of potash " and then flamed. A metal so dramatically secured, and so dramatically decomposing water by a touch, again exhibited Davy as a veritable hero of a laboratory drama. As all eyes were captured by laughing gas, so they were captured again by potassium and sodium. There was less drama, though there was genuine scientific achievement, in his subsequent procuring of the alkaline earth metals and magnesium by the power of the pile.

A severe illness interrupted Davy's researches as 1807 merged into 1808. Dr. John Davy described it as "the golden period of his life" because public solicitude made him still more popular. His successes with magnesium and the alkaline earths rewarded his return to work in 1808, but a period of chemical perplexities preceded the clarifying disclosure of "undecompounded" chlorine in 1810. During this period, when Davy was wrestling with analytical difficulties and surveying alternative explanations, he seriously considered a revised phlogistianism. There was $x +$ phlogiston in any metal, according to phlogistians. Revised phlogistianism substituted hydrogen for the phlogiston. It was appealing to some chemists, but Davy never adopted it, though he did seriously consider it. Potassium and sodium were very *light*. So was hydrogen. *Ammonium* did contain hydrogen, and it seemed to be metallic because it appeared to form an *amalgam* with mercury. These data, and others, suggested that all metals might contain hydrogen. Davy had not finally surrendered this possibility even in 1812, and phlogistic remnants lingered elsewhere, as in Murray's chemistry of 1819, but Davy's serious consideration of revised phlogistianism up till 1810 can be regarded as the last great effort of the old phlogiston theory to reassert its ancient power. Even in 1812, however, Davy himself had not completely laid the ghost of phlogiston.

The year 1812 sharply divided Davy's life, for he then severed his old connection with the Royal Institution, though his unsalaried services were partly retained without restricting his liberty of action. His marriage with Mrs. Apreece made the division of his life sharper still. The Prince Regent also knighted him : Bart. was added to his title in 1818, when he had invented the "wire-gauze safe-lamp" in 1815.

He had startled science again before this decisive year. His systematic failures to decompose chlorine presented no singly dramatic episode like the silk bag of laughing gas or the flaming globules of potassium, but they handled some scientific concepts severely. Davy was suspecting several substances of containing oxygen before he finally dismissed his suspicions that chlorine did. He suspected nitrogen of being very inert towards oxygen because it was already very saturated with it, and he suspected hydrogen of holding some oxygen too tightly to part with it. The vigour of chlorine, or, as it was then usually called, oxymuriatic acid, suggested that it contained oxygen. This had been the traditional view since Berthollet, in 1785, regarded it as "oxygenated muriatic acid" (HCl). When a solution of the gas in water evolved oxygen, and produced muriatic acid, the oxygenated muriatic acid was presumed to lose its oxygen and revert to

the original acid. Davy showed that the oxygen had come from the water, and not from the oxymuriatic acid itself. He showed that well-ignited charcoal did not extract oxygen from the gas. He showed that oxygen was never apparently produced from chlorine when other substances that contained oxygen were absent. Davy displayed his mental power in his ability to discard a heavily preconceived opinion by well-contrived experiments. His experimental failures to obtain oxygen from oxymuriatic acid convinced him that there was none to obtain, though he had assumed, as all other chemists assumed, that there was. He proposed the name *chlorine* for the gas that had been called oxymuriatic acid because no oxygen could be obtained from it. The name denoted its colour, and would not embarrass any future successes in decomposing it. For the moment it appeared to be "undecompounded," and should be regarded as an element until it was discovered to be compound. Scheele had regarded it as "undecompounded" when he first prepared it in 1774, and in 1810 it ought to be so regarded again. Davy always insisted on the relativity of undecompounded chlorine, as of all presumed elements, to analytical achievement. He never forgot that an ostensible element might be decomposed.

Davy never believed in Dalton's *atoms*. He never used a chemical formula, nor a chemical equation, and, though he employed exposition by particles, he regarded even corpuscles as hypothetical. He did adopt the "proportions," as he usually called what Dalton preferred to call atomic weights. By observing that chlorine united in combining proportions represented by the "proportion" 32·9 (now 35·457), or in simple multiples of it, he completed his case for its elementary nature.

Davy's rapidity, his gluttony for work, and perhaps also his desire for fame, sometimes made him hurried as well as rapid. Competent critics have detected haste in his *Elements of Chemical Philosophy*, which was published, soon after his marriage, in 1812, when he broke his life into two distinguishable halves. John Davy more discerningly wrote, in 1832, "a second edition of it has not been required," than "though rapidly composed, it was not, in fact, hurried." It was, doubtless, "in much estimation when it appeared," for Sir Humphry Davy could hardly write without authority, nor without receiving deference. The *Elements of Chemical Philosophy* is obviously from a great pen, and the historian will always value its conspectus of the science of the period, and its disclosure of Sir Humphry's own concepts, but few will deny its manifestation of the haste to which his rapidity sometimes predisposed him. It recorded one change of conception that undecompounded chlorine had imposed upon

chemistry. Oxygen had been central and pre-eminent in the science since Lavoisier. Its vigorous support of combustion and its powerful electro-negative character conferred a uniqueness upon it. Chlorine itself was originally presumed by Davy to support combustion vigorously, and to be strongly electro-negative, because it contained oxygen. In the *Elements of Chemical Philosophy* oxygen shared its unique position with the new chlorine. Both were vigorous supporters of combustion, and both were strongly electro-negative.

Undecompounded chlorine, and its analogies with oxygen, sharpened Sir Humphry Davy's eyes to perceive companion elements. He failed to isolate fluorine, for, like "the fabled waters of the Styx," it could not "be preserved," but he had experimentally revealed its probable presence in compounds before his continental tour in 1813. Fluorine was too active an analogue of chlorine to be bottled, but iodine was less reluctant. Davy arrived in Paris in 1813, and Ampère gave him a little *x* in the November of that year. Courtois, a French manufacturer of saltpetre, had prepared a violet vapour, two years before, by acting on the ashes of sea-weeds with sulphuric acid. The condensed and lustrous crystals of this vapour was *x*. It was puzzling the French chemists when the gift to Sir Humphry Davy, as Dr. John Davy noted with brotherly admiration, speeded the inquiry. Davy, working quickly on very little material, and with eyes directed by chlorine and fluorine, soon recognised iodine as an analogue of chlorine. His sally into the research was not welcomed, for he was personally unpopular in Paris, but his usual discerning rapidity, directed by his previous discoveries, quickly solved a puzzling problem.

His continuous course of continental research, conducted in foreign laboratories, with his portable apparatus, and with the assistance of Faraday, who accompanied him as assistant, was constantly communicated to the Royal Society. He returned to London on April 23, 1815, to communicate the latest news of his researches during travel. By the end of the year he had again confirmed his experimental powers by protecting coal-miners against explosions.

The finest Davy was displayed in his invention of the "wire-gauze safe-lamp." His power of rapid and effective experimental achievement was perhaps never more evident. In well less than a year he had sufficiently perfected it. His unselfish devotion of scientific knowledge to the service of humanity forbade adding "four horses" to his "carriage" by patenting his invention. There was a controversy over the prior claim of George Stephenson to the invention of a lamp, but a banquet and a gift of plate from the coal-owners

in 1817, Rumford Medals from the Royal Society, and a baronetcy in 1818, recognised the achievement and humane devotion of Sir Humphry Davy.

He was busy with other researches till he travelled abroad again in 1818, and he was busy during his tour till he returned in the June of 1820. He had resigned his Secretaryship of the Royal Society in 1812, when he made so many changes in his life without abstaining from research, and now, after the death of Sir Joseph Banks, he became its President. He was re-elected for seven successive years, till 1827, when his health finally failed and he had only two more years to live.

A science of electro-chemistry had sprung, like Jonah's gourd, from the significant experiment by Nicholson and Carlisle in 1800. In 1820, the first year of Davy's Presidency, Oersted disclosed, by another significant and rapidly fertile experiment, a further power of the pile. The galvanic current had decomposed water in 1800; its passage along a wire was now observed to deflect a compass needle. In July 1820 Oersted publicly announced, in the words of Dr. John Davy, that "when the extremities of a voltaic pile, or battery, are united by a perfect conductor, as a metallic wire, and the compass is brought near it, the needle is deviated by the wire, and may be made to deviate from its natural position." Natural philosophers had been too preoccupied with the decomposing powers of the pile to observe its influence on the compass needle. Sir Humphry Davy always insisted that experimenters had been too busy with decompositions to perceive the magnetic effects of the galvanic current. When the concealed effect was revealed he was instantly ardent in following up the clue. A friend's letter from Geneva, hinting at the discovery by the "Danish philosopher," kept him from Cornwall at his "magnetising" table. He followed rapidly along routes of inquiry, and he disclosed significant data, but Ampère was the great reaper of this harvest, as Davy himself had been of the former.

Though Davy's health was showing distinct signs of failure in 1823, his life of effective research did not end. In this year, about three years after Oersted had directed science to new achievements, Sir Humphry Davy prepared for another harvest. Chlorine had disturbed scientific concepts, and enforced an adjustment upon them; it now initiated a progressive line of research. The yellow crystals deposited from a cooled solution of chlorine in water seemed to Berthollet to be the solidified gas. This was the usual opinion till 1810, when Davy identified them with a *hydrate of chlorine*. In 1823, Faraday, under Davy's instructions, and applying his own experimental skill, heated chlorine hydrate in one limb of a sealed glass

tube, shaped like a Λ, and compelled the evolved chlorine to squeeze itself into a liquid in the other limb. A handful of the more coercible gases was also liquefied by Davy and Faraday after chlorine had indexed the method, but Davy was dead before Faraday again extended the list of successes, and Faraday himself did not live to see the final triumph of liquefied air. Sir Humphry Davy, however, had given one impetus to a progressive conquest over intractable nature.

Davy was less completely successful with a problem from the Admiralty than with the "wire-gauze safe-lamp." The inquiries occupied him from the latter end of 1823 to the summer of 1826. The copper bottoms of ships were liable to corrosion, and Sir Humphry Davy discovered that contact with zinc, or other suitable metals, protected them. He discovered this protecting effect, in which the protecting metal suffered instead of the copper, by reasoning from the connection of electrical action with chemical change, and by many experiments. The principle can be expressed, though Davy himself had his own special version, by saying that the more electropositive metal bore the brunt of the corrosion. A sufficient area of iron plate, for example, on the copper bottoms of ships diverted the corrosion from the copper to itself. Unfortunately for Davy, such "protectors" were less effective towards fouling by weeds or barnacles or insects or mineral deposits than towards corrosion of the copper, and the Admiralty finally removed them from their warships. Sir Humphry Davy was intensely annoyed by the decision, and his brother shared his indignation. The "protectors" would have been perfectly effective, Dr. John Davy bitterly remarked, if the bottoms of ships were as religiously cleaned as their decks were holystoned. In 1826, when Sir Humphry Davy gave his last Bakerian Lecture, he chose "On the Relations of Electrical and Chemical Changes" for his theme. He then finally summarised the electro-chemical researches with which he had been so intimately associated, and again described his version of the relations between electricity and chemical affinities.

The death of his mother in the September of 1826 struck heavily at Davy's already failing health. A painful journey to Ravenna secured little relief in 1827, and the resignation of his Presidency of the Royal Society, on his return to England, cast a distinct shadow of the coming event. In the same year, in 1827, the publication of the fourth edition of his *Elements of Agricultural Chemistry* reminded the public of his long association with applications of chemistry to agriculture, and virtually concluded it. The Royal Society was to receive two more communications from him, but, in 1828, when they

were read for him, Mr. Davies Gilbert had succeeded him as President, and in the spring of that year he himself left England, never to return.

His last paper, read for him to the Royal Society when he was on the Continent, "An Account of Some Experiments on the Torpedo," was not an altogether inappropriate epitaph. It ended inconclusively with the recognition of an "animal electricity" that was "distinctive and peculiar." It also hoped to have made a first step towards a more complete knowledge. Sir Humphry Davy had always a lively sense of the progressiveness of science. He realised the labileness of hypotheses and theories. Dr. John Davy once illustrated his brother's distinction between hypothesis and theory by fluorine and chlorine. Many data concorded with the presumption that fluorine did lurk in many compounds, but its existence was *hypothetical* because it had not been isolated. The *theory* that chlorine was an element expressed all the known facts of its behaviour. The gas invariably behaved as if it were an element. Sir Humphry Davy constantly urged that hypotheses, with their unverified presumptions, should be experimentally converted, as far as possible, into theories that summarised facts. Chlorine might not be an element, it would lose its elementary status if it were decomposed by superior methods, and even a theory, however successfully it expressed the data of the moment, might fail to express the data of the future. Davy's last paper expressed his sense of the co-operatively secured progressiveness of science. It also revealed the inveterate experimental philosopher: neither fame nor wealth nor love nor poetry could seduce Sir Humphry Davy from his appointed task, nor could any other distraction of the world, nor could death itself until it dealt its last decisive stroke.

That last decisive stroke was soon to fall. Sir Humphry Davy, accompanied by Lady Davy, reached Geneva on May 28, 1829. The hour had come for the child who had been born to enduring fame at Penzance, nearly fifty-one years before, at last to stay his inquiring hand. The call that comes to all men came to him early in the morning of the next day, and the cemetery of Plain-Palais received all that was mortal of one of Cornwall's greatest sons.

NOTES

Chemical Research in Czechoslovakia (G. D.)

At the beginning of last year the Czechoslovak chemists founded a monthly journal in which original contributions have since been published either in English or French in order to make known to scientists at large that much research work was being carried out in pure science at the universities and technical institutes of that country. The *Collection* of Czechoslovak Chemical Communications has published original articles covering the whole field of pure (but not applied) chemistry, and some of them are particularly noteworthy.

Prof. Emil Votoček, with his collaborators, Drs. F. Rač, L. Rys and S. Malachta, has made a study of the methylpentoses, the hydrazones and osazones obtained by the interaction of di- and trichloro-phenylhydrazines with various aldehydes and ketones. In particular he has utilised the results of these investigations in identifying the less well-known hexoses and pentoses, and has thus added to the knowledge of the carbohydrates generally. He has also studied the conversion of certain sugars into furane and hydro-furane derivatives.

In Physical Chemistry, Prof. J. Heyrovsky (the other co-editor of the Collection) continues to publish fresh investigations made with his polarographic arrangement and dropping mercury cathode apparatus. The latest papers from his laboratory deal with the electro-reduction of arsenious oxide in acid solution (by Miss K. Kačirkova), the deposition of zinc and of cadmium from alkali cyanide solutions (by Dr. I. Pines) and the influence of fatty acids on the maximum of current due to atmospheric oxygen (by Dr. J. Rasch). The same method has been employed by Prof. Heyrovsky and Dr. Terechov in a study of the acidity of mannitol towards alkali hydroxides, and the apparatus is also finding application in the detection of small amounts of impurities in reagents and in micro-analysis.

Of the papers dealing with Analytical Chemistry there are two by Drs. Jilek and Lukas describing respectively a new rapid electrolytic method for estimating bismuth in acid

solutions, and an electrolytic method for the determination of thallium as thallic oxide. In the same branch, Dr. S. Landa has devised a convenient apparatus for the estimation of sulphur in organic liquids. Prof. O. Tomiček has been engaged upon an examination of the various argentometric titration methods that have been proposed for the determination of mixed halogen salts and he has modified the more suitable ones to his determinations of the small amounts of bromine and iodine in the numerous Czechoslovak natural mineral waters, and the saline products obtained therefrom by evaporation. His results are valuable since he has found the limits of reliability to be placed upon the figures found for the amounts of iodide and bromide when admixed with a large excess of chloride.

Work has also been carried out upon the hydrolysis of beryllium salts, and the influence of alkali metal ions upon the precipitation of zinc ferrocyanide. Finally, two chemists at the Masaryk University of Brno have made an exhaustive study of the additive compounds of organic bases with the inorganic salts of certain heavy metals, including copper, zinc, lead, and the alkaline earth halides. The *Collections* also occasionally include reviews and bibliographies of articles in pure and applied chemistry by Czechoslovak scientists. Those so far given contain some hundreds of references to papers published within the last two years, and from this it is apparent that there is considerable scientific activity in Czechoslovak universities and research institutes.

Transatlantic Short-wave Radio (S. K. Lewer)

Just over a year ago, at the request of the American Telephone and Telegraph Company, the staff of the Bell Telephone Laboratories undertook the design of a group of short-wave radio-telephone stations to connect the United States with Europe and South America. To-day all the stations, except one, are in operation, and this will be completed by the time this appears in print. The stations, which are situated in New Jersey, are representative of the very latest developments in short-wave radio communication. They are described in some detail in the August 1929 number of the *Bell Laboratories Record*. The research which started in 1921 and led up to the present-day design is outlined in a manner which is likely to give an erroneous impression, for no mention is made of the valuable results obtained by amateur radio workers in all parts of the world. It was their efforts alone which first drew attention to the hitherto unknown properties of the shorter waves. This example of cumulative discovery, the work of vast

numbers of untrained men rather than organised research by a skilled body of workers, is probably unique in the whole history of science.

In view of the progressive development of short-wave technique, ample provision has been made throughout the stations, including the buildings and masts, for alterations and additions.

The transmitting stations, which are located at Lawrenceville, utilise a well-tried circuit with up-to-date refinements. For transatlantic telephonic communication the power limit is 15 KW, and for code, 60 KW. Each transmitter is designed to operate on those wave-lengths in the short-wave range which are found necessary for communication during the hours of operation. These wave-lengths are 16, 22, and 33 metres. In each case, the wave-length is very accurately controlled by a quartz crystal piezo-electric oscillator operating at six times the transmitted wave-length. Quartz crystals having natural frequencies corresponding to less than about 40 metres are unsatisfactory for several reasons, so that frequency-doubling and harmonic generators are necessarily introduced. The valve controlled directly by the crystal is made to amplify at three times the crystal frequency by means of high-grid bias and suitably tuned circuits. The following valve doubles the frequency, which at this stage corresponds to the transmitted wave-length. This output is then made to control the main power oscillator. Considerable care is taken to maintain constancy of temperature, and of other operating conditions in order to ensure stability.

All the apparatus is remotely controlled from a central power-control board, and the entire station may be switched on or off by pressing the master " start " or " stop " button. Extremely elaborate alarms and protective devices have been introduced in all circuits which can possibly cause trouble. For instance, the high-voltage supply is immediately cut off as a result of any of the following conditions: overloads in rectifier and anode circuits; excessive water temperature (for valve cooling); and failure of grid-biasing potential, filament-heating supply or water flow. Each time an interruption occurs, one or more guard-lamps go out, and an audible signal operates for ten seconds to attract the operators' attention. All guard-lamps operate on the principle of " light on " when conditions are correct, and " light off " when trouble occurs. An interlocking system similar to that employed in modern central power stations has also been adopted for the safety of personnel and the protection of equipment.

An interesting point in short-wave technique is the need for simplicity and symmetry. As an example, the insertion of

ammeters in radio-frequency circuits at the usual points is not permissible, and instead the desired indications are obtained from small valve voltmeters used in pairs. Somewhat paradoxically, the transmitting aerial system is a vast complicated network of wires. Unlike the focal aerial and parabolic reflector system, which has been used for some years, the modern directive aerial consists of sets of radiators, *i.e.* straight half-wavelength wires, suspended from a line of masts. By interconnection in various ways, the radiation can be confined within a very small angle, of which the bisector is normal to the plane of the network. The aerial at Lawrenceville is built on this design. Two sets of conductors, one suspended one half-wavelength above the other, constitute the main radiator, while a similar network, excited parasitically, is placed one quarter-wavelength behind the first to render the system unidirectional. The efficiency of such a directive aerial increases with the number of radiators in the network, but owing to the need for simplicity, a limit is imposed upon the dimensions. However, even for wave-lengths of about sixteen metres, this type has an efficiency of one hundred times that of a single vertical wire. Each of the transmitting stations must have three aerials since it must operate on three different wave-lengths, so that there are altogether twelve aerials for the four stations constituting the group (three for Europe and one for South America). For the three channels to Europe, nine of these aerials are lined up end to end, giving a total length of nearly one mile. This arrangement is necessary in order to prevent screening.

A precaution, which is never needed in this country, but which is of vital importance in the United States, is the removal of sleet from the aerial system. Not only does the sleet load impose severe mechanical stresses in the system, but the high dielectric constant of the ice covering on the wires may seriously detune the aerial. Provision is therefore made to remove sleet from the aerial by heating the wires with low-frequency currents. If necessary, the sleet-melting currents may be applied even while the transmitter is in operation. Nevertheless, the masts have been built to withstand the load of the largest aerial system which can possibly be supported when the wires have a coating of ice one-half inch thick, and subjected to a gale of ninety miles per hour.

Each receiving station consists of three directive aerials for the three wave-lengths, as in the transmitting station, and a receiver. Here, however, the aerials are located several hundred feet apart, so that they do not influence one another. The transmission lines from the aerials to the receiver consist of copper tubes, one inside the other, separated by insulating

rings, and supported a few inches above the ground. The aerials are of the broadside type, but are designed somewhat differently from the transmitting aerials. Each consists of two rows of conductors, one spaced a quarter-wavelength behind the other and so connected with it that the two differ in phase by a quarter period. Each aerial delivers to the receiver a signal intensity nearly forty times that delivered by a single vertical half-wave aerial. The variability of the apparent direction of the received wave is, however, a major factor in limiting the degree of directivity which it is useful to attempt. The angle between the two extreme directions from which satisfactory signals may be received is about ten degrees.

The receivers are of the superheterodyne type. The signals are first amplified by two screened-grid valves, and after rectification and filtering, are amplified at 400 kilocycles by a six-valve amplifier. The second rectifier, at the end of this amplifier, is preceded by another filter. Finally, the signals are amplified at audio-frequency and are sent by land-line to New York. The disconcerting fading, which always occurs in long-distance short-wave transmission, is minimised by an automatic control. In this ingenious device, an auxiliary amplifier, fed from the 400-k.c. amplifier, provides, after rectification, an additional bias on the grid of the first rectifier in the superheterodyne. An increase in the received signal tends to increase the output from the control unit, which, when applied to the grid of the first rectifier, decreases its efficiency, and greatly reduces the rise in output volume of the receiver over what would otherwise be obtained. The output level is thereby maintained substantially constant. Fuses and failure-lamps are provided throughout the receiver, and every effort has been made to ensure convenient and reliable operation in routine commercial use.

This last remark applies equally to the entire group of stations. Alterations and additions to cope with the ever-increasing traffic are rendered possible with the minimum of interruption by the foresight of the designers in making ample provision in this direction.

The Fading of Radio Signals (S. K. L.)

One of the greatest hindrances to nearly all forms of radio communication is the more or less rapid fluctuation of signal intensity known as fading. This phenomenon has been the subject of close investigation for some time, and in recent years has proved very fruitful in aiding the study of wave propagation in the upper atmosphere. Diurnal and seasonal variations in signal strength have also contributed to these investigations,

likewise magnetic storms and solar eclipses (SCIENCE PROGRESS, Notes, January 1929), but it is the short-period fading, in which the maxima occur at intervals ranging from a few seconds to several minutes, which has yielded so much information.

A vast amount of experimental work has recently been revised and confirmed by the United States Bureau of Standards, the results being contained in a paper by T. Parkinson in the Bureau's *Journal of Research* for June 1929. Although the investigation is far from complete, as the author states, the paper very clearly summarises all previous work connected with short-period fading. The method used in these experiments consisted in selecting a particular broadcasting station and making simultaneous graphic records of its carrier wave as received by different combinations of aerials connected to duplicate receiving sets. The aerials were single vertical wires and two vertical coils situated in maximum and minimum positions respectively. The receivers were of the superheterodyne type, adjusted to give the same amplification.

The graphic records obtained with various combinations of aerials were capable of translation in terms of the apparent changes in the direction of the incoming radiation, and the phase relationships between the direct, or ground, ray from the transmitter to the receiver, and the indirect, or atmospheric, ray, which undergoes reflection and refraction in the ionised upper atmosphere. The use of various combinations of aerials rendered it possible to separate the many causes of fading, and to investigate their comparative importance. In this way it was proved that much of the fading arises from fluctuations in the intensity of the indirect ray, due to varying absorption, and that interference between the direct and indirect rays is responsible for fading over distances sufficiently short to produce a ground-wave at the receiver. The rotation of the plane of polarisation of the indirect ray was found to be a prominent cause of fading during the sunset period, but the cause of such rotation has not yet been explained. Indirect rays arriving by multiple paths were evidenced in records showing a periodic type of fading superposed on the main intensity variations. This has thrown some doubt on the calculation of the effective height of the ionised layer by means of the angle of incidence at the receiver. No proof was obtained in support of the belief that fading can arise from fluctuations in the height of the layer. The apparent direction shifts, which were required to explain some of the records, had been previously ascribed chiefly to elliptical polarisation of the indirect ray, and very little, if at all, to actual changes in the lateral angle.

Experiments are being continued to investigate possible

seasonal effects in short-period fading, the fading characteristics of other wave-length ranges, the effect of the earth's magnetic field, the influence of the intervening distance, and other related factors.

A paper by Ivy Jane Wymore in the same issue of the Bureau's *Journal* describes some investigations into the effect of magnetic storms on long-wave transmissions (12,000–20,000 metres). Over distances of a few thousand miles the signal intensity was found to fall below normal for several days before the storm maximum, followed by an increase lasting for some days. Over distances of a few hundred miles the approach of the storm produced an increase in signal strength; the intensity fell below normal during the height of the storm, followed by a strong increase from two to four days after the storm.

To interpret the influence of a magnetic storm, or that of a solar eclipse, is a difficult task. There is little doubt that the main effect is a drastic change in the ionisation of the upper atmosphere, but beyond this speculation is necessarily rather vague.

Huxley Memorial Lecture, 1929 (E. M. C.)

Prof. Bower, choosing for the subject of this lecture "The Origin of a Land Flora" (1908–29), gave a short summary of his working hypothesis of 1908 and then reviewed some of the more important work—up to the present date—which touched various aspects of the hypothesis.

Dr. Lang's view that retention of the zygote might be a causal factor in the differentiation of the sporophyte and the gametophyte is regarded as requiring experimental proof.

His theory that the suspensor represents the base of a filamentous primitive stage is held to be very probable.

Dr. Church's Subaerial Transmigration is held wanting. There is no evidence that there ever was a continuous, unbroken ocean surface, nor that "large marine algæ are or even have been open to a land change"; nor does there seem any reasons why there should not be parallel evolution of a marine and of a land flora.

The suggestion of Prof. Fritsch that higher Isokont Algæ gave rise to the land plants, and in doing so were automatically wiped out, is also regarded as unacceptable, mostly on the ground that in none of the existing green algæ is there any sign of a postponement of meiosis on the division of the zygote.

Prof. Bower is of the opinion that the bridge between the algæ and the higher plants is as great as it was twenty-one years ago. It is a different story with the Bryophyta and

Pteridophyta. Here the remarkable discoveries by Lang and Kidston of Rhynia, Hornea, and Asteroxylon, fossil plants which undoubtedly were land forms, the sporophytes only of which are known, have changed the situation.

These plants show certain characters common to the last-mentioned groups, and also throw light on the possible origin of the root and the leaf, at least in megapyllous forms. The distal sporangia of these plants also help in the linking up of sporangia with the single terminal spore-capsule of the Bryophyte.

The Psilophytales therefore " close the gap between the Bryophytes and the Pteridophytes," and it is the earlier stages in the origin of the land flora that are missing.

The Work for Agriculture of Two Great Englishmen, by Sir John Russell (E. H. Gregory)

One of the most prosperous periods of British agriculture was between the years 1840 and 1870. During these years the production of the soil was increased between 40 and 50 per cent. At the beginning of the period the average wheat yield was hardly twenty bushels per acre, and at the end it was approaching thirty bushels. There were many reasons for this increase, but one of the greatest was the research work done with artificial fertilisers by Lawes and Gilbert. The work which these two Englishmen did for agriculture and what has been the outcome of such work, is most admirably set out by Sir John Russell, in his lecture, now published in pamphlet form, to the Cawthron Institute, New Zealand. Sir John is the present Director of Rothamsted Experimental Station, the oldest agricultural research station in the world, being founded by Lawes in 1843.

Recent South African Congresses

The Editor has received an interesting letter from Prof. H. B. Fantham, of the University of the Witwatersrand, in which he describes briefly the recent meeting of the British Association. He says: " Perhaps you would like some general impressions. There were 535 visitors, who arrived in four or five different ships. The meetings lasted about a week each in Cape Town and Johannesburg, with a day's stop at Kimberley (for the diamond mines) *en route*. The South African Association merged its annual session with the British Association, the South African President (Mr. J. H. Hofmeyer) giving his presidential address in Cape Town, while Sir Thomas Holland gave the British Presidential address in Johannesburg.

There were public symposia on 'Science of Industry' and on the 'Nature of Life,' the latter more philosophical than scientific. Many of the visiting scientists are now on tour in the country. Some have gone farther north than the Victoria Falls and are returning via Kenya. Many lectures are being given in various towns of the Union by the visitors. I think that many of the visitors were surprised at the progress, development, and amount of scientific research in South Africa, and they were certainly interested in and commendatory of the papers read by South African authors.

"Honorary degrees were given by the Universities of Cape Town, Johannesburg, and South Africa. Among biologists Profs. Nuttall, D'Arcy Thompson, and D. M. S. Watson received Doctorates of Science. Dr. Monckton Copeman gave very interesting and much-appreciated papers on the treatment of cancer.

"Dr. J. S. Haldane suggested impregnating the air in the Rand gold-mines with shale dust in order to reduce and then prevent silicosis; but there are serious practical difficulties.

"There was much critical discussion and divergent views on Miss Caton Thompson's report on Zimbabwe. Probably she has not been able to work long enough at the problem yet. I think that it is generally agreed that the meetings were very successful, and that both sides have learned much from the meetings. I was able to exhibit some new Protozoa to the visitors, and we also had a general parasitological exhibit in my Department."

Adder Blood (Norman Morrison, D.Sc., F.Z.S.)

Much research could be undertaken to correlate the chemistry of adder blood with that of adder venom. The chemistry of the latter has been fairly well worked out. It contains a high amount of protein in globulin and albumin form, and an important element in it is the metal—zinc—bound up in organic combination. The significance of its invariable presence has not yet been determined. Of adder-blood chemistry there is very little exact knowledge. Normally it has a hydrogen-ion concentration approximating to that of human blood, and its taste (which I have recently experienced with no ill effect) is slightly alkaline. The condition of the shed blood is suggestive of a sluggish and primitive circulation on the part of the adder. It coagulates immediately on exposure to air, and forms a reddish-purple clot from which a limpid yellow serum containing a lipochrome called luteine can be isolated in very small quantity. It is interesting to

note that this yellow substance is identical with a yellow pigment found in human blood serum, in the yolk of the egg, in the corpus luteum of the ovary, in carrots, turnips, and is closely associated with the pigment called lycopin found in the rind of the tomato. Lipochromes are widely distributed in nature, and their significance in plant and animal biology is being gradually and surely interpreted by the competent methods of modern biochemical research. It is quite possible that some toxic substance is produced from lutein to collaborate with the zinc and protein fractions of the venom to achieve the sinister purpose of the reptile.

Such work as a determination of urea and uric acid content of the blood could be undertaken with fruitful interpolative results. It is a well-known fact that during certain months of the year the adder undergoes an automatic process of hibernation. The creature is then, to all intents and purposes, suffering from an almost complete suspension of vital activity. It is comatose, and in this condition there is a sudden onset of profound metabolic change. All secretory glands suddenly diminish their productiveness, neuro-thropic and neuro-tropic mechanisms are rendered inert, there is a stasis, or slowing down of the blood circulation, and the skin of the creature becomes a dark and loose-fitting blanket for it to sleep in comfortably during its enforced period of rest. This blanket is conveniently discarded after its emergence from its winter sleep. In view of the drastic change of condition consequent on hibernation, the adder makes special provision beforehand to maintain a metabolic equilibrium. In the immediate pre-hibernative stage it becomes noticeably increased in size, this increase being due to the laying down of fat, not only in the superficial tissues under the skin, but also in the intestinal coverings and especially in the liver. On dissecting an adder at this period the coils of the intestine present a fatty appearance, and the fat has darkened to a yellow brown colour. There is, further, an obvious increase in the size of the liver, although on visual examination the fat stored in it cannot be seen. In this organ, which is the central laboratory of the body, the fat is stored up in microscopic particles and is known as "masked fat." It is conveyed to the liver from the food taken after it has been subjected to a disintegrative process at the wall of the intestine. The fat of the animal consumed is broken down by digestive enzymes found in the intestine until the final products are glycerine and fatty acid. These products permeating the wall of the intestine are coupled up again and carried direct to the liver where they are ultimately changed into a fat characteristic of the snake. The liver is thus an adjusting mechanism and controls the blood chemistry

of the creature during its period of inactivity. A reserve of fat proteins and carbohydrates stored in the liver must meet the needs of a lowered metabolism during the stage in which the creature cannot fend for itself and the utilisation of these essential food principles is carried out in an orderly fashion. When one of the three is entirely used up the others, in virtue of their interchangeability, are summoned to the breach. This change in the co-ordinate mechanism of the liver necessarily involves a change in the blood chemistry of the adder. It is possible that fats and cholesterol circulating in the blood are decreased and that the same applies to uric acid and urea. There must be a remarkable conservation of energy, since actually an increase of about half an inch in a young adder born and kept in captivity for a period of over four months without taking a particle of food has come under my observation. Adders will not feed in captivity.

A New Pest threatens Australia (A. M. B.)

Flies are hardly considered dangerous animals, on account of their insignificant size ; nevertheless, they are grisly opponents which are proving one of the greatest menaces to life, human and otherwise.

The latest reports from Australian cattle-raisers have given rise to considerable alarm with regard to the future of the great beef industry of the Commonwealth. A little-known fly, called the Buffalo Fly (*Lyperosia exigua*, from the Greek, meaning " troublesome "), is rapidly spreading through the chief cattle-rearing areas, and making it impossible to raise fat cattle for slaughter during certain seasons.

This pest attacks the flesh at the base of the horns, or any injured part of the skin, sucking the blood, and causing intense irritation. The tormented cattle get no respite day or night, and loss of all rest reduces their condition until they become as ill-favoured as Pharaoh's lean kine.

It is thought that this winged enemy will prove the greatest factor in limiting the output of beef from the northern areas.

The Buffalo Fly came originally from Java, where the cattle appear less sensitive to its ravages, and until recently it was unknown in Australia, save in the northern coastal regions. To-day, it is rapidly spreading south. It has entered Queensland from the east, and has already reached west ; Wyndham is extremely badly infested, and recent reports state that it has even come so far south as Fremantle, with the cattle boats.

This creature is related to the Tsetse Fly, the dreaded carrier of Sleeping Sickness, being similar in habits and appear-

ance, and belonging to the blood-sucking family. The only practical possibility of controlling the Buffalo Fly that so far has presented itself, is by the introduction of its parasites and predators, who, it is hoped, will make life as burdensome for the Buffalo Fly as this pest has for the cattle. To this end several scientists, in connection with the Council for Scientific and Industrial Research of Australia, are at work investigating its life and habits.

American Inventiveness (R. R.)

The Americans are always very fertile in ideas, but whether the ideas are always sound is another question. A man I once met on a steamer going to New York discovered a wonderful thing which would enable us to do away entirely with the engines of steamers, coal, and any form of motor power. His idea was merely to make the ship's lines in such a way that the weight of water at the stern would push it forward. Unfortunately, he could not afford to make the requisite models. Now I have received another notion in the form of an advertisement regarding a book written by Charles E. Neupert, price $1.50. We can have our outlook upon everything completely changed for this small sum. It appears that everyone has been quite mistaken regarding the entire universe for a long time. What we look at in the sky, whether sun or stars are present, is not the sky at all but the inside of a vast spherical shell which encloses these absurd heavenly bodies and the earth as well. It is certainly true, as the author thinks, that many scientific men are fools ; but, nevertheless, we hope that some of them will really set to work to establish Mr. Neupert's interesting theories. What goes on outside this spherical shell is not discussed by him. Whether the shell is like that of a nut or is composed of some sugary substance like that of certain sweets eaten by ladies, is not known as yet for certain. Why not find out ? All we need do is to inquire of some of our numerous spiritualists who are so familiar with our still more numerous ghosts as to *their* experiences on the point. Doubtless the ghosts will be swarming just inside the shell in the hope of finding their way out, like flies in a bottle, and should know all about it—a much-neglected method of solving many scientific difficulties.

Notes and News

The President and Council of the Royal Society have, with the approval of H.M. the King, awarded Royal medals to Prof. J. E. Littlewood for his mathematical work, and to Prof. R. Muir for his contribution to the science of immunology.

The Copley medal has been awarded to Prof. Max Planck, of Berlin; the Davy medal to Prof. G. N. Lewis, of California, for his work on thermodynamics; and the Hughes medal to Prof. Hans Geiger, of Kiel, for his work on radioactivity.

Sir Ernest Rutherford has been re-elected President of the Society. Lord Rayleigh has been appointed Foreign Secretary, and Dr. H. H. Dale and Dr. F. E. Smith secretaries.

The Nobel prize for physics for 1928 has been awarded to Prof. O. W. Richardson, King's College, London, for his work on the emission of electrons from incandescent bodies. For 1929 the prize goes to the Duc de Broglie, whose paper, *A Tentative Theory of Light and Quanta*, published in 1924, formed a starting-point of wave mechanics. The prize for chemistry for 1929 has been divided between Prof. A. Harden, of the Biochemical Department of the Lister Institute, and Prof. H. von Euler-Chelpin, of Stockholm, both of whom have made notable contributions to our knowledge of fermentation.

The obituary list this quarter is much longer than usual. Among many well-known names are the following: Karl Auer, Ritter von Welsbach, inventor of the incandescent mantle; Mr. A. Berry, mathematician and Vice-Provost of King's College, Cambridge; Dr. E. S. Bieler, Assistant Professor of Physics in McGill University; Dr. T. J. I'. A. Bromwich, F.R.S., mathematician; Prof. L. H. Cooke, Professor of Mine-surveying at the Imperial College of Science; Dr. E. B. Craft, of the Bell Telephone Laboratories; Dr. W. G. Duffield, Director of the Australian Commonwealth Solar Observatory; Prof. F. A. Gooch, chemist, of Yale University; Mr. W. Heape, F.R.S., embryologist; Prof. J. S. Kingsley, zoologist of the University of Illinois; M. Auguste Lebeuf, of Besançon, astronomer; Dr. G. P. Merrill, of the U.S. National Museum, geologist; Prof. C. Moureu, Professor of Organic Chemistry in the Collége de France; Prof. W. H. Perkin, F.R.S., Waynflete Professor of Chemistry in the University of Oxford; Prof. R. P. Pictet, of Geneva, famous for his work on the liquefaction of gases; Mr. H. C. Robinson, lately director of the Federated Malay States Museums; Prof. S. B. Schryver, F.R.S., Professor of Biochemistry at the Imperial College of Science; Dr. E. E. Slosson, Director of Science Service, Washington; Sir W. Baldwin Spencer, F.R.S., anthropologist; Prof. R. Zsigmondy, N.L., Professor of Inorganic Chemistry in the University of Göttingen.

Prof. E. A. Sharpey-Schafe has been elected President of the Royal Society of Edinburgh; Prof. R. A. Sampson is the General Secretary of the Society.

Lord Rayleigh, Sir Arthur Balfour, Sir William Bragg,

and Sir James Walker have been appointed to be members of the Advisory Council to the Committee of the Privy Council for Scientific and Industrial Research in the places of Sir Harold Carpenter, Dr. G. C. Clayton, Sir Richard Glazebrook, and Sir James H. Jeans, who have retired from the Council on completion of their terms of office. Mr. F. E. Smith, Director of Scientific Research at the Admiralty has been appointed to be Secretary of the Committee. Dr. C. V. Drysdale succeeds Mr. F. E. Smith at the Admiralty.

Lord D'Abernon, Major A. G. Church, Prof. J. J. R. McLeod, and Mr. Wilfred Trotter have been appointed members of the Medical Research Council in succession to Lord Balfour, Sir Charles Trevelyan, Prof. E. P. Cathcart, and Sir Charles Sherrington respectively.

Mr. P. Barter, of the Ministry of Health, Whitehall, has been appointed secretary to the National Radium Trust.

An advance edition of eleven thousand copies of the catalogue of the London section of the British Industries Fair (February 17–28) will be ready on January 1, in time for delivery to most overseas buyers before they leave their home towns. The index is in nine languages. The arrangements at Olympia will be much more convenient than was possible at the White City; for example, no stand will be more than 130 yards from the nearest entrance, and no two sections are more than 100 yards apart, the corresponding figures last year were ½ mile and 1 mile.

The Secretary of State for Scotland and the Minister of Agriculture and Fisheries have appointed a Committee to investigate the occurrence of furunculosis and similar diseases among salmon, trout, and other freshwater fish in England, Wales, and Scotland. The terms of reference are as follows: "To investigate the origin, predisposing causes, and mode of dissemination of furunculosis and similar infectious diseases among salmon, trout, and other freshwater fish in England and Scotland, and to conduct experiments with a view to ascertaining methods of combating the diseases, and to report the results of their proceedings." The Chairman of the Committee is Prof. T. J. Mackie, M.D., Professor of Bacteriology, Edinburgh University; the other members are: Prof. J. A. Arkwright, F.R.S.; Mr. T. E. Pryce-Tannatt, M.B., D.P.H.; Mr. J. C. Mottram, M.B., D.P.H.; Mr. Douglas Johnston, O.B.E; and Mr. W. J. M. Menzies. The Secretary of the Committee is Mr. William Martin, of the Fishery Board for Scotland, 101 George Street, Edinburgh, to whom communications regarding the work of the Committee should be addressed.

The formation of the Committee is the outcome of the consideration given by the responsible Departments—the

Ministry of Agriculture and Fisheries and the Fishery Board for Scotland—to a series of epizootics of furunculosis among salmon and trout in Great Britain in recent years. These outbreaks have been sufficiently extensive to occasion considerable damage in some cases to valuable fisheries in the rivers affected. A smaller Committee has already made preliminary investigations and several reports have been issued on the scientific work which has been undertaken and on the specimens which have been taken in the areas of infection and examined. A most important result which has been established is that apparently healthy fish may act as carriers of furunculosis and the relationship of this fact to the incidence and spread of the disease among home and imported fish will form one of the considerations to be investigated by the Inter-departmental Committee which has now been set up.

Two committees are now at work on the arrangements to be made for the celebration of the Centenary of Faraday's discovery of electromagnetic induction (August 29, 1831). The first, consisting of representatives of the Royal Society, the British Association, and other scientific societies, as well as the Royal Institution, is concerned with the purely scientific aspects of Faraday's work in relation to the proposed celebrations; the second committee, which has been called together by the Institution of Electrical Engineers, consists of representatives of the principal organisations of those industries which have risen in the past hundred years upon the scientific foundation of Faraday's discoveries, and is dealing with the industrial aspects of the celebrations. The two committees are working in close co-operation; the preliminary discussions which have taken place indicate that the significance of the Centenary is very widely appreciated and that the celebrations are likely to arouse world-wide interest and support. The dates have been fixed, and the proceedings will commence in London on Monday, September 21, 1931. The Centenary Meeting of the British Association, which assembles in London for the first time, begins on September 23, so that delegates and visitors from overseas will be able to join in both meetings.

It is somewhat disappointing to read that the British Cotton Industry Research Association, at the end of ten years' work, is still in need of assistance from the Department of Scientific and Industrial Research. An income of £75,000 is required during the next quinquennium and towards this sum the trade can contribute only about £42,000. The D.S.I.R. has, therefore, agreed to give pound for pound on all income from approved trade sources over £25,000, so that the total income actually available will be some £60,000. If, in 1934, the industry is still unable to support its own Association it will be

hard to avoid the conclusion that it is also unable to make profitable use of the results obtained and the *raison d'être* of the association then disappears.

The authorities at University College, Cardiff, have initiated an interesting experiment by arranging a special course in General Science open to the public, and consisting of lectures delivered, on Saturday mornings during the Michaelmas and Lent terms, by well-known authorities on the college staff. The ten lectures to be delivered between January and March have a biological bias and conclude with a discourse on the "Reality of Human Experience," by Prof. Livens. A descriptive booklet contains a syllabus of each of the lectures and shows that a most attractive course has been arranged. A fee of 10*s*. 6*d*. per term may, perhaps, defeat the purpose of the lectures, which is, presumably, to attract the interest of people who would not otherwise enter the portals of the College.

The Committee of the Safety in Mines Research Board, which is studying methods of preventing accidents from falls of roof and side, has been carrying out, in addition to experimental work, a survey of colliery practice as regards the support of underground workings with a view to calling attention to the best methods and suggesting improvements where possible. The Committee have just published their fifth report (Safety in Mines Research Board Paper No. 55), dealing with "The Support of Underground Workings in the Coal-fields of Lancashire, Cheshire, and North Wales." The Committee's first four reports related to the coal-fields of South Wales, Scotland, the East Midlands and the South Midlands. They were not sent to us for review, and this is the first time we have had an opportunity of calling attention to them. They are intended for the instruction of *all* colliery workers, and to this end the report of ninety odd pages and many excellent diagrams is sold at a nominal price of 2*d*. Methods of working and of supporting the workings are described and criticised and detailed recommendations are made. In one of the appendices a number of accidents are described with notes as to ways in which they might have been avoided. A most admirable report.

The Research Board has issued another paper on the ignition of fire-damp, this time with special reference to ignition by sparks from picks. Earlier work, *e.g.*, by the French Fire-damp Commission in 1890, had suggested it to be exceedingly difficult, if not impossible, to ignite fire-damp by friction and frictional sparking occurs so often in practice without ill results that it has not been generally regarded as a serious danger. However, in a paper published last year Burgess and Wheeler showed that the heat generated by the impact of rock against

rock might be sufficient, in certain conditions, to ignite fire-damp. The experiments were continued, therefore, with a view to determining whether the heat of impact of metal against rock can cause ignition. Experiments of this kind could not be carried out in a mine; but after a number of laboratory tests, such as the abrasion of steel by a revolving carborundum wheel, full-scale tests were made with an ordinary chain coal-cutting machine working against a built-up mass of rock in explosive atmospheres. It was found that, with certain kinds of rock, fire-damp could be ignited quite readily though it is not certain whether the ignition was caused by the sparks emitted or by the heating of the rock surfaces by the impact of the coal-cutter picks. The Report contains an historical account of early views and experiments on frictional spark ignition, and it is interesting to note that Volta was the first (1777) to make experiments and to point out the danger of the steel mills then used for creating showers of sparks for illuminating purposes in mines where naked flame-lamps would cause explosions.

A complete survey of the Board is given in the Seventh Annual Report, including an account of the steps taken to disseminate knowledge of the work of the Board and its results among miners and colliery officials.

Methuen's *Monographs on Physical Subjects*, edited by Dr. Worsnop of King's College, London, and sold for the modest sum of 2*s.* 6*d.* net, are supplying a very definite want in the literature of Physics. The six volumes so far issued or in the press deal with *Wave Mechanics* (Dr. H. T. Flint); *Spectra* (Dr. R. C. Johnson); *The Physical Principles of Wireless* (J. A. Ratcliffe); *The Conduction of Electricity through Gases* (Dr. K. G. Emeléus); *Magnetism* (Dr. E. C. Stoner); and *X-rays* (by the Editor); the two last mentioned being still in the press when this note was written. They are written primarily for the information of Honours Students and of those who are interested in the modern developments of physics, but have neither the time nor the specialised knowledge required to study original papers or more detailed (and more expensive) monographs. Dr. Flint's book is the only serious account of its subject known to the writer suitable for students' use. It contains a tempered account of the mathematics of the subject, a description of the work of Davisson, Farmer, and G. P. Thomson, and a most helpful discussion of the significance to be attached to the wave function. In an introduction to Mr. Ratcliffe's book, Prof. Appleton says that "In spite of the limited space at his disposal, Mr. Ratcliffe has succeeded in giving a wonderfully clear and complete account of present-day problems, both solved and unsolved," and there is no exaggera-

tion in this statement. Anyone not frightened by a square root or a $\sin \omega t$ who desires to acquire a real knowledge of the fundamentals of wireless practice cannot do better than read the book. Dr. Emeléus gives a succinct account of the phenomena of the discharge tube, and these, of course, are chiefly of interest to the student of physics. The volume on *Spectra* has been reviewed elsewhere in these pages.

From Macmillan's list of forthcoming books we see that the third volume of the *Lectures on Theoretical Physics*, by Lorentz, is in the press. It deals with the special theory of relativity. Sir Richard Paget has written up his work on *Human Speech*, and it is to be published by the Broadway House Press (Routledge & Kegan Paul, Trench & Trübner). Prof. Sir C. V. Raman is writing a book on the *Scattering of Light*, which will be published by G. Bell & Sons. There is at present no comprehensive account of this subject in English.

Bulletin No. 71 of the National Research Council (Washington, D.C., price $1.50) is a first supplement to the *Bibliography of the Bibliographies on Chemistry and Chemical Technology*, published in *Bulletin* No. 50. That *Bulletin* covered the period 1900–19–24 and the supplement deals principally with the period 1924–9. It contains a list of 4,10c bibliographies classified under 1,050 headings. The bibliographies themselves have been published as separates, or at the end of books or magazine articles, or as footnotes to such articles, and deal with the numerous aspects of pure and applied chemistry. Each entry gives name of author or compiler, title, and place of publication. The majority of the entries state the number of references, thus giving an indication of the completeness of the particular bibliography. The entries are classified under the proper subject-headings, alphabetically arranged. The duplication of individual entries has been largely avoided by the liberal use of cross references.

In the *Journal* of the Council for Scientific and Industrial Research of the Commonwealth of Australia, in a paper, " Blue Mould of Tobacco: Investigation Concerning Seed Transmission," by Dr. H. R. Angell, some experiments are described which seem to demonstrate that the disease is due to diseased seed, and is not transmitted by air-borne conidia. Infected seedlings are produced from diseased seeds from which the disease spreads rapidly. It is not yet known how the disease is carried by the seed, but it is worthy of note that soaking the seed in alcohol for three minutes appears to be effective in eliminating the disease. This method of control, as well as others, may be found to be reliable.

In the *New Zealand Journal of Science and Technology* for June 1929 a paper, " Apple Rot Fungi in the Overseas Trans-

port and Marketing of New Zealand Apples," by Mrs. M. N. Kidd, described experiments which were carried out to study the effects of temperature inequalities in refrigerated holds by the method of split consignments. Nine experiments were conducted on eight vessels, and three varieties of apple were used: namely, Cox's Orange Pippin, Jonathan and Sturmer Pippin. These were surveyed with regard to the fungi responsible for the wastage at the time of discharge and several weeks after discharge. It was found that Cox's Orange Pippin showed the most waste and Jonathan the least. The greatest amount of waste, 40 per cent. in the case of Cox's Orange Pippin, is caused by *Diaporthe perniciosa*, and in the case of Jonathan and Sturmer Pippin by *Physalospora Cydoniæ*. It was shown that wound and stem infection predominated, and it is suggested that there should be an increase in spraying procedure among growers shipping their fruit to the United Kingdom.

ESSAY-REVIEWS

FUNGI. By FREDERICK REISS, M.D., Asst.-Professor of Dermatology, National Central University College of Medicine, Shanghai, China. Review of **Fungi and Fungous Diseases** by Sir ALDO CASTELLANI, K.C.M.G., D.Sc., M.D., F.R.C.P., Professor of Tropical Medicine, Tulane University, New Orleans, Director of Tropical Medicine, Ross Institute, London, etc. [Pp. 203, with 44 figures.] (Chicago: American Medical Association.)

THE monograph is introduced by a preface from D. J. Davies, Dean of the College of Medicine, Illinois University, explaining that the monograph is based on a series of lectures given by Prof. Castellani in accordance with a lectureship founded by Mrs. Albertina Gehrman, widow of the late Dr. Adolph Gehrman, who for many years was Professor of Bacteriology and Hygiene at the University of Illinois. Prof. Gehrman was an untiring worker, and always interested in active investigation. To inaugurate the lectureship, the University was most fortunate in securing Prof. Castellani, who is regarded as a world authority on fungous diseases and dermatology.

The monograph is divided into three parts: (1) General Classification of Fungi; (2) Internal Diseases due to Fungi; (3) Skin Diseases due to Fungi.

In a short historical introduction a brief review is given of the development of mycology, the study and investigation of the fungi, *sensu stricto*, which are important in nature, not only for their evil effects, but also for the beneficent functions they may perform.

Although the study of Bacteriology, or lower fungi (Schizomycetes) seems to gain more and more importance, it is interesting to note that mycology came into existence long before bacteriology.

It began in the days of Charles II, when Hooke, in 1677, constructed a magnifying-lens and described the yellow spots on the leaves of the damask rose caused by fungi. After him Malpighi devoted a paper to the genus "Mucedo" (1686). Ray published his *Historia Plantarum* in 1706, where he describes the *Pilobolus crystallinus*. Micheli, in 1729, gave an account of "Aspergillus," and finally Linnæus wrote his famous work, *Species Plantarum*, in 1753. The names of

Persoon, Link, Kützing, Fückel, Naegeli, Saccardo, of the eighteenth and nineteenth centuries, finally Zopf, Engler and Prantl, Vuillemin, Polacci, and Nannizzi of our days are mentioned.

Medical Mycology did not interest the scientist until the beginning of the last century, when Langenbeck discovered in 1839 the thrush-fungus, the knowledge of which was greatly increased through the investigations of Charles Robin (1853), who called it *Oidium albicans*. In the same year the fungus of favus was found by Schönlein, which was further investigated by Lebert, who called it *Oidium schoenleini*, but subsequently Remak named it *Achorion schoenleini*. In 1843 Gruby discovered the fungus of ringworm and in 1846 Eichstedt the fungus of *Pityriasis versicolor*.

With the great discoveries of Pasteur and Koch in the field of Bacteria mycology was placed in the background, where it still remains; but there is no doubt that the importance of mycology will be recognised, as more than 20 per cent. of tropical diseases are caused by fungi, and moreover diseases due to fungi are quite numerous in the Temperate Zone.

In the "General Remarks" the elementary principles of mycology are explained, there follows the "Classification of Fungi."

The fungi are separated into two divisions: Myxomycetes and Eumycetes; the latter, being of no importance in human pathology, is entirely omitted, while the Eumycetes are subdivided into four classes: (I) Fungi imperfecti, or Hyphomycetes; (II) Ascomycetes; (III) Phycomycetes; (IV) Basidiomycetes.

Class I. The Fungi imperfecti, which are of the greatest importance in human pathology are subdivided into (*a*) *Deuteromycetes* Saccardo and (*b*) *Hyphales* Vuillemin, the latter group contains most of the fungi parasitic in man.

The Hyphales contain the following orders: Microsiphonales, with the family Nocardiaceæ Castellani and Chalmers (type genus *Nocardia* Toni and Trevisan; Mycobacteriæ, with two genera: (1) *Mycobacterium* Lehmann and Neumann, with the tubercle bacillus as a type, and (2) Corynebacterium Lehmann and Neumann with the diphtheria bacillus as a type.

In the family Nocardiaceæ the genus *Conistreptothrix* Pinoy is dealt with as a separate genus as it grows best or solely anaerobically, and is usually difficult to cultivate, in contrast to the Nocardia, which grows aerobically and is easy to cultivate. To this family belong the following genera: *Leptothrix* Kützing, *Cladothrix* Cohn, and *Vibriothrix* Castellani, a polymorphous, motile, gram-negative not acid-fast organism which

has been isolated by Castellani from cases of dysenteric stools in Ceylon.

The Second Order, Thallosporales, contains two sub-orders: (*a*) Blastosporineæ Vuillemin, which contains five principal families: (1) Cryptococcaceæ Kützing, (2) Oösporaceæ Saccardo, (3) Enantiothamnaceæ Chalmers and Archibald, (4) Haplographiaceæ Saccardo, (5) Cladosporiaceæ Saccardo. It is noteworthy that the Trichophytons are omitted from this order. Special consideration is given to the *Monilias*, which have been extensively studied by Castellani and whose fermentation methods of various carbohydrates is still the only way of differentiating the various species.

The second sub-order, Arthrosporineæ Vuillemin, contains the genera *Trichosporum* Bhrend, *Madurella* Brumpt, and *Indiella* Brumpt.

The Third Order, Hemisporales, contains one genus Hemispora with two species: *Hemispora asteroides* and *H. rugosa*.

The Fourth Order, Conidiosporales Vuillemin, is divided into fungi with imperfect conidium (Aleuriospore) and with perfect conidium: (1) True conidiophores absent: Sporotrichineæ Vuillemin; (2) True conidiophores present: Sporophoralineæ Vuillemin, Phialidineæ Vuillemin, and Prophialidineæ Vuillemin.

The second class, Ascomycetes, is divided into two subclasses: Asci separate or scattered, Protoascomycetes and Asci approximate, usually forming a hymenium: Euascomycetes. The Protoascomycetes contain a single order, the Sacharomycetales, with one family, Saccharomycetaceæ, and the following genera: *Saccharomyces* Meyen, *Debaromyces* Klocher, and *Willia* Hansen. The family Endomycetaceæ contain only two species parasitic in man: *Endomyces* Rees and *Coccidioides* Rixford and Gilchrist; but as the fungi of this group do not produce true ascospores, Castellani suggests that they should be placed in a separate genus: Blastomycoides. In the sub-class Euascomycetes is described in a very detailed manner, the family Gymnoascaceæ Baranetzky, which is divided into two tribes: Ascomycetes, type Gymnoasceæ Castellani and Chalmers, and Fungi imperfecti, type Trichophytoneæ Castellani and Chalmers, which contains the genera *Microsporum* Gruby, *Tricohyton* Malmsten, *Neotrichophyton* Castellani and Chalmers, *Ectotrichophyton* Castellani and Chalmers, *Atrichophyton* Castellani and Chalmers, *Achorion* Remak, *Epidermophyton* Lang, *Endodermophyton* Castellani and *Pinoyella* Castellani and Chalmers. A brief reference is made to the new classification of Ota and Langeron, who emphasise the point that the usual classification of these fungi is based more or less on the clinical lesions, which is not satisfactory, and propose a new classification based on botanical characteristics.

In the family Aspergillaceæ the genera Penicillium, Aspergillus, and Sterigmatocystis are briefly dealt with. In the third class, Phycomycetes, only the family Mucoraceæ are parasitic in man.

A very useful key is given for clinicians and clinical pathologists for the classification of fungi according to their biological characters. In this direction very important and, so to say, pioneer work has been done by Castellani. With the help of his "Symbiotic fermentation method," quite a good number of micro-organisms can be differentiated; furthermore, a very simple and useful method is given for identifying various carbohydrates with the help of the fermentative character of certain fungi and bacteria. The second part of the volume deals with fungous diseases in man, which comprise, according to Castellani, about 20 per cent. of all tropical diseases.

A very clear description is given of the various Tonsillomycoses, the chronic forms of which have been studied by various other authors, but so far little has been known about the acute types, as for instance tonsillomoniliasis, which has been precisely and clinically defined by the author. The various Bronchomycoses are illustrated with a fair number of clinical examples, which helps the reader to clearly understand a not very well known part of tropical medicine. The chapter deals, furthermore, with the mycoses of the nervous system and organs of special sense, as well as with urethromycoses, a subject about which very little was known before Castellani's investigations.

Finally, in a very comprehensive way are described the various Dermatomycoses and Trichomycoses, several of which have been differentiated by Castellani, *e.g. Trichomycosis flava, rubra*, and *nigra*.

A very interesting clinical picture is described under the name of "furunculosis cryptococcica," which results in a kind of baldness similar to *Tinea decalvans tropicalis*, but whereas the cause of the latter is the "Trichophyton decalvans" the "furunculosis cryptococcica" is produced by a yeastlike fungus, either Cryptococcus or Monilia.

A very peculiar type of dermatosis is the *Cryptococcosis epidermica*, which has been differentiated through the investigations of Castellani from the ordinary Dermatophytic affections.

Two common diseases, *Tinea flava* and *Epidermophytosis inguinalis* are treated *in extenso*, as well as *Tinea imbricata* and *cladiosis*, a group of tropical dermatoses, which are well known only since the investigations and clinical studies of Castellani.

Fungi and Fungus Diseases is undoubtedly a very valuable contribution to medicine in general and to tropical medicine

in particular, which will be most especially appreciated by practitioners and pathologists in tropical countries, who will find it not only a great help in daily practice, but a guide in the most complicated questions on mycology.

THE SCIENCES AND THE PHILOSOPHIES. By J. H. Woodger. Being a review of **The Sciences and Philosophy**, by J. S. Haldane, C.H., M.D., F.R.S., etc. [Pp. ix + 344.] (London: Hodder & Stoughton, Ltd., 1929. Price 15*s*. net.)

Since the time of the Reformation one of the most remarkable features of the history of the people of Europe and North America has been the way in which their interests, pursuits, and opinions have continued to multiply, and have given rise to a corresponding multiplicity of animosities and antagonisms which seems to grow worse with the passage of time, until one wonders where the process will end, and whether what is called Western civilisation will not finally go to pieces from the sheer inability of its members to pull together. This state of affairs is not confined to the national conflicts which have now reached such catastrophic dimensions as to demand urgent attention and intervention, but extends to human activities whose consequences are, in the long run, no less potent for good or evil in actual life. We see precisely the same situations in the splitting up of religious sects, in the clash of æsthetic and commercial interests, and in the competition of rival scientific theories. The amount of effort expended in propaganda in the interests of one faction or another at the expense of their competitors is prodigious, but no common purpose or co-operation seems to be achieved in this way. Fear seems to be the only means of bringing about any cessation of these conflicts, but union reached in this way lasts only as long as the fear itself endures.

This complex state of diversity of aim is discernible with special clearness in that particular interest which attempts to take a general view of the innumerable departmental ones, namely speculative philosophy. If, for example, we compare the various Gifford Lectures of recent years we find an extraordinary divergence of opinion regarding the interpretation of natural science, and the relation of scientific pursuits to such others as politics, art, religion, etc. Moreover, the different lecturers entertain quite different opinions even about the scope and method of philosophy itself. Dr. Haldane calls his book *The Sciences and Philosophy*, recognising that there is no single unitary entity that can be called science, but rather a number of sciences. But he does not seem to acknowledge the fact that the same is true of philosophy, since it is equally true that there are a number of philosophical

sciences, just as there are a number of natural sciences. Dr. Haldane appears to give the name "philosophy" to one of these, namely metaphysics. But he does not appear to do justice to the fact that there are also a number of rival metaphysics. He apparently wishes to persuade the reader that there is only *one* metaphysic, namely his own, and it is to this that he gives the name of Philosophy. And he evidently believes that if only people would adopt his particular theory of the world all our troubles would cease, and we should all live happily ever after. This is probably correct in so far as men would have less to quarrel about if there were only one system of beliefs on the sort of topics commonly discussed by Gifford Lecturers. But such a state of affairs would, it seems, only be reached if there were a system such that everyone was capable of understanding it, and its intrinsic merits so far and so palpably transcended all others that it had only to be understood to be adopted. Now there appear to be two quite different kinds of metaphysics. Some philosophers have tried to discover what propositions are certain and are agreed to by all men, and have then attempted to infer, from these, equally certain propositions about the nature of existence. This method is not so popular now as it was formerly, because it is not generally held to be possible to decide between the many possibilities which thought presents. Another method is to proceed, like the natural sciences, by induction and by sticking together all the fragmentary and tentative "conclusions" of the various departmental sciences. But this method can only give probability, not certainty, and therefore cannot hope to *compel* acceptance. Moreover, it depends upon "sampling" and therefore can never be "finished" because it is impossible to know that all future samples will conform to past ones. There is always the possibility of surprises. But both of these methods are one in so far as they appeal, or aim at appealing, to data and inference and not simply to beliefs which have to be taken on trust in spite of the fact that they can be doubted. Now Dr. Haldane does not consistently pursue either of these methods. All through his book he says that this or that doctrine which he wishes to urge upon his readers can only be adopted by "an act of faith." If this were all there would be nothing more to be said, because there clearly is no scope for discussion. If anyone says: "I am going to believe that A is B, whatever happens," this means that he is taking this belief to be fundamental and not capable of further analysis or debate. If he wants to urge this belief upon others this can only be done by showing them its intuitive rightness (assuming that it is incapable of being deduced from some other beliefs). But the trouble is, of course, that it is

just here that we find the root of all disagreement. What to Dr. Haldane *must* be accepted by an act of faith will seem to many people very doubtful indeed. If Dr. Haldane proposes to adopt this method it is difficult to understand how he can deny the right of others to do the same with *their* beliefs. But in dealing with other people's beliefs Dr. Haldane pursues quite a different method, namely, a *logical* method resting ultimately on the law of contradiction. Because these other beliefs clash with his own Dr. Haldane dismisses them. But clearly his equally clash with those others, and hence people who believe those others are equally entitled to dismiss Dr. Haldane's. Which is adopted depends entirely on your choice of starting-point, and this, according to Dr. Haldane, can only be settled by an act of faith.

Dr. Haldane believes that the salvation of the world is to come through philosophy and it is the business of the latter to "enable us to frame as consistent as possible a working conception, not merely of part, but of the whole of our experience." Many people would deny all these assertions. Some might say that the salvation of the world cannot wait until "philosophy" has finished a task which in its very nature is without end. Some would point out that innumerable exemplary lives have been lived without any attention to consistency or working conceptions. Still others might urge that in any case there is no such thing as "our experience," and that instead each person has his own experience, so that the longer the splitting-up process continues the more diverse and restricted these individual experiences become, and the more rival "conceptions" we have to deal with. Now Dr. Haldane devotes the first part of his course of lectures to an examination of the three chief schemes of interpretation which have so far been developed in the sciences—the physical, the biological, and the psychological. He has no difficulty in showing that no *one* of these is capable at present of framing a "working conception of the whole of our experience." But this is hardly surprising since none of them was originally devised with any such intention. Each has developed on account of its success in dealing piecemeal with a deliberately restricted sphere, hence it is hardly to be expected that one should take the place of all. It is only the deep-seated monistic urge of mankind which leads people to take one of these schemes and turn it into a philosophy *überhaupt*. And this is also what Dr. Haldane wishes to do. He does not think that the physical or the biological schemes can do this, but he does think that the psychological one can; one reason for this being that the former two schemes are "abstract," whereas the last is not, or at least is less so than the others. But Dr. Haldane does not seem

to realise that abstraction is inseparable from the procedure of thought. He seems to think that abstraction is in some way necessarily fallacious and imperfect, and he labels abstractions as "unreal." But abstractions are only fallacious if they are misused, and the abstractions of psychology seem to be just as liable to misuse as those of physics or biology, and there does not seem to be any especially compelling reason for regarding them as "more real" or "nearer to reality" than those of the other sciences. Any "working conception," even if it is "of the whole of our experience," will still be abstract, and therefore according to Dr. Haldane unreal; it will not *be* that experience itself, otherwise it would not be a conception, *i.e.* knowledge. What Dr. Haldane wishes to do apparently, in common with a great many other philosophers, is to mix up the activity of the intellect, with its special peculiarities, with other activities—such as the æsthetic and the religious—which have quite different characteristics and different aims. A botanist and an artist both looking at a landscape may have the same or similar sensory experience, but one proceeds to scientific knowledge and the other to æsthetic creation. There does not seem to be any means of bringing them down to a common denominator. There might be a "conflict" between them if the botanist wanted to chop down the trees, but this would not be a *logical* conflict, it would be a conflict of interests, not of the outcome of those interests. Yet in dealing with the various clashes of human beings Dr. Haldane does not seem to distinguish between these different types, nor does he seem to preserve a consistent attitude towards science. When he is defending his own beliefs against scientific theories he treats the latter as "abstract," etc., but when he wants to criticise other people's beliefs he *uses* scientific arguments against them. Thus, in reference to the "many examples of men who have been eminent in natural science, and who have nevertheless remained quite orthodox members of various religious bodies," Dr. Haldane says that "one cannot help feeling that they are abnormal persons who keep their science in one part of their minds and their orthodoxy in another." This passage seems to suggest that "science" and "orthodoxy" are in some way incompatible, so that *unless* one keeps them in separate "parts" of one's mind one is bound to realise their incompatibility. But on Dr. Haldane's view of "science" it is difficult to see how this can be the case, because according to that view science is committed to abstractions which are merely "useful" and not in any sense to be interpreted realistically. Consequently it is difficult to see how the propositions of science can conflict with orthodoxy any more than with the metaphysical doctrines which Dr. Haldane is so anxious to

defend against them. And yet it is to this same scientific knowledge that Dr. Haldane appeals for arguments against "orthodoxy" and supernatural beliefs. Is it consistent to appeal to science in this way in one part of the book and to argue against it in another? Surely if we are to treat science on pragmatist principles we must do so consistently throughout, and if we do this we cannot then appeal to it to furnish arguments against one set of religious beliefs rather than another. And yet, in spite of his frequent appeals to acts of faith, Dr. Haldane is always condemning this or that doctrine on grounds of consistency. But he seems to apply the adjective "consistent" in a way peculiar to himself. Thus he says that what is called "physical" cannot be "real" because otherwise "the universe would not be consistent with itself." But what can it possibly mean to say this? One can only speak significantly of consistency with reference to propositions, and surely Dr. Haldane does not mean that the different constituents of our world are propositions. Thus language not only enables us to communicate with one another, but also tempts us to join words together in meaningless combinations.

Returning to the differences between the intellectual and other human activities, and to Dr. Haldane's view of the business of philosophy, it seems clear that if metaphysics is to be a branch of intellectual inquiry, issuing in propositions and resting on the law of contradiction, it is essential that it should be conducted impartially, just as in any of the special sciences. To conduct such investigations with a view to making the result "come out right" from the standpoint of some one particular interest is rather like cheating at cards or playing with loaded dice. The natural sciences have been successful in proportion as they have taken care to free themselves from the embarrassments and biases of non-intellectual interests. But Dr. Haldane says he identifies philosophy with religion, and in that case it cannot be a branch of intellectual inquiry, which attempts to be unbiased, at all, and consistency and other truth functions cannot be demanded of it. Idealist philosophers of all kinds persist in muddling up the intellectual activity with the others, to the great detriment of them all, and metaphysics then ceases to be what William James said it was, "An unusually obstinate effort to think clearly." Instead it becomes a medium for the expression of one's feelings which may or may not be art, but certainly is not knowledge, nor will it furnish "consistent conceptions." That Dr. Haldane does not attempt to distinguish these things is clear from the fact that he speaks of "the logical judgment which embodies artistic perception." What has artistic perception to do with *logical* judgment? Would it not be manifestly

absurd to apply logical canons, say, to a poem of Keats? Similarly, Dr. Haldane refers to faith as an *inference*! What is the use of different words for different things if they are to be treated as interchangeable?

Dr. Haldane's use of the term "philosophy" for his own particular metaphysical beliefs seems to be most deplorable, and likely to give the scientific reader a most erroneous impression, which will have an effect diametrically opposite to that which Dr. Haldane is anxious to give. All through the book he repeatedly asserts old dogmas as though they were not at all matters of debate, and without any real effort to meet modern criticisms of them, *e.g.*: "There is no such thing as a physical world existing apart from consciousness." Similarly, it is extremely misleading to the uninitiated reader to say: "For philosophy the immensities of space and time are not outside, but within us." We again have "philosophy" in the singular when it should be "some philosophies" in the plural. And in any case what does it *mean* to say that the *immensities* of space and time are *within* us? Clearly the terms "immensity" and "within" cannot be used here in any spatial sense, and without further explanation we are left in complete darkness by this oracular assertion. Indeed Dr. Haldane's various assertions about space and time are very inconsistent. On p. 189 he says that space and time are "only the order" in which "spiritual reality expresses itself." On p. 138 we read that "From an analysis of conscious experience he [Kant] drew the conclusion that time can only be a form in accordance with which, owing to the nature of Mind, our conscious experience is arranged." Again, on p. 295 it is stated that time and space represent only "the unlimited scope of the ideas through which we have ourselves ordered our experience." It is rather difficult to reconcile these assertions, but it is very difficult indeed to reconcile any of them with what Dr. Haldane says in other places, when he wishes to *use* the notions of space and time and not merely to explain them away. For example, he wishes to base a distinction between the biological schemes of interpretation and the psychological on the alleged fact that time-relations are especially important in the latter and not in the former. He says (p. 146): "Since interest and values are expressed in relations of time as well as of space, psychology is distinct from biology, and must be regarded as an independent branch of knowledge or science." How are we to interpret these statements in the light of the above assertions of the status of time? And in any case this is a curious way of distinguishing between psychology and biology, because an organism conceived purely spatially in abstraction from time is a monstrosity not to be found in

nature, but only in textbooks of anatomy. Dr. Haldane himself points out on p. 327 that "From the biological standpoint life is continuous in time, and represents something inherent in the very existence of Nature." But Dr. Haldane does not bring these assertions into relation with the statement that the immensities of space and time are within us. Moreover, it is incorrect to say that Kant reached the above position "from an analysis of conscious experience." He reached it in quite a different way. He believed that there were such things as "synthetic *a priori* judgments," *e.g.* in pure mathematics, and in order to explain how this was possible he was driven to suppose that space and time were "forms of pure intuition *a priori*." But it is now believed by mathematical logicians that Kant was mistaken about the nature of pure mathematics and about his synthetic judgments *a priori*, and, in consequence, this part of his doctrines falls to the ground. And yet Dr. Haldane repeats this mistake when he makes the extraordinary statement on p. 323 that "The mathematical sciences deal merely with time relations (arithmetic and algebra) and space-relations (geometry)." It is difficult to see why arithmetic and algebra should have anything especially to do with time, and certainly the modern view seems to be that pure mathematics as such has no more to do with space and time than it has with mechanics or biology; space and time, surely, are the concern of physics. But Dr. Haldane seems to ignore nearly all that has been going on in the philosophical sciences since he was a student at Oxford. And a great deal has happened since that time, at Cambridge if not at Oxford.

A great part is played in Dr. Haldane's discussions by such adjectives as "physical," "chemical," "inorganic," "organic," "biological," "psychological," "spiritual," etc. But no analysis is given of the meaning of these terms, it being assumed (as usual) that everyone is agreed on this point when in actual fact this assumption is entirely unwarranted, and the use of these terms is apt to become little more than a habit. And yet clarity on this point is of paramount importance for Dr. Haldane, since he is anxious to persuade his readers that what they ordinarily take to be physical is in fact spiritual, and that "being spiritual" is infinitely superior to "being physical." But if everything is spiritual why should we ever have come to invent the adjective "physical," and what can it then mean to say that what is spiritual is finer and more valuable than what is physical?

All these difficulties are traceable in the long run to the fact that we have not yet discovered the correct analysis of such simple propositions as those of the type to which "That is a table" belongs. Gifford lecturers are often in such a hurry

to press on to more exciting topics that they are apt to neglect these preliminaries. But without a satisfactory analysis of such simple propositions we have no satisfactory basis for the use of such adjectives as "physical," "spiritual," etc. Some writers would say, for example, that "that" refers to some unperceived entity to which the table we are said to see is related in some obscure way. Some would say that the former entity is physical and the latter spiritual. But this does not seem to provide any basis for regarding one as in any way superior to the other. Our cognitive relation to these two kinds of entities is clearly different and therefore provides no means of comparing them, since we cannot compare what is perceived with what is not perceived. Other authors, who dislike appealing to unperceived entities, propose to dispense with "physical objects" entirely. But as they also usually hold that there are other percipients who can perceive the same things as they perceive, and as they at the same time believe that what each percipient perceives is private to that percipient, it becomes difficult to understand how a number of people can all talk about and agree about the same topic to the extent that they do, if there is no public and neutral basis common to them. There appear to be many proposed analyses of this simple proposition, no one of which is entirely satisfactory, or has received more than a restricted acceptance. And yet it is clear that until we do agree on this point we shall never know how to agree about the interpretation of natural science, or about the topics in which Gifford lecturers are interested.

This seems to be a curious state of affairs. On the one hand, we have an enormous amount of what is ordinarily called "knowledge of the physical world" which enables us to *do* all manner of things which, only a hundred years ago, would have been impossible, and yet, in the strict sense of the term, we do not *know* what we are talking about, *i.e.* there is the greatest difference of opinion concerning what physics enables us to *know* about the physical world, or even concerning what is meant by the latter expression. Of this anyone will be quickly convinced who reads the discussions of the three leading current English writers on this subject, *i.e.* A. N. Whitehead, Bertrand Russell, and A. S. Eddington. Each of these authors would give a totally different analysis of the proposition, "That is a table," particularly of what he meant by "that" and "is" and "table."

This being the present state of affairs, it seems somewhat premature to interpret the attempts of metaphysics to "frame as consistent as possible a working conception . . . of the whole of our experience" in the way Dr. Haldane wishes to do, namely,

as providing a basis for the conduct of life to which everyone will subscribe. It is very difficult to see how Dr. Haldane's particular metaphysics is going to liberate the world from all the ills of which he speaks, and which he seems to think are a consequence of the prevalence of other philosophies which he regards as false. It has been said, for example, that Russian Bolshevism and the present regime in Italy are both traceable to the influence of Hegel, and the doctrines which Dr. Haldane wishes to inculcate are of the same origin. Dr. Haldane says on p. 188 that "by an act of faith" "all the variegated experiences which appear to us as 'Nature' are interpreted as in ultimate analysis spiritual." But how is everyone to be persuaded to perform the same act of faith since this result is not to be reached in any other way? Surely we have arrived at the various adjectives by discovering *differences* in the variegated constituents of our world. They signify that these constituents can be tied up in a number of bundles labelled "physical," "biological," "spiritual," etc. When we are told that everything is spiritual these distinctions are completely blurred, and the adjectives are emptied of meaning. To call lice, copper sulphate, and cheese "spiritual" as well as saints and seers, is like awarding the Victoria Cross to all who take part in a campaign irrespective of merit. But monistic philosophers are not content with a number of bundles with different labels, they want one big bundle and one big label. And in order to do this they are driven to select one bundle as "real" and dismiss the others as "mere appearance." The original distinctions, which we often make with only too painful vividness, and which seem genuine enough, have to be explained away. And the perpetual wrangling which goes on among monistic philosophers results from the difficulty of deciding which bundles are to be chosen for the dignity of being called "real." Mr. Bertrand Russell, for example, would make his choice in quite a different way from Dr. Haldane, and yet both call their doctrines philosophy, and much can be said on both sides. These differences of selection seem to rest at bottom on the differences in the judgments of value of different men, and on the fact that they are not content to differ but insist on setting up their choices within the frame of some speculative scheme which assigns an inferior place to other people's choices. If Dr. Haldane had contented himself with explaining that what he calls "spiritual values" are the most important things for human beings as contrasted with the beasts of the field, and if he had simply explained that exclusive attention to the topics dealt with in the departmental sciences tends to obscure this fact and leads to an exaggeration of the importance of the "bio-

logical values," many people would probably have agreed with him. For this would remain true independently of the ultimate metaphysical fate of this planet and its inhabitants. Dr. Haldane's treatment seems, in the opinion of the reviewer, to be calculated to antagonise the very people whom his arguments are intended to convince.

And the same seems to be true of what Dr. Haldane says about biology. Those biologists who do not admire Dr. Haldane's biological views are hardly likely to be favourably impressed with his rather high-handed way of dismissing their opinions, and by his frequent appeals, even here, to "acts of faith." They will probably content themselves with dismissing Dr. Haldane's unorthodox views on physiology as the outcome of "metaphysical prejudices." On the other hand, if a proposition is true the particular motives which may have been instrumental to its discovery have nothing to do with its truth. And if a point of view is valuable for science its value is not in the least impaired by the fact that it was reached by one route rather than another. So far as its truth or falsehood is concerned the metaphysical opinions of its discoverer are as irrelevant as the colour of the ink with which he writes. But Dr. Haldane's admirers can hardly be blamed if they feel disappointment at his failure to develop his biological views any more systematically or fully here than in his former works. He seems to be so convinced of the correctness of his views that it does not appear to him necessary to state them so clearly and compellingly that they will force themselves into the understanding of even the most tough-minded biologists. Here again he contents himself with appeals to acts of faith, and to bare statements of his views with little or no argument. He says, for example, that "no scholastic absurdity was ever more of an absurdity than a mechanism of heredity," but he makes no detailed attempt to meet the views of those who believe that they have discovered, and know a great deal about, this "absurdity." If Dr. Haldane has some deeper insight which shows him that these theories are absurd it is a misfortune that he has not made it more clearly available for others. His method of presenting his views will make little impression on the unsympathetic reader. Nevertheless, it seems to be possible to isolate a number of arguments from Dr. Haldane's book which are worthy of the attention of biologists, whether sympathetic or not, since they represent definite challenges to traditional biological opinion which cannot be met merely by labelling them with the word "prejudice" any more than the traditional opinions can themselves be met by labelling them "absurd."

One argument rests on a denial of the Newtonian view of

the world, which is sometimes referred to as the doctrine of the "billiard-ball universe." Dr. Haldane points out that if this theory is interpreted realistically and literally it implies that all occurrences (or "activity," as he calls it) are either chaotic (*e.g.* "statistical effects" of the chaotic movements of ultramicroscopic particles), or they are in "some way guided." But organic occurrences are not chaotic. Therefore: *either* the Newtonian theory is false, *or* organic occurrences are "in some way guided." The latter alternative is that taken by vitalism, and hence if we do not care for this one we are left with the first. And since modern physics no longer interprets the Newtonian theory realistically biologists need not fear that they are doing anything very shocking in following their example. It only means that they will need to do a little fresh thinking on their own account. The only objection to this is that it is painful and tedious—Dr. Haldane himself says that "nothing is more difficult than to persuade people to think"—and the old habits of thought are quite good enough for heuristic purposes, which is all most biologists are much concerned about.

The second argument concerns the mutual dependence of the parts of an organism. The behaviour and the structure of the cell-units depend upon the local medium in which the cells are placed. But the normality of this medium is maintained by the activities of the cells. Hence the cell-behaviour cannot be attributed to their particular structure alone in the way contemplated by the "machine theory." This is simply another way of saying that the parts in an organism are internally related to one another, so that the properties of a part are what they are in virtue not merely of their own "make up" but of their specific organic relations to other parts. In other words, the traditional view attends only to intrinsic properties and neglects relational properties. This certainly seems to be what we observe in organisms, and it is only the success of the machine theory and of the Newtonian mechanics which has prevented biologists from recognising it sooner.

A third argument is a familiar embryological one. If we assume that the orderly changes observed in the development of the fertilised egg depend upon some physico-chemical structure (conceived on Newtonian lines) present in the ovum we should expect, after division, that this structure would be different, and that in consequence each part would behave differently in isolation. But it is found that (in some cases) each part behaves like the original whole, after it is isolated. We seem driven, therefore, either to revise our current ways of thinking or to fall back upon vitalism. But Dr. Haldane's treatment of this difficult problem is far too brief, and it is

clear from what he says that (perhaps because he is not an embryologist) he has not fully appreciated its difficulties. He says, for example (p. 73): "When the cells are separated their primitive environment is restored, and they naturally return to their primitive physiological state. We can regard their characteristic normal behaviour when cell-division again progresses as the natural response to the same series of changes in environment as occurred in the original development." But one has only to consider the development of two eggs from different species of birds both in the same incubator to see that the *specific series* of changes which characterise the development of a given organism cannot simply be correlated in a one-one fashion with a *series of changes* in its environment. Dr. Haldane entirely misinterprets Driesch when he accuses him of assuming that "the development must occur independently of stimuli from the environment." The point is that those "stimuli" are not serially ordered and will not, therefore, enable us to interpret the serially ordered changes which constitute development.

In the face of all these difficulties the practice of most contemporary biologists seems to be either to assume that we need only go on in the old way and it will all come right in the end, or to give up all hope of understanding the organism and to fall back upon vitalism. Both of these are lazy alternatives, and it is difficult to see how anyone who appreciates the present situation can be content with them. Moreover, they have lost any clear meaning they may once have had in view of the revolutionary changes that have been going on in the foundations of natural scientific knowledge. But beyond pointing out the difficulties, or some of them, Dr. Haldane does not seem to be very helpful. He constantly reiterates that "the mechanistic theory of life gives no coherent account of the natural tendency of the body to maintain actively and reproduce its normal structure and activities and to restore them after disturbance"; but he offers no clear and detailed suggestions towards an alternative way in which all this is to be *interpreted*. The accumulation of precise data, as Dr. Haldane himself has shown, has only served to bring the *fact* into greater and greater prominence. What is now wanted is an entirely independent investigation of the logical properties of the organic concept and all its implications. This Dr. Haldane does not appear to have made, but if he succeeds in persuading people to think with a view to such an undertaking this alone will be no mean achievement. It is also very much to the credit of Dr. Haldane that he appreciated the shortcomings of the "Newtonian philosophy" before the present criticisms became current. Moreover, the fact that Dr. Hal-

dane's ideas have proved so successful in the guidance of his own empirical investigations should also be an encouragement to the more timid and conservatively minded to take a more truculent attitude towards tradition. But should such an undertaking ever be brought to a successful issue, and should Descartes ultimately be dethroned from the dominant position he has occupied at the foundations of biological thinking, there will still be no occasion to suppose that the last word has been said, or that biological theories can be turned into a metaphysic, with Hegel in the place of Descartes, and a biologist in that of Newton.

NEW LIGHT ON THE PRIMITIVE VERTEBRATES. By CHAS. H. O'DONOGHUE, D.Sc., F.R.S.E., being a review of **The Downtonian and Devonian Vertebrates of Spitzbergen,** by ERIK A : SON STENSIÖ. [Pp. xii + 391, with 103 text-figures, some coloured, and 112 plates.] (Skrifter om Svalbard og Nordishavet Resultater av de Norske Statsunderstøttede Spitzbergenekspeditioner, Oslo, Jacob Dybwad, 1927.)

IN the early years of the nineteenth century there were collected by Murchison and other geologists in the lower Palæozoic rocks of Scotland, England, and Wales certain interesting fossils of a new type. Their principal part consisted of an approximately semilunate, curved shield, with a backwardly projecting, pointed cornu at each side and a pair of low, rounded prominences near the middle. Before they had been investigated to any extent they were considered to be Trilobites, from a certain superficial resemblance to these forms. Probably no other group of animals, certainly no group of vertebrates, has been so severely handled by the systematist, for they have been bandied from pillar to post in a most remarkable manner. Their geological range extends from the Upper Silurian to the Lower Devonian, but from the high degree of specialisation they exhibit they must have arisen earlier, and they appear to have become extinct before the Carboniferous. So far they are known with certainty only from Europe and Eastern Canada.

Probably some suspicion of their true nature led to their being sent to Agassiz, who was then receiving the British fossil fishes, and this author first recognised their distinctness when, in 1835, he created for them the genus Cephalaspis with four species *C. lyelli*, *C. rostratus*, *C. lewisii*, and *C. lloydii*. He classed them as fishes and regarded them as related to the old group of Ganoids, but more particularly to the armoured Siluroids. Subsequent work has removed the last three from the genus and placed them in a separate genus, Pteraspis, thus leaving as the type of the genus *C. lyelli*, which was practically designated the type species by the author himself.

The examples of the species described by Agassiz came from three sources, some collected by Murchison at Hereford, Brecknock, and Ludlow in England, some by Prof. Jameson at Glamis in Forfarshire in Scotland; but he states that the most perfect examples were those from the collection of Lyell which had also been obtained from Glamis. In view of this statement, of the fact that the species is named for Lyell and that the first two specimens figured on Plate 1 were from Lyell's collection, there is no doubt that Glamis is to be regarded as the type locality in the strict sense and certainly not Arbroath, as suggested by Zittel. It was Kerr, in 1847, who set up Pteraspis to contain the last two, but he considered them as Cephalopods allied to Sepia. Pictet, seven years later, looked upon them as related to the Sturgeons and Polydon. Huxley showed that *C. rostratus* also belongs to Pteraspis. He first thought they might be Ganoids, or Teleosts, but later regarded them as Chondrostei, a group with which Lankester also placed them provisionally in 1868. Meanwhile, new forms had been discovered resulting in the erection of five or six genera and further details of their structure became known.

A break from the old point of view was made by Cope in 1889, who placed them, together with the Cyclostomes, in a larger group termed by him the Agnatha. This was contested by Woodward in 1891, who referred them back again to the fishes, but later, after Dean came forward in support of Cope, Woodward also came round to this opinion. Lankester, however, in 1897 strongly objected to this, and again they became primitive fishes, and in several papers from 1889 on Traquair added considerably to our knowledge of their anatomy and definitely claimed for them Elasmobranch relationships. Dean now made for them a separate group, being content to regard them as a lowly Chordate stock, but Kenna, in 1903, returned them to the Cyclostomes. Regan, the following year, made them out to be Gnathostomes related to both Elasmobranchs and bony fishes. Jaekel, in 1911, took them in hand and led them back to the Cyclostomes, to remain there until in Abel's work in 1919 they once again became fishes. Gaskell and Patten looked on them as a group, Palæostraca, related to Arthropods like Scorpions, Limulus, and Trilobites, but also as forms indicating a possible intermediate group on the way to Cyclostomes. Even as late as 1923 Jordan, in discussing their various possibilities, does not rule out that the group may be " . . . a variant of primitive Crustaceans."

This is but a brief summary of their kaleidoscopic changes, but provides them with sufficient disguises to rival the most " aliased " criminal of a modern detective novel. It is suffi-

cient to show, however, that their true nature, which mystified so many masters, was for long a profound puzzle whose solution was a matter of much difficulty. All the more credit, then, to the worker who replaced surmises by certainties, fancies by facts, and elucidated their structure so completely that never again can the century-long discussion be reopened. The credit of this remarkable achievement is due to Erik A: son Stensiö of Oslo, who has set forth the results of his investigations in two volumes entitled, *The Downtonian and Devonian Vertebrates of Spitzbergen*, of which the first contains 391 pages of text with 103 text-figures, and the second 112 admirable plates. The first 73 plates are reproductions of photographs of the original specimens, the dissections, and sections in various planes, often two or three to a plate. The remaining plates each contain a considerable number of drawings of sections illustrating in detail the structure of five distinct specimens.

The work is based upon the material collected by Norwegian expeditions in Spitzbergen during 1909–12 and 1925, which altogether secured 105 specimens. The introduction gives an account of the collecting of the specimens and the geological horizons from which they were produced. The last part of the book treats of the specimens from the systematic point of view and adds a number of new species to those previously known, including nineteen in the genus Cephalaspis itself. It also includes an illuminating discussion on allied forms. This part of the work is ordinary systematic palæontology, and as such is well done and of great interest.

The bulk of the book is devoted to a detailed description of the anatomy of the creatures, and will, to most palæontologists and zoologists, prove the most interesting. The investigations were carried out by means of sections ground according to the Sollas method, and either the sections were drawn or, where the series of sections was complete enough, wax reconstructions were made. The most instructive preparations were made by dissection with very fine steel needles under alcohol or thin Canada balsam, and here the author shows himself to be a master of technique in a very refractory medium and possessed of more than the average patience. One such dissection, an extremely good one, took two months to complete. It is not possible to go into this part of the work in detail, but it should be pointed out that the descriptions and diagrams of the brain, cranial nerves, and anterior blood-vessels are extraordinary and would put to shame those of living animals given in most textbooks or indeed in many monographs. Indeed, it all seems too good to be true, and one cannot perhaps avoid the feeling, although without tangible reasons, that the account is coloured somewhat by what is known of

living creatures. The most outstanding result has already been alluded to; it is the demonstration of the fact that both the Cephalaspids and Pteraspids are so closely related to the Cyclostomes that they must be grouped with them and not with the fishes. This appears to be proved beyond the shadow of doubt and is in itself a notable advance, even if it only confirms Cope's surmise. On p. 31 it is stated that "The cephalic shield of the Silurian Cephalaspids is thus a single large bone which was cartilaginous in the interior of its endocranial aspect." On the exterior this single structure is divided into areas by grooves and the older and perhaps widely accepted idea that the subdivision of the outer layer of the exoskeleton into polygonal areas is due to a coalescence of small plates is refuted by the author. Another very astonishing result is set forth, for it is claimed that both the endo- and exoskeleton contains bone although it is admitted that its microscopic structure was not satisfactorily made out. This has previously been stated to be the case by both Huxley and Lankester. It is to be noted also that the author has elsewhere claimed that the Elasmobranchs primitively had a bony skeleton. If these conclusions be accepted or substantiated we are left with the somewhat revolutionary idea that the primitive Vertebrate both Agnathous and Gnathostomatous possessed bone and the present cartilaginous skeleton in both Cyclostomes and Elasmobranchs is a secondary condition as it may well be in the Chondrostei. The presence of a bony endo- and exo-skeleton in such primitive forms is taken as showing that the former did not give rise to the latter as is generally supposed, but that both developed simultaneously. In most fossil Ostracoderms there was a pectoral girdle and fins, so that once again their absence, like that of bone in living Cyclostomes, is secondary. Here is a challenge to those who look to a Selachian-like form with a more or less continuous lateral fin fold as the ancestor from which the paired appendages of higher forms have been derived! The hinder part of the specimens is not so satisfactorily preserved, and so it is not possible to state whether or not they had a pelvic girdle and fins. Many other points emerge from the details that are given, but the foregoing are sufficient to show how important they are, and how acutely they challenge the orthodox conception of the primitive vertebrate. Some of these difficulties might be overcome by assuming the Agnathous vertebrates to be the "degenerate" fishes, but so far as we know there is little evidence to support this, and a good deal to be said against it. We shall be forced sooner or later to take up the whole question again.

Lastly, we should note the author's conception of the relationships of the group. The Ostracoderms, or Cyclostomes,

since this name has priority, are a group of primitive agnathous vertebrates. They fall into two independent divisions derived from a common but unknown ancestral stock. On the one hand we have the Cephalaspidomorphs with three groups, the Osteostraci (Tremataspidæ and Cephalaspidæ), the Anaspids, and, thirdly, the Petromyzontes, as the living representatives. On the other hand are the Pteraspidomorphs, also with three groups, the Heterostraci (in which are the Pteraspidæ), Palæospondylus, with the Myxinoids as the living representatives.

The author is to be congratulated on a most remarkable and fundamental work, not the less since it appears in English with very few slips to show its foreign origin. There is little doubt that it will long form the standard book of reference on the Cephalaspids, and that, whether or not all the suggestions put forward will stand the test of time, it is probably the most outstanding contribution to both vertebrate palæontology and zoology that has been made for many years.

REVIEWS

PHYSICS

Crystal Structure and Chemical Constitution : A General Discussion held by the Faraday Society, March 1929. [Pp. 168, with authors' figures and plates.] (London : Gurney & Jackson. Price 8*s*. 6*d*.)

"In this way, an almost ideal programme evolved itself, which gave rise both to a lively discussion and to a report of far-reaching interest and importance. Moreover, to quote Prof. Goldschmidt's own opinion, the invitation to be present at the lecture and discussion brought together the most representative gathering of workers in this field that has yet taken place; he said that he had never been present on any occasion when so many other people were present who were competent to criticise and discuss the problems set before them."

Such were the words of Prof. T. M. Lowry, President of the Faraday Society, in his concluding remarks to the Society's General Discussion on Crystal Structure and Chemical Constitution. It is hardly necessary to enlarge further on this striking recommendation, unless it be to add that the gathering of crystallographers of international repute was so impressive that the opportunity was seized of holding an additional meeting in the Royal Institution to discuss certain questions of crystallographic nomenclature, etc.

The papers presented are classified under four headings: (1) Inorganic Compounds, (2) Organic Compounds, (3) Metals, (4) General; but they are introduced by a masterly lecture by Prof. V. M. Goldschmidt, in whose honour the meeting was called. No structure analyst should be without a copy of this memoir on Crystal Structure and Chemical Constitution. It is a vivid summary of present-day opinions in a subject of ever-increasing interest and value.

W. T. Astbury.

The Earth. By H. Jeffreys. 2nd ed. [Pp. xii + 346, with 2 plates.] (Cambridge: At the University Press, 1929. Price 20*s*. net.)

In the original edition (1924) of *The Earth* a wide range of modern knowledge bearing on the origin, history, and constitution of the earth was, for the first time, brought together in one volume, and discussed on a quantitative basis. In so doing, Dr. Jeffreys rendered a service to geophysics and geology which probably no one else was so well qualified to perform; the success and general appreciation of his work is indicated by the call for a second edition before the lapse of five years.

The book has been completely revised and considerably extended, particularly in the section dealing with seismology; and the geophysical papers which gained for the author the award of the Adams Prize of the University of Cambridge in 1927 have been incorporated. As a result of his own recent work, and that of other writers, his conclusions on many points relating to the history and internal properties of the earth have been rendered more precise than in the first edition.

Dr. Jeffreys' direct and uncompromising style adds a piquancy to discussions some of which, on account of their technicality, might otherwise be

somewhat forbidding to the non-specialist reader; and where he is at issue with other geophysicists, or with geologists, he indicates his opinion of their work with great plainness. He is to a large extent justified in the definiteness of his statements, because of the care he takes to submit every possible question to quantitative test; but a test may sometimes fail because of unknown or neglected factors, and perhaps this risk is not always sufficiently borne in mind. A possible instance may suffice for illustration.

Terrestrial magnetism is excluded from the scope of his work, and, where so much is given, it would be unreasonable to complain of the omission, particularly since a large part of the subject of terrestrial magnetism deals with atmospheric and extra-terrestrial phenomena. But the earth's main magnetic field is produced in the interior, by causes still unknown; and it undergoes a rather large, relatively rapid, and somewhat irregular secular variation. This indicates that certain large-scale changes are occurring within the earth; one possible explanation of them invokes an internal circulation of the earth's substance. If this exists, then, as I have suggested elsewhere, the motion may produce a significant degree of drag on the surface layers, capable of causing relative movement of the continental masses over the body of the earth. Such a possibility seems worthy of the author's consideration, particularly in connection with his discussion of the hypothesis of continental drift.

As in the case of the first edition, the moderate price of the volume deserves mention; both size and price are now rather greater than for the original issue. S. Chapman.

Light, an Introductory Textbook. By C. G. Vernon, M.A., B.Sc. [Pp. vi + 191, with 147 figures.] (Cambridge University Press. Price 4*s.* or 3*s.* 6*d.* without examples.)

The author of this book says, in his preface, that there are probably many teachers of science who, like himself, are not satisfied with the teaching of light in schools, and he wishes to suggest an improved method of treating the subject. His suggestion is, in fact, to use waves instead of rays in the study of the refraction and reflection of a beam of light. It is not obvious in what way his method removes what he calls "the dreary grind of thinly disguised geometry," for, in his treatment of the two methods, both are equally geometrical. The formulæ arrived at are, of course, equivalent, and the author himself says, on p. 65, that the ray type of formula is, actually, the more convenient. The truth is that liveliness and interest can only be put into the study of light by keeping in contact with actualities, and by illustrating these dry formulæ at every step by applying them to cameras, magnifying-glasses, spectacles, and other optical appliances found in every house. In this book the camera is dismissed in four lines, the telescope nearly as briefly, and the simple magnifying-glass is not clearly explained. The difficulties the author aims at overcoming still remain. To take a comparable case; a mere formal change in the proofs used by Euclid would never have satisfied the reformers of geometrical teaching in schools.

The first chapters consist of a short historical summary of theories of light, an elementary outline of wave motion, a description of the ripple tank with experiments, and an elementary account of some of the simple properties of beams of light. It is shown that trains of waves have similar properties to beams of light. The effect of a curved reflecting or refracting surface in changing the curvature of a wave is taken up qualitatively in Chapter VI, and quantitatively in the next two chapters. In many details the treatment is confused and scrappy. The formulæ for refraction at spherical surfaces are not properly given and these chapters do not give at all a fair picture of the possibilities of the wave method. The convention

of signs is not consistent or clear, and most of the proofs are very incomplete. There are further chapters on Velocity of Light, Colour, Illumination, and Interference and Diffraction.

The book, however, is not likely to help in the teaching of the subject of Light in Schools. T. B. V.

Polar Molecules. By P. DEBYE, Ph.D. [Pp. 172, with 33 illustrations.] (New York: The Chemical Catalog Company, 1929. Price 3.50 dollars.)

THIS is a very important book; thus it is a little regrettable that a speedier passage through the Press has not been possible, for at the date of publication (May 1929) it barely represented the "last word" on the subject, and subsequent months have only added to this unfortunate lag.

Nevertheless, the appearance of a work by Professor Debye on any aspect of molecular polarity is an event; he contrives to combine, as only a master can, the insight of the mathematical physicist with the passion for facts characteristic of the engineer.

One thing that may strike the reader is that (the title notwithstanding) the subject index only contains three entries under "polar molecules." As would be expected, one such reference gives the definition of such a molecule as one for which the electric moment has a finite value. Considering the difficulty usually experienced in framing a satisfactory statement acceptable to scientists in general, this is a matter for mild exultation. Actually, the author's field of inquiry is so widespread that, with polar molecules as his "text," he has delivered a homily of extreme interest to physicists, chemists, and the more theoretically-minded of engineers.

The first two chapters, dealing with the necessary parts of electrostatics, and the earlier views concerning polarisation by distortion are largely formal; they show incidentally the powers of the classical methods, while at the same time laying bare their limitations. There is no gainsaying the tendency—and the author drives the point well home—of recent developments to show a return to conceptions more closely akin, in certain respects, to the older physics. That the new wave-mechanics in the hands of Schrödinger and his followers should need so little special mathematical machinery is more than a hint that doctrines of continuity are likely to come into their own again.

Chapter III will be welcomed by chemists for the excellent review it contains of recent work on polarity as measured for molecules in different solvents (benzene, carbon tetrachloride, carbon disulphide, and hexane). The iodine molecule (I_2) possesses a considerable electric moment, a fact for which one would not have been well prepared a few years ago, but is by no means incompatible with recent evidence from other quarters. Tin tetraiodide, on the other hand, has zero moment, presumably due to its tetrahedral character akin to methane.

In Section 20, dealing with anomalous dispersion and absorption in solids, Prof. Debye has some interesting remarks to make upon the conservation of molecules as such in the crystal lattice. This is another instance (of which several have come to light in the last few years) of the way in which increasing knowledge of the dielectric constant is leading to results of importance in our understanding of the true nature of the solid state.

Later chapters explain some of the more important issues involved in the application of the quantum theory to the general question of polarity. A very elegant deduction of the laws of geometrical optics (as the limiting case of high frequency) from the equation of wave propagation is given in Section 26, and forms an excellent introduction to the basic ideas of the new wave mechanics.

The book is not quite free from minor blemishes. A factor, ½, has gone adrift in the last term of the first equation of Section 3, and a negative index, in place of a positive one, has found its way to the head of the last column in Table VII. On page 30 "takes care of" is a somewhat too obvious rendering of "pflegt," but otherwise the volume reads pleasantly enough. It is certainly one to possess. F. IAN G. RAWLINS.

Wien-Harms: Handbuch der Experimentalphysik. Band XXII.
Zeemaneffekt, von E. BACK.
Ergebnisse und Anwendungen der Spectroskopie Ramaneffekt, von G. Joos, br. R.M., 41.
(Akademische Verlagsgesellschaft, m.b.H. Leipzig. Price geb. R.M., 42–80.)

THE Zeeman Effect has assumed such importance in the development of modern physics that an authoritative description of its investigation is bound to excite our interest, particularly when the responsible authority is one whose name is so intimately connected with recent work on the subject as Prof. Back. The short historical and theoretical introduction provided here is an adequate guide to the experimental procedure, with which this Handbuch is more properly concerned. The construction of the powerful electro-magnets, the spectrographic arrangements, and the sources of light used in these investigations are all described in an interesting manner. The theoretical treatment of the Zeeman Effect is, however, not exhausted in the introductory sections, and we find that the classical and quantum theories of the effect are, later, extensively set forth, the introduction of the idea of the rotating electron, postulated by Goudsmit and Uhlenbeck, being given special prominence. The Paschen-Back effect is fully described, and attention is drawn to the work of Darwin on the effects of magnetic fields of moderate intensity. The description of the Zeeman Effect is completed by chapters on the behaviour of band spectra, and on the effect of magnetic fields on the absorption lines exhibited by crystals, contributed by G. Joos. In the latter description the writer inclines to the view that the oscillators responsible for the production of these lines are normal components of the crystal lattice—and not foreign centres, such as are postulated by Lenard in his explanation of the behaviour of certain phosphorescent substances—oscillators which, for some unknown reason, have a very restricted probability of absorption. In this section we also find a description of the experimental arrangements used by Becquerel for the investigation of the Zeeman Effect at very low temperatures.

As the title indicates, the book is divided into three parts, the second of which is devoted to the applications and results of spectroscopy, again contributed by G. Joos. Naturally, a good deal of theoretical treatment is introduced here, for the writer deals with the ideas of Bohr, Stoner and Pauli on the distribution of electrons in the atoms of the periodic system, and later he gives a complete catalogue of the line spectra emitted by the elements of the periodic system in their various stages of ionisation. He discusses the different types of band spectra and their theoretical significance, and also the intensity relationships which exist in line and band spectra. An examination of all these matters appears to be necessary before the important modern developments of the application of spectroscopy to other branches of physics can be discussed. These comprise the determination of e/m_0, the proof of the relativity correction in fine structure, and the displacement towards the red of spectral lines due to the gravitational field of the sun. Considerable attention is directed to the applications of spectroscopy in chemistry, such as quantitative spectral analysis by the method of Hartley and de Gramont, to the spectroscopic examination of isotopes, the relation

between absorption spectra and chemical combination, and the spectroscopic determination of thermochemical data. Among the recent applications of spectroscopy in astrophysics we note investigations of the relation between the spectral class of a star and its temperature, the Russell diagram, the systematic examination of the Doppler Effect, the "flash spectrum" of the sun, and the auroral green line.

The third portion of the book is a very neat description of the Raman Effect. It occupies only a few pages, but it gives a convenient outline of the experimental arrangements, such as those of R. W. Wood, and of the main experimental results, and on the whole is quite stimulating in its effect.

The printing, illustration, and binding of this volume are of that standard of excellence which its precursors have well laid down. It is another welcome addition to the literature of modern experimental physics. L. F. B.

Introduction to Theoretical Physics. By Prof. Leigh Page, Ph.D. [Pp. x + 587]. (London: Macmillan & Co., 1929. Price 25*s*. net.)

In these days, when theoretical physics is making such rapid advances, there must be many who will welcome the opportunity of reading a book such as this to improve their mathematical equipment. The author's aim is to provide an elementary survey of the whole field of theoretical physics, assuming only a working knowledge of the differential and integral calculus and an acquaintance with the elements of differential equations. After an introductory chapter on Vector Analysis, the author deals with the subjects of Dynamics, Hydrodynamics, Thermodynamics, Electrodynamics, and, finally, Optics and Spectroscopy. As all these subjects are dealt with within the limits of about 600 pages, it is not possible to give more than a cursory treatment of each section.

In the section on Dynamics the author begins with the definitions of velocity and acceleration, and in the course of four short chapters reaches the subject of Hamiltonian dynamics and phase integrals. While this condensed treatment will doubtless be a useful and welcome summary to those who have studied this subject previously, it will hardly commend itself to the uninitiated. The subject is obviously developed with a view to its application to the older orbital theory of atoms. Had the author intended to introduce the reader to the newer wave mechanics the treatment would no doubt have been different. For such a purpose an adequate account of vibrations and normal modes would have been useful.

The subject of classical Thermodynamics is dealt with somewhat sketchily in a single short chapter, and one wonders whether, in view of the central position of the subject in physics, it would not have been an advantage to limit the range of other subjects discussed in order to allow of a more elaborate treatment. Statistical Mechanics is also dismissed in a dozen pages, and the Kinetic Theory (very elementary in its scope compared with the treatment of Dynamics) in a score or so.

The author is at his best in his account of Electromagnetic Theory, where due emphasis is placed on the experimental laws of Coulomb, Ampère, and Faraday, and on Maxwell's derivation of his field equations. Some of the modern developments are touched upon, as, for instance, the principles of relativity and the Compton Effect, though not, of course, in any detail. The results are expressed in vector form wherever possible, as is now customary and desirable. The notation used is that of Gibbs, which is explained at some length in the introductory chapter, and numerous useful analytical theorems are there proved in vector notation.

In the section on Optics the author rightly gives some prominence to geometrical and physical optics, which are now often neglected for the more exciting theories of atomic spectra. A knowledge of interference and

diffraction is necessary, if only to emphasise the limitations of the present quantum theory. The author has felt it necessary, in the theory of spectra, to limit himself to the older quantum theory. The book stops short of the matrix and wave mechanics, though, in view of the importance of recent advances, an introductory book on Theoretical Physics published in 1929 might with advantage have introduced such terms as *eigenfunctions*.

The book will probably be most appreciated by those who want a refresher course in theoretical physics as a preparation for the study of more recent advances. The book is well written, carefully edited, attractively printed, and the publishers are to be congratulated on producing so large a book for so modest a price. J. E. L.-J.

CHEMISTRY

An Introduction to the Chemistry of Plant Products. By Paul Haas, D.Sc., Ph.D., and T. G. Hill, D.Sc., A.R.C.S. Second Edition, Volume II, *Metabolic Processes*. [Pp. viii + 220, with 12 text figures.] (London: Longmans, Green & Co., 1929. Price 10s. 6*d*. net).

Such has been the progress of physiological botany in recent years that a student now entering upon the subject is in grave danger of losing his way among the masses of conflicting data and opposing theories. He is indeed fortunate to have now offered to him such an authoritative guide as Volume II of Haas and Hill's *Introduction to the Chemistry of Plant Products*, now in its second edition. The authors do not claim to have prepared an exhaustive survey of the literature involved, but rather a critical treatise such as may "form the basis for further study." And that from their hands is of far more value. The reader is grateful for the clear, impartial setting forth of the data, the concise statement of the theories evolved, and summary of the evidence pro and con, sometimes with a frank admission of its inadequacy.

With respect to subject-matter, the book follows the plan of the first edition, which appeared in 1922. At that time Volume I was reserved for discussion of plant chemistry and Volume II for the metabolic processes. This division of the matter is adhered to in the present editions (the fourth edition of Volume I having appeared recently). The metabolic processes are considered under the following headings: synthesis of carbohydrates, fats, and proteins, respiration, and growth. All plant physiologists will agree that to discuss these subjects adequately, and that within the space of only 220 pages, is a very difficult task. Many phases of plant physiology are to-day under intensive study and, in consequence, the literature is a confusing mass of uncorrelated data. It requires nice judgment to choose what of the new shall be included in a general survey and what of the old shall be excluded in an attempt to make the discussion conform to current thought. The chapter on respiration is illustrative of the difficulty. The views of Wieland and Warburg on the mechanism of oxidation are set forth, together with those of Moureu and Dufraisse, Thunberg, Quastel, and their associates. In the section on respiratory pigments it is interesting to find reference to so recent a publication as that of Keilin, 1929, on cytochrome and to find the section on the mechanism of respiration labelled "Current Ideas." In the latter section, by the way, is Blackman's recent hypothesis of respiration according to which respiration is conceived to be a series of phases: A-hydrolysis of various reserve carbohydrates to free hexoses, B-activation of hexoses to heterohexoses of unstable internal ring structure, C-glycolysis to form intermediate products such as methyl glyoxal, acetaldehyde, etc., and D-conversion of these into final products whose nature depends upon whether anaerobic or aerobic conditions obtain. Under aerobic conditions there is thought to be an oxidative anabolism, as a side reaction to the last phase of respiration. The chapter ends by observing that the discussion

thus far has covered only respiration of carbohydrates, whereas a full discussion should include also the derivation of energy from fats and proteins by higher plants and from sulphur, iron, and ammonium salts or nitrites by certain bacteria. Obviously such a discussion cannot be given in the present state of our knowledge. The same is true of every chapter in the book.

E. McCoy.

Enzyme Actions and Properties. By Prof. Ernst Waldschmidt-Leitz. Institut für Biochemie, Deutsche Technische Hochschule, Prag. Formerly at the University of Munich. Translated and extended by Robert P. Walton. [Pp. viii + 255.] (London: Chapman & Hall, Ltd., 1929. Price 20*s*. net.)

Recent progress in this field has been particularly rapid, and the work of R. Willstätter and his associates in the last ten years has led to so new a conception of the ultimate nature of enzymes, that theories of their physicochemical properties must be revised and reorientated accordingly. It follows from R. Willstätter's concept of an enzyme as a colloidal bearer carrying a specific active group that the properties of a crude enzyme preparation are not independent of the concomitant material. As an example from this book (p. 14), stomach lipase and pancreas lipase, which had previously been differentiated on account of their different *p*H optima, were identified by R. Willstätter and shown to possess the same *p*H optimum after purification. Other properties which have been found to be disturbed were inactivation specificity and the reaction system. The last determines the behaviour of the enzyme on adsorption and elutriation, which were utilised for purposes of purification.

The author, Prof. Waldschmidt-Leitz, was associated with R. Willstätter in part of this research and can therefore write authoritatively; that he deals with the subject mainly from the view-point of the Willstätter school makes the book no less welcome. The first section gives a concise account of the principles of enzyme behaviour, the general properties of enzymes and the methods used for their preparation and purification. In the second or special section the more important enzymes are described in greater detail, though no attempt is made to deal with the subject exhaustively. Special attention is paid to such questions a specificity and kinetics in the light of recent work. The treatment throughout is on general lines and admirably brief.

This book should prove useful to chemists and biologists who are interested in this field; its particular value lies in providing a convenient account of the work and methods of the Willstätter school and in representing the modern views on enzymes and enzyme behaviour.

R. P. Hobson.

The Constitution of Sugars. By W. N. Haworth, D.Sc., Ph.D., F.R.S. [Pp. viii + 100, with 2 plates.] (London: Edward Arnold & Co., 1929. Price 8*s*. 6*d*. net.)

The author has given, in these hundred pages, a most excellent summary of the chemistry of the sugars; this is what one would expect from such an acknowledged authority on the subject. The student reading for honours in chemistry is fortunate in that he now has such a book available. The special problems connected with the sugars are fully discussed, and the present-day position clearly indicated. The book is well written and also well illustrated, both with structural formulæ and reproductions of models.

J. N. E. D.

Photometric Chemical Analysis. Vol. II, Nephelometry. By John Y. Yoe. [Pp. xvi + 337.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, 1929. Price 22s. 6d. net.)

Nephelometry is concerned with the measurement of the "cloudiness" of dilute suspensions. The theory of the scattering of light by small particles was given by the late Lord Rayleigh as long ago as 1871. The first nephelometer in which any regard was paid to the principles developed in Rayleigh's theory, and which had any sort of claim to being a scientific instrument, was devised by T. W. Richards in 1894, and was used by him to determine *residual* silver chloride contained in the filtered suspensions occurring in his work on the atomic weight of silver. For such work his nephelometer was ideally suitable. Richards knew this, and, what is more important, knew the limitations of the method. For a single reading, he considered an accuracy of about 3 per cent. to be attainable. Further, he regarded the practice of nephelometry as unsuitable for general analytical work. And there can be no doubt that Richards was right, although the author of this work would have us believe the contrary. Richards's instrument was simple and can be constructed at very small cost by anyone having a moderately useful pair of hands. The author describes modifications in design, including the provision of plungers, lamp house, colorimetric reflectors, and what-not, which merely add to the convenience and cost of the instrument without largely, if at all, increasing its accuracy. This was Richards's opinion in 1912. Nephelometers have been designed since that date and some of these are described in the present book, a companion volume to Volume I, dealing with colorimetry. These descriptions are very largely of the trade catalogue type and the working instructions are not very helpful. We miss reference to the Zeiss instrument, which has a range of application exceeding that of any of the instruments described. A lot of confused and cloudy thought has gone to the preparation of this work. The author has a flair for the development of nomenclature—we find references to tyndallometry, turbidimetry, micro- and macro-nephelometry. The practice of nephelometry is no clearer to us when we have pondered these words. But we excuse the author, as we believe that this habit of developing systems of nomenclature will die hard, for it comes down to us from Adam, who, on the very first day when he assumed control of the zoological gardens in Eden, started right away by naming the animals under his care. And are we not all importuned by the "old firm" in these later degenerate days to "give it a name, gentlemen"? The most useful part of the book is that giving full details of analytical nephelometric methods developed for the estimation of ammonia, arsenic, calcium, chlorium, phosphorus, sulphur, acetone, amylase, dichloro-ethyl-sulphide, fats, oils and fatty acids, lipase, nucleic acids, butyric acid, pepsin, proteins, purine bases, and trypsin. The book is well got up, is well printed on good paper, and is well bound. Its price is unreasonably high, and could be reduced. We are not of the breed of the frugal Scotch housewife who, when ordering neck of mutton, desired that it should be cut off near the tail from an animal suffering from goitre. We don't even ask for any sort of bonus on our purchase, but here we have it without asking, in the form of 56 pages of very moderate squared paper interlarded through the volume, and 4 pages of logarithms and antilogarithms at the end. These constitute together about one-fifth of the book! Our supply of squared paper contained in Volume I is not yet exhausted!! The book contains a valuable bibliography, and is provided with adequate author and subject indexes.

J. S. G. Thomas.

A Comprehensive Treatise on Inorganic and Theoretical Chemistry. By J. W. Mellor, D.Sc. Vol. IX, As, Sb, Bi, V, Cb, Ta. [Pp. xiv + 967, with 161 diagrams.] (London: Longmans, Green & Co., 1929. Price 63*s*. net.)

It would be an interesting experiment to set a literary critic the task of reviewing a few modern scientific works to see what he could make of them and whether the said critic could succeed in avoiding the methods of the *Monthly Review* in which, according to Hazlitt: "It was deemed sufficient to recommend the work in general terms—'this is an agreeable volume' or 'this is a work of great learning and research,' to set forth the title and table of contents, and proceed without further preface to some appropriate extracts, for the most part concurring in opinion with the author's text, but now and then interposing an objection to maintain appearances and assert the jurisdiction of the court."

Certainly in the present case, whilst it may be a matter of opinion whether any treatise on chemistry can be described correctly as "agreeable," Dr. Mellor's treatise certainly merits the second description as a "work of great learning and research" and will continue for many a decade to supply powder and shot for the ever-advancing army of research workers no less than for the "civilian population" who are daily utilising the results achieved by the army of investigators.

The elements dealt with in this, the ninth volume, namely Arsenic, Antimony, Bismuth, Vanadium, Columbium, and Tantalum, are those with which industry in general is rather less concerned, though they have special properties which make them essential components in small amounts for many industries, such as arsenic compounds for the production of sheep-washes, weed-killers, insecticides, drugs, and some alloys, antimony as an important component of type metal, vanadium for high-speed steel, and tantalum for electric light filaments and current rectifiers.

In his desire to adhere to the strict letter of his self-imposed terms of reference Dr. Mellor is chary of mentioning any organic derivatives or metallurgical applications, so that the important chemo-therapeutic uses of organic arsenical compounds are confined to the mention of Salvarsan (incorrectly described as "di*oxy*diaminoarsenoben*zol* dihydrochloride") and atoxyl, though there is an interesting account of the physiological actions of arsenic which may be recommended to those contemplating the removal of unwanted members of the community by means of weed-killers; then there is no mention of the widespread use of "Tartar Emetic" as a fixing agent in the dyeing industry, and in the matter of high-speed alloys of vanadium we are told only enough to whet our appetite for more.

On page 386 it is stated that "antimonic acid is used in the manufacture of aniline red and yellow," by which are meant presumably Magenta and Phosphine, but antimonic acid is certainly not used to any extent for this purpose nowadays, if it ever has been outside patent specifications, though on the other hand antimony chloride is used extensively at present for the production of certain important yellow vat dyes.

These are trifling matters, however, and the latest volume of "Mellor" may be safely left to stand up for itself in the library shelves with its equally sturdy brethren.

F. A. Mason.

Reference Book of Inorganic Chemistry. By W. M. Latimer and J. H. Hildebrand. [Pp. viii + 442.] (New York: The Macmillan Co., 1929. Price 16*s*. net.)

The purpose of this volume is to provide a descriptive chemistry to complete the series begun by Hildebrand's *Principles of Chemistry*, and continued in Bray and Latimer's *Course in General Chemistry* (which forms a laboratory course), and which have been in use in California University since 1912.

The present volume has been compiled as a reference book rather than as a textbook, and in it the authors have sought to present essential facts briefly and in due relation to other facts and principles. The teacher using it will have to map out his own course following the order of arrangement best suited to his special requirements.

As the volumes, of which this is the third, are intended chiefly for use in American Universities it is uncertain how far the *Reference Book* will appeal to teachers on this side of the Atlantic, but those seeking new methods of approach to the subject of general and inorganic chemistry would do well to examine the present volume. F. A. M.

Perfumes, Cosmetics, and Soaps, with especial reference to Synthetics. Vol. II, Third Edition. By WILLIAM A. POUCHER, Ph.C. [Pp. xiv + 522.] (London: Chapman & Hall, 1929. Price 25*s.* net.)

FOR the benefit of those readers who are not familiar with the earlier editions of this book it should be stated that the complete work is composed of two volumes of which the first is of the nature of a dictionary of the more important raw materials, including pigments and dyestuffs employed in perfumery. The volume under consideration is the second and is described on the title-page as a Treatise on Practical Perfumery; it is divided into two parts, of Perfumes and Cosmetics, containing thirteen and eleven chapters respectively, and there are, in addition, an appendix consisting of tables of measures, etc., and a guide to the purchase of materials and plant.

The book opens with an historical sketch containing a number of illustrations of Egyptian and Roman toilet articles and includes a sort of "apologia" for the use of cosmetics in general, upon the soundness of which opinions may differ. The second chapter, entitled the Production of Natural Perfumes, opens with an attempt to explain the origin of perfumes in plants and to account for their physiological significance, but as most of this is pure speculation its value is not very great. The rest of the chapter is devoted to a description of the methods, such as distillation, expression, enfleurage, etc., employed in extracting the natural perfumes of plants and contains a number of plates illustrating the processes described. Next follow two short chapters on "The Purchase of Flower Absolutes" and "Odour Classification" and then one devoted to the important subject of Fixation. Chapter VI, consisting of about 140 pages, contains interesting monographs on flower perfumes, which give accounts of the history and the varieties of plants used, and notes on the odour and composition of the essential oils concerned, with again a number of quite good illustrations. The remaining chapters of Part I are devoted to toilet waters such as eau-de-cologne, lavender water, etc., and to soap perfumery, tobacco flavours, incense sachets, and solid perfumes. Part II on Cosmetics is more of technical interest to the practical man, dealing as it does with formulas for bath, dental, hair, manicure and shaving preparations as well as lip-salves, skin-creams, toilet-powders, and theatrical preparations. As may be seen from the above brief summary, the book covers a large field and deals with the subject exhaustively; it is attractively got up and contains a wealth of interesting information and can be strongly recommended to anyone wishing to make himself *au fait* on the subject of flower perfumes and their numerous uses. P. HAAS.

GEOLOGY

Diamond: A Descriptive Treatise. By J. R. SUTTON, M.A., Sc.D. [Pp. 118, 111 illustrations on 35 plates, 3 text-figures.] (London: T. Murby & Co., 1928. Price 15*s.* net.)

THIS book is the outcome, as the author tells us, of thirty-five years of observation chiefly in South Africa. As the title indicates, it is strong on the

descriptive side. Many curious, interesting, and little-known facts are recorded on the crystallography and surface-markings of the gem. After an introductory chapter briefly describing the geology of the Kimberley mines, the author deals with the physical properties and morphology of diamond; its surfaces, colours, inclusions, aggregated forms, and overgrowths. Much first-hand information is illustrated by numerous excellent pencil drawings or photographs. The author is not so strong on the genesis of diamond as he is on its description. One of his objections to the orthodox view of diamond as an early crystallisation at high temperature and great pressure is that it must, therefore, have formed in the presence of uncombined oxygen. There is, however, no proof that uncombined oxygen was present in the diamond pipe magmas. We confess that we find it difficult to follow the alternative theory put forward by Dr. Sutton, namely, that diamond is formed at a late stage in the magmatic history of the pipes as "plastic crystals" growing on the margins of inclusions of eclogite, and that it grew by the superimposition of plastic shells on the plastic or solid crystal core. The book is a rich storehouse of curious lore concerning the diamond.

G. W. T.

Agricultural Geology. By F. V. EMERSON, Ph.D. Second Edition, revised by J. E. SMITH. [Pp. xvi + 377, 271 figures.] (New York: J. Wiley & Sons. London: Chapman & Hall, Ltd., 1928. Price 16*s*. net.)

THIS is one of those very efficient machine-made textbooks which are turned out by the score on the other side of the Atlantic. Suitable lengths of geological material appropriate for agricultural students have been sawn off, and then neatly dovetailed together. This is not to say that the selection has not been well made, or that the material is poor; on the contrary. Yet we can imagine a textbook on this subject which would make much less "dreich" reading (if we may use an expressive Scots term) than this. The book was compiled by the late Prof. F. V. Emerson, of the Louisiana State University, and has been thoroughly revised by Dr. J. E. Smith, of the Iowa State College. Only minor changes have been made in the original work; new questions have been inserted into the lists at the ends of the chapters; and tables for the identification of minerals and rocks have been introduced. The reviewer is very sceptical of the practical value of this and all such tabular matter. The best chapters are those in which the various classes of soils are described; those on pure geology tend to degenerate into strings of definitions. Good sets of reading references are appended to sections, and comprehensive sets of questions are put at the ends of chapters. The United States soil maps are usefully listed and classified in an Appendix. The illustrations are partly clear-line diagrams and partly photographs, which are sometimes reduced on too small a scale for clearness. A few of these photographs are perfectly useless, as, for example, that of phosphate nodules on p. 305, which might be anything, and the photograph of obsidian on p. 20, which is merely a black smudge. The American derivation of practically all the material, textual and illustrative, will tend to limit the usefulness of this work in this country; but the intelligent agricultural student will, no doubt, be able to select such material as will be of value to him. G. W. T.

Patterns for the Construction of Crystal Models representing Actual Crystals. By F. SMITHSON, Ph. D., F.G.S. [Thirty-six patterns with instructions and labels.] (London: T. Murby & Co., 1928. Price 4*s*. net.)

THIS excellent series of patterns or nets is intended to supplant the series drawn some years ago by the late Mr. J. B. Jordan. Unlike the earlier set, however, this set is based on the forms displayed by actual crystals. Twenty-seven different minerals are represented in the set of thirty-six models, and

six twinned crystals are supplied. The set is also intended to illustrate *Crystallography*, by Dr. J. W. Evans and Mr. G. M. Davies. We have had some of the models made, following the clear instructions, and find that they answer their purpose well, although rather on the small side. We should like to see a similar series on a very much larger scale for use in lecture demonstrations. G. W. T.

Patterns for a Series of Twelve Block Models illustrating Geological Structures, with Descriptive Notes. Patterns for a series of Fourteen Block Models illustrating Geological Structures. Second Series (introducing Igneous Phenomena), with Descriptive Notes. By F. Smithson, Ph.D., F.G.S. (London : T. Murby & Co., 1929. Each Set, 1*s*. 6*d*.)

Dr. Smithson has followed up the Crystal Models with two further series of patterns illustrating familiar geological phenomena, comprising faulting, folding, unconformity, overstep, overlap, sills, dykes, laccoliths, and phacoliths. These patterns are to be cut out and pasted on to specially prepared cardboard blocks which can be obtained from the publishers for an additional shilling or two. The prepared models are small and light, but they are exactly adapted for individual making and handling by students, who will acquire by their study the highly desirable habit of seeing geological structures in three dimensions. Larger models of the same kinds would be very useful for lecture demonstrations, but the ingenious lecturer can no doubt prepare them himself. G. W. T.

Instructions for using the Quantitative Mineralogical Classification of Eruptive Rocks proposed by S. J. Shand. By Prof. S. J. Shand, D.Sc., F.G.S. [Pp. 16.] (London : T. Murby & Co., 1929. Price, 1*s*. 3*d*. net.)

In this pamphlet Prof. Shand explains the application of the method of classification he proposed in his recent book on *Eruptive Rocks* (see Science Progress, April 1928, p. 722). He deals first with rocks that are completely determinable by means of the microscope and micrometer ; then with rocks that are not completely determinable by optical means ; and concludes with a tabular arrangement of the groups of the classification. He gives numerous valuable hints of petrographical methods, especially in regard to the determination of partly crystalline and glassy rocks. Mineralogical, refractive index, density, flame, and microchemical tests of the composition of rock glasses are suggested ; and the mode of calculation of the potential mineral composition from a chemical analysis is given. The student of "Eruptive Rocks" cannot do better than provide himself with this elucidatory pamphlet. G. W. T.

Applied Geophysics in the Search for Minerals. By A. S. Eve and D. A. Keys. [Pp. x. + 253, with 92 illustrations]. (Cambridge : The University Press, 1929. Price 12*s*. 6*d*. net.)

Until a few years ago the science of Geophysics had practically no association with the occurrence of minerals, at any rate in regions of the earth's crust accessible to the drill and the shaft. During the War the scarcity of minerals led to an intensive investigation into the possibilities of rendering the location of valuable deposits less hazardous and more scientific, and the notion was fostered of utilising those characteristic physical properties of minerals which could be detected and measured at the surface of the earth. Isolated attempts to use the magnetic and electrical qualities of certain ores had previously indicated these two promising lines of attack, but their further exploitation had to await the evolution of more sensitive,

yet portable apparatus for field use, and the gradual development of field technique. In addition the gravitational researches of Baron von Eötvös had indicated another fruitful field for further exploitation. Finally, the researches carried out during the War with the view of locating submarines through the intervening water, ranging guns by means of the air and shock waves, and detecting subterranean sapping activities via the sounds transmitted through the earth, formed the basis of what are now known as the sonic or seismic methods of detecting structural anomalies associated with mineral deposits, and thereby locating regions favourable to the existence of these latter.

The present work, compiled by two scientists of unquestionable status, will be welcomed both by the general technical public and the specialist, whether physicist, geologist, or mining engineer. The introductory chapter is a specially valuable contribution in that it stresses just those points which the reader must assimilate if he is to appreciate the scope and the limitations of the various methods and apparatus afterwards described, and also if he is to assess correctly the various articles devoted to the subject which are appearing with increasing frequency in the periodical technical literature.

The chapter devoted to each method—magnetic, electrical, electromagnetic, gravitational, seismic, and other methods—are very carefully compiled; and the scope and limitations of each process are assessed with reasonable fairness. The authors have devoted more space to electrical than to the other methods, and in particular have given a very full account of electrical-resistivity measurements, with which they have themselves been intimately associated in practice. The value of the book would have been enhanced by the provision of tables of physical characteristics of typical rocks and minerals. Although these are necessarily only available between fairly extreme limits, they are of great value in indicating the particular minerals and associated rocks which are adapted to location by means of any one method or any combination of methods.

The book is an introduction to Applied Geophysics, which should certainly be the first textbook for the serious student of this modern science. Thus equipped he will be able to attack the various specialised articles which focus on particular problems of theory or practice which can only be briefly discussed in such a comprehensive general treatment.

H. Shaw.

BOTANY

A Laboratory Manual of General Botany. By Emma L. Fisk and Ruth M. Addoms. [Pp. viii + 103.] (New York: The Macmillan Co., 1928. Price 4*s*. 6*d*. net.)

Written to accompany the revised edition of *A Textbook of General Botany*, by Smith, Overton, Gilbert, Denniston, Bryan, and Allen, this manual follows the lines of the work in the elementary courses in Botany in the University of Wisconsin. It follows the usual general plan of such a course. The physiology is with advantage studied in connection with the anatomy and every opportunity is given to the student to think out matters for himself, as he often has to supply the facts in answer to numerous questions put in the text. This method should prove useful in classes where there are comparatively few demonstrators. The treatment of secondary thickening is much too slight and cannot be regarded as satisfactory, and the absorption and transportation of water are only mentioned in short notes. The illustrative plants are nearly always given their common names only; the addition of botanical names would have increased the usefulness of the book.

E. M. C.

ZOOLOGY

The Cowbirds: A Study in the Biology of Social Parasitism. By HERBERT FRIEDMANN. [Pp. xvii + 421, with 28 plates and 13 text-figures.] (Baltimore: Charles C. Thomas; London: Baillière, Tindall and Cox, 1929. Price 27*s*. net.)

ONE of the most attractive aspects of Ornithology is the study of parasitism. The classical example from Aristotle's time has been the Cuckoo, and in spite of the mass of writing that has accumulated on this bird and its habits it is only in recent years that a group of workers, notably Chance in this country, have succeeded in solving the 2,000-year-old mystery of its actions. We now know that parasitic breeding habits occur in various groups of birds, African Honey-guides and Weaver-birds, a South-American duck, other Cuckoos in India and Malay, and the American Cowbirds. This book records in some details the information gathered during five years' continuous observation on this relatively small group: three years in New York State, one in Argentina, and one on the Texan-Mexican border. In dealing with each species the author has carefully and critically collated all the previous observations, in many cases little enough.

The cowbirds belong to three genera, Agelaioides (2 spp.), Molothrus (3 spp.), and Tangavius (2 spp.). These and their many geographic races are dealt with in detail and good keys for their differentiation are provided. It is interesting to note that the species of the first genus are non parasitic. Not only that but all the genera belong to the family Icteridæ, or Hangnests, in which nest-building reaches practically the highest point of its development. From this the author concludes that we must seek to derive the parasitic cowbirds from a typical nest-building, egg-incubating type of bird.

The main part of the book is composed of well-illustrated accounts of the various birds, together with very full records of their mating, laying, and migration habits. Beyond noting the accuracy of the descriptions, it is only possible to congratulate the author upon his assiduity and the interesting nature of the facts he has revealed. The most common and best known of the North American Cowbirds is *Molothrus ater*, and of this species and its races a map of distribution is provided. We should like to say that the dotted line indicating the "probable" northern boundary is more accurate than that indicating the recorded distribution. We have not only observed this species, probably as *M. ater artemisiæ*, up to this northern limit on the east and west sides of Lake Winnipeg north of the Lake of the Woods and in Jasper National Park, but have also recorded it from an island in Lake Winnipeg north of this limit again.

Not the least interesting part of the book for the general reader is the general discussion on the origin and evolution of the parasitic habit at the end of the book, and to this unfortunately we cannot do justice in the limits of a review. This is a subject closely connected with breeding areas, protecting instinct, nesting habits, and the like. Briefly the author concludes that Cowbirds are descended from non-parasitic ancestors, the parasitic habit has been developed, or, outside America, we may say been *evolved* separately in the different groups of birds in which it is found, and that the cause of the appearance of the parasitic habit has been the disruption of the typical synchronisation or sequence of area-taking, nest-building, egg-laying, incubating, and protecting.

Beyond doubt this is an interesting and useful contribution to the subject and the more so since theorising has been subordinated to observation. The book is well written and well produced.

C. H. O'D.

Economic Biology: A Text for Students of Agriculture and General Biology. By George P. Weldon. [Pp. xi + 457, with 191 text figures.] (London: McGraw-Hill Publishing Co., 1929. Price 12*s.* 6*d.* net.)

"This text has grown out of the author's experience in teaching to juniors and seniors in the high school." The high school pupils who have the good fortune to study under the writer of this book have many advantages, for it is obviously written with enthusiasm, covers a wide field, and carries the applications of biology as far as possible. So far as we know, it is a much more thorough course than is given in this country, and indeed many graduates of our universities do not know a large part of the fundamental practical biology that it contains. It suffers, as do all books covering a wide field, from lack of depth and a terseness of statement that sometimes give an erroneous impression. Thus on p. 267 we read of the heart, ". . . some of the muscular tissue which enters into its structure resembles that of the voluntary muscles." Without modification this statement is so inaccurate that it would be better omitted. Again, on p. 271, regarding the conditions surrounding the first men, it is stated, "Plant and animal life, *little resembling the present forms*, was abundant." The italics are ours, the grammar the author's. Is it really necessary to introduce a homily on smoking and drinking (pp. 277–8) in a book of this sort?

C. H. O'D.

A Handbook of the Dragonflies of North America. By James G. Needham and Hortense Butler Heywood. [Pp. viii + 378, with numerous illustrations.] (Baltimore: Charles G. Thomas; London: Baillière, Tindall & Cox, 1929. Price 31*s.* 6*d.* net.)

The contents of the present volume are divided into two parts. The first is a general introduction to the external structure and the life-history of dragonflies, and while the former is not so detailed as given in Tillyard, it is ample for its purpose, which is to enable the reader to make use of the keys. This part is very readable and contains much interesting information.

The second and main part of the book is a systematic account of the dragonflies of North America, together with keys to the families, genera, and species and bibliography of the relevant literature supplementing and bringing up to date that in Muttkowski's Catalogue of 1910. In all 360 species distributed among seventy-five genera have been recorded for the area, so that a very considerable task is involved. Not only are the adults treated, but wherever possible the nymphs as well, and interspersed with the descriptions are accounts, either from other writers or very frequently from the authors themselves, of outstanding points in the habits or distribution of the species. It has not been found possible to test the book thoroughly—a long task—but it has been done in a few instances, and the results were very satisfactory. A number of excellent illustrations in black and white of the important features, wings, venation colour patterns and genitalia are provided, and add greatly to the usefulness of the book.

The volume is intended for collectors or those who have to identify species for ecological or other purposes, and in this field is indispensable. Apart from this, however, it furnishes an up-to-date record of the North American species, and authors and publishers have combined to produce a book that might well serve as a model for this type of work. The result could only have been so successful in the hands of a master such as the senior author.

C H. O'D.

Anatomy and the Problem of Behaviour. By G. E. COGHILL. [Pp. xii + 113, with 52 figs.] (Cambridge, The University Press, 1929. Price 7*s*. 6*d*. net)

THIS book consists of a series of three lectures delivered at University College, London. It is based on two parallel lines of research, carried out chiefly on Amblystoma; namely, the development of the behaviour pattern and the origin and development of the neural mechanisms in the embryos and young larvæ. The correlation of these two aspects of development provides some remarkably interesting results. It is shown in the first lecture that the development of the behaviour pattern takes place in a regular sequence of movements, which can be correlated with the development of the nervous system. The functions of aquatic and terrestrial locomotion and feeding follow, in a precise manner, the development of the neural mechanisms controlling these actions. Behaviour develops from the beginning by the gradual expansion of an integrated total pattern. Within this total pattern partial patterns (reflexes) become individuated and acquire various degrees of discreteness. This conception of the origin of partial patterns within a total pattern is in marked contrast to the conception which has gradually grown up that reflexes are discrete phenomena. The second lecture deals with the origin of neural structures and the part played by pre-neural structures and functions in their development. The author is in agreement with Childs in attributing the regulation of the development of the nervous system to physiological gradients and in considering them as the dynamic antecedents of the neural mechanisms. The third lecture is largely devoted to a discussion of the results and their interpretation.

The book as a whole admirably illustrates the importance and value of closely correlating morphological and physiological findings. Thus, and thus only, can a true conception of either structure or function be obtained. Recent years have tended to remove Anatomy from Physiology, and the result has been detrimental to both. Many misconceptions would have been avoided, and a truer conception of the living organism would have resulted, if these two great subjects had been more closely co-ordinated in the past than they have been. Prof. Coghill, realising that this correlation was nowhere more important than in the study of the nervous system, has achieved results of fundamental importance. His book, which is clearly written and easily read, will be of interest to psychologists and metaphysicians as well as to biologists. It is essentially stimulating and demands thought.

F. W. R. B.

Creation by Evolution. Edited by FRANCES MASON. [Pp. xx + 392, with numerous illustrations.] (New York: The Macmillan Co., 1928. Price 21*s*. net.)

THIS is a compilation by a number of eminent English and American biologists. Its chief aim seems to be to combat anti-evolutionist propaganda and to convince people that the doctrine of evolution provides a world in which, in the words of one of the contributors, "the religious man can breathe freely." It seems strange that the doctrine of evolution should have been felt to have offered special difficulties to religious respiration when we consider that most of these difficulties were presented in the Book of Job, long before anyone dreamt of evolution, and are sufficiently plain in ordinary life. It seems strange, too, that biologists should have continued to take so much interest in this controversy, or have thought it worth their while to keep reiterating the usual arguments. It would not be difficult to bring forward reasons for believing that the persistence of this controversy has done more harm than good to biological science, in so far as it has diverted attention from the *scientific* criticism of the doctrine itself. A doctrine

which is perpetually on the defensive is not likely to tolerate criticism from its own side. This may account for the fact that so little progress has been made in clarifying biological notions relating to evolution, as will be evident from a perusal of this book. There is considerable disagreement about the meaning of the term " evolution " itself. No appreciation is shown of the logical peculiarities of evolutionary knowledge as contrasted with the branches of biology which are not historical. Thus we frequently find the expression " law of evolution," which seems to be entirely meaningless. If it is intended to assert that all organic races are perpetually changing into different ones then this does not appear to be the case. Thus W. M. Wheeler says: " We are therefore unable to detect any significant evolution of the ants as a whole during the millions of years of Tertiary time." Yet H. S. Jennings says: " The doctrine of organic evolution is the doctrine that animals and plants are slowly transforming, producing new kinds; that they have done this in the past and are continuing to do it now." The failure to recognise the different types of scientific propositions is also illustrated by the following passage (p. 13): " Although we do not know of any competent biologist to-day, however skeptical and inquiring he may be, who has any doubt as to the fact of organic evolution, yet no one would assert that it can be demonstrated as one might demonstrate the law of gravitation, or the conservation of matter and energy, or the development of a chick out of a drop of living matter on the top of the yolk of the egg." It will be evident, after a little reflection, that the three doctrines here referred to are quite different in their relations to what is perceived, and that consequently one cannot speak of " demonstration " in regard to them in the same sense in all cases. In the case of evolutionary assertions we have a fourth and different type. We have a large number of assertions of the type: " Here and there are found fossils having certain determinate characteristics. If they are the remains of extinct animals, and if the rocks in which they are found have been deposited in a certain temporal order, and if the later organisms were genetically derived from the earlier with modification, then we can understand roughly how the present state of affairs in any particular group for which we have such data has come about." But this is historical knowledge asserting what has actually once happened in the past, not inductive knowledge at all in the sense in which the other examples are inductive, although of course inductive knowledge is used in obtaining this historical knowledge, as when an historian infers that certain marks on a parchment were made by ink and by human hands. The failure to distinguish these different kinds of knowledge is responsible for much of the disagreement regarding " recapitulation." There seems to be plenty of scope for further clarification of our thoughts on these matters.

One of the most enthusiastic essayists says that he understands by evolution " the universal process of orderly change." He insists that all these changes " are *orderly*, never random nor accidental," and that " such phrases as ' blind force ' have no real meaning." Many people would agree with the last assertion, but it is difficult to reconcile the others with the fact that the Mendelian doctrine in its modern form requires the assumption that the homologous chromosomes assort in pairs at random (with respect to their parental origin) during the meiotic division, and that fertilisation between different kinds of germ-cells also occurs at random. Moreover, the same notion also occurs, of course, in the doctrine of natural selection. This author states, too, that as this " universal process " " occurs throughout all that we know, evolution becomes another name for Nature." But if change were quite so universal it is difficult to see how knowledge would be possible. That $2 + 2 = 4$, and that black is different from white, do not appear to change, hence either this process is not quite so universal as this author states, or Nature is not all that we know.

The notion of progress plays a considerable part in some of these essays. This, again, introduces propositions of quite a different type from either historical or inductive ones. In relation to evolutionary occurrences the use of this notion presupposes a temporal series in which we can discriminate "stages"—*a*, *b*, *c*, *d*, etc., and such that (1) *a* is earlier than *d*, and (2) *d* is *better than a*. It thus involves value judgments, and not simply judgments concerning magnitude, but until we know more about such judgments, particularly in relation to natural scientific problems, until, that is to say, we know what we mean by "better than" in situations other than those in which human beings are involved, we cannot make much progress with the understanding of "progress." This book is beautifully produced, the printing and illustrations being excellent. J. H. W.

Haliotis. L.M.B.C. Memoirs, XXIX. Liverpool Marine Biology Committee. [Pp. viii + 174.] (Liverpool: at the University Press, 1929. Price 10*s*. 6*d*. net.)

The latest addition to the well-known *L.M.B.C. Memoirs* is one of the best of the series, and the reason for it is certainly due to the fact that Dr. Crofts has studied her material in the field as well as in the laboratory. The notes on the life of Haliotis, or "Ormer," are extremely valuable and give us much information that was not before known. Although there has been considerable over-fishing of this mollusc in the Channel Islands, especially in Guernsey, it is still very abundant, and the author states that on some of the islands fourteen dozen can easily be obtained at good spring-tides, and that on one occasion the catch of a party on one small beach was estimated at nearly two thousand. Since 1898 the gathering for import and export of ormers less than three inches in largest diameter has been prohibited.

Nearly all the work of this monograph has been done on *Haliotis tuberculata*, the English Channel species, which inhabits the deeper parts of the littoral zone and the shallow waters beyond, but must always have clean water of suitable temperature and salinity. It is difficult to keep the adults alive when removed from their natural home, but the young can be kept in bowls for several months with daily changes of water. Young from two millimetres to seven millimetres in length, hitherto rarely seen, were found living at exceptionally low tides under stones in protected gullies. Both young and adult are protectively coloured so as to resemble their surroundings, the minute forms coral-pink mottled with grey and white like Lithothamnion and Spirorbis amongst which they live, their soft parts almost transparent and pale greenish, the older animals resembling the larger rocks and the growths which cover them. Experiment showed that white light is invariably avoided, whilst red light has no effect. The young forms were important in the study of the pedal glands, for it is found that only in those between three and four millimetres in length and under the anterior pedal gland is well developed; at eight millimetres it has almost disappeared. This explains why other workers have been unable to find it, although sub-epithelial gland cells still remain. This gland is regarded by the author as useful in assisting in adhesion in the post-larval stage where the pedal sole is so minute that the usual method of adhesion by a vacuum cannot be used. Many other disputed points are cleared up, and the whole work is a most useful contribution to the study of the Mollusca. A special point to be mentioned is the accompaniment of "notes for dissection" to the various parts. This is a real boon to both student and demonstrator. The illustrations are good, but perhaps err slightly on the small side in the anatomical drawings of the plates. Details of the life-history are left for a future occasion, and we shall look forward to an account of the larval life which is so little known. M. V. L.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Foundations of Geometry and Induction. By Jean Nicod. Containing Geometry in the Sensible World and the Logical Problem of Induction. With Prefaces by Bertrand Russell, F.R.S., and André Lalande. London: Kegan Paul, Trench, Trübner & Co.; New York: Harcourt, Brace & Company, 1930. (Pp. 286.) Price 21*s*. net.

Solid Space-algebra. The Systems of Hamilton and Grassmann Combined. By Sir Ronald Ross, K.C.B., K.C.M.G., F.R.S., with Extracts from his Paper, The Algebra of Space, 1901. London: Harrison & Sons, 44 Martin's Lane, W.C.2. (Pp. iv + 70.) Price 10*s*. 6*d*. net.

The Universe Around Us. By Sir James Jeans, M.A., D.Sc., LL.D., F.R.S. Cambridge: at the University Press, 1929. (Pp. x + 352, with 24 plates). Price 12*s*. 6*d*. net.

The Passage of Electrons through Surfaces and Surface Films. Being the Thirty-first Robert Boyle Lecture delivered before the Oxford University Junior Scientific Club on May 28, 1929. By R. H. Fowler, F.R.S. London: Oxford University Press, 1929. (Pp. 21.) Price 1*s*. net.

Handbuch der Experimentalphysik. Band 22. Herausgegeben von W. Wien und, F. Harms. Zeemaneffekt, von E. Back. Ergebnisse und Anwendungen der Spektroskopie, Ramaneffekt, von G. Joos. Mit 51 Abbildungen. Leipzig: Akademische Verlagsgesellschaft, m.b.H., 1929. (Pp. x + 436.) Price 41 R.M., geb. 42·80 R.M.

Sydney University Reprints. Series XI. Physics, Mathematics, and Astronomy. Nos. 1–7, 1924, Nos. 8–19, 1927. Sydney: at the University Press.

Der Kampf um das Kausalgesetz in der Jüngsten Physik. By Hugo Bergmann. Braunschweig: Friedr. Vieweg und Sohn. Akt.-Ges. 1929. (Pp. viii + 78.) Price 5.60 Rm.

Müller-Pouillets Lehrbuch der Physik. II Auflage, Zweiter Band, Lehre von der Strahlenden Energie (Optik). Unter Mitwirkung Zahlreicher Gelehrter Herausgegeben von A. Eucken, O. Lummer, E. Wastmann. Herausgegeben von Karl Wilhelm Meissner, Frankfurt a.M. Braunschweig: Friedr. Vieweg & Sohn Akt.-Ges., 1929. (Erste Teil., pp. xvi + 1709, with 408 figures and 3 plates; Zweiter Teil, pp. xvi + 1710–2392, with 223 figures). Price for both parts 87.50 R.M., bound 95 R.M.

A History of Physics in its Elementary Branches, including the Evolution of Physical Laboratories. By Florian Cajori, Ph.D., Professor of the History of Mathematics in the University of California. Revised and Enlarged Edition. New York: The Macmillan Company, 1929. (Pp. xiii + 424.) Price 15s. net.

At Home among the Atoms. A First Book of Congenial Chemistry. By James Kendall, M.A., D.Sc., F.R.S., Professor of Chemistry, University of Edinburgh. London: G. Bell & Sons, 1929. (Pp. xi + 270, with 16 illustrations.) Price 7*s*. 6*d*. net.

Molecular Magnetism. By W. Peddie, D.Sc., F.R.S.E., Harris Professor of Physics, University College, Dundee. London: Edward Arnold & Co., 1929. (Pp. viii + 140.) Price 10*s.* 6*d.* net.

Die Grundlagen der Quantenchemie. Eine Einleitung in Vier Vorträgen. Von Arthur Haas, Dr.Phil., Professor für Physik an der Universität in Wien. Leipzig: Akademische Verlagsgesellschaft, m.b.H., 1929. (Pp. 74.) Price 3·80 M.

Physics. Part IV. Magnetism and Electricity. By W. J. R. Calvert, M.A., Science Master at Harrow School. London: John Murray, Albemarle Street, W. (Pp. x + 333.) Price 4*s.* net.

Examples in Elementary Physics. By S. R. Humby, M.C., M.A., and F. W. Goddard, M.A., F.C.S. London: Longmans, Green & Co., 1929. (Pp. viii + 165.) Price 2*s.* 6*d.* net.

Leçons sur L'Hydrodynamique. Par Henri Villat, Professeur à la Faculté des Sciences de Paris. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1929. (Pp. iv + 296.) Price 50 fcs.

Everyday Chemistry. By J. R. Partington, M.B.E., D.Sc., Professor of Chemistry at the East London College, University of London. London: Macmillan & Co., St. Martin's Street, 1929. (Pp. 667 + xiii, with 376 diagrams.) Price 7*s.* 6*d.* net.

Sydney University Reprints. Series III. Chemistry, Organic and Inorganic. Vol. I., Nos. 1–32, 1924, Nos. 33–44, 1927. Sydney: at the University Press.

A School Certificate Chemistry. By G. H. J. Adlam, M.A., B.Sc., Senior Science Master, City of London School. London: John Murray, Albemarle Street, W. (Pp. x + 334.)

Hydrogen Ions. Their Determination and Importance in Pure and Industrial Chemistry. By Hubert T. S. Britton, D.Sc., D.I.C., F.I.C. Being Vol. III of a Series of Monographs on Applied Chemistry. Under the Editorship of E. Howard Tripp, Ph.D. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1929. (Pp. xiv + 515, with 107 figures.) Price 25*s.* net.

Analytical Processes. A Physico-chemical Interpretation. By T. B. Smith, A.R.C.S., B.Sc., Assistant Lecturer in Chemistry at the University of Sheffield. London: Edward Arnold & Co., 1929. (Pp. viii + 373.) Price 12*s.* 6*d.* net.

The Essentials of Chemical Physiology. For the Use of Students. By W. D. Halliburton, M.D., LL.D., F.R.S., J. A. Hewitt, Ph.D., D.Sc., and W. Robson, Ph.D., D.Sc. Twelfth Edition. London: Longmans, Green & Co., 1929. (Pp. xii + 383.) Price 9*s.* net.

Photo-Processes in Gaseous and Liquid Systems. By R. O. Griffith, D.Sc., and A. McKeown, D.Sc. London: Longmans, Green & Co., 1929. (Pp. viii + 691, with 52 diagrams.) Price 25*s.* net.

Chemistry of Food and Nutrition. By Henry C. Sherman, Ph.D., Sc.D., Mitchell Professor of Chemistry, Columbia University. Third Edition, Rewritten and Enlarged. New York: The Macmillan Company, 1928. (Pp. xii + 636.) Price 12*s.* 6*d.* net.

Experimental Chemistry. By A. C. Williams, B.Sc. London: Longmans, Green & Co., 1929. (Pp. xii + 141, with 54 figures.) Price 2*s.* 6*d.* net.

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Sedimentary Petrography. With Special Reference to Petrographic Methods of Correlation of Strata and to Subsurface Oil Geology. By Henry B. Milner, M.A., D.I.C., F.G.S., M.Inst.P.T. London: Thomas Murby & Co., 1 Fleet Lane, E.C.4; New York: D. van Nostrand Co., 8 Warren Street, 1929. (Pp. xxi + 514, with 181 figures.) Price 21*s.* net.

Our Restless Earth. An Introduction to the History of the Rocks. By Herbert L. Hawkins, D.Sc., F.G.S., Professor of Geology in the University of Reading. London: George Routledge & Sons, 68 Carter Lane, E.C., 1929. (Pp. 76, with 7 figures.) Price 6*d.* net.

The Platinum Deposits and Mines of South Africa. By Percy A. Wagner, D.Sc., D.Eng., F.G.S. With a Chapter on the Mineragraphy and Spectography of the Sulphidic Platinum Ores of the Bushveld Complex by Prof. Dr. H. Schneiderhöhn. Edinburgh: Oliver & Boyd, Tweedale Court, 33 Paternoster Row, E.C.4. (Pp. xv + 326, with 38 plates and 37 figures.) Price 21*s.* net.

Deposition of the Sedimentary Rocks. By J. E. Marr, Sc.D., F.R.S., Woodwardian Professor of Geology in the University of Cambridge. Cambridge: at the University Press, 1929. (Pp. vii + 245.) Price 7*s.* 6*d.* net.

Patterns for a Series of Fourteen Block Models. Illustrating Geological Structures. Second Series, with Descriptive Notes. By Frank Smithson, Ph.D., F.G.S. London: Thomas Murby & Co., 1 Fleet Lane, E.C.4; New York: D. van Nostrand Co., 8 Warren Street, 1929.

The Structure of Asia. Edited by J. W. Gregory, LL.D., D.Sc., F.R.S., Professor of Geology in the University of Glasgow. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xi + 227, with 8 illustrations, 18 maps, and 18 diagrams.) Price 15*s.* net.

The Structure and Life of Forest Trees. By Dr. M. Büsgen, Professor in the Royal Prussian Forest Academy in Hann-Münden. Third, Revised and Enlarged Edition by Dr. E. Münch, Professor in the Forest Academy at Tharandt. English Translation by Thomas Thomson, M.Sc., University Lecturer in Forestry, University College of North Wales, Bangor. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1929. (Pp. xi + 436, with 173 figures.) Price 30*s.* net.

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Practical Botany. An Elementary Course in the General Morphology and Physiology of Plants, Especially Adapted for the Requirements of Students at the University of Manitoba. By A. H. Reginald Buller, F.R.S., B.Sc., Ph.D., etc. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4. (Pp. viii + 275.) Price 6*s*. net.

The Application of Science to Crop-Production. An Experiment carried out at the Institute of Plant Industry, Indore. By Albert Howard, C.I.E., M.A., Director of the Industry of Plant Industry, Indore, and Agricultural Adviser to States in Central India and Rajputana, and Gabrielle L. C. Howard, M.A. Second Imperial Economic Botanist. Oxford: at the University Press, 1929. (Pp. iv + 81, with illustrations.) Price 9*s*. net.

Man and Animals in the New Hebrides. By John R. Baker, M.A., D.Phil. London: George Routledge & Sons, 68 Carter Lane, E.C., 1929. (Pp. xiv + 200, with 45 figures.) Price 12*s*. 6*d*. net.

Self-sterility. By E. M. East, Harvard University, Bussey Institution. Reprint from Bibliographia Genetica V., 1929. The Hague: Martinus Nijhoff. (Pp. 331–370.) Price F. 2.

Vererbungserscheinungen bei Pilzen. By H. Kniep, Reprint from Bibliographia Genetica V., 1929. The Hague: Martinus Nijhoff. (Pp. 372–478.) Price Fcs. 5·60.

Genetics of Brassica. By Edmund Malinowski. Reprint from Bibliographia Genetica V., 1929. The Hague: Martinus Nijhoff. (Pp. 26.) Price Fcs. 1·60.

The Genetics of the Tettigidæ (Grouse Locusts). By Robert K. Nabours. Reprint from Bibliographia Genetica V., 1929. The Hague: Martinus Nijhoff. (Pp. 27–104.) Price Fcs. 4·40.

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Outlines of Zoology. By J. Arthur Thomson, M.A., LL.D. Eighth Edition. Revised. London: Oxford University Press, 1929. (Pp. xxviii + 972, with 328 illustrations.) Price 21*s*. net.

Heredity in Man. By R. Ruggles Gates, Ph.D., LL.D., F.L.S., F.R.A.I., Professor of Botany in the University of London, King's College. London: Constable & Company, 1929. (Pp. xiii + 385, with 87 illustrations.) Price 24*s*. net.

Laboratory and Field Ecology. The Responses of Animals as Indicators of Correct Working Methods. By Victor E. Shelford, Professor of Zoology, University of Illinois. London: Baillière, Tindall & Cox., 8 Henrietta Street, W.C.2, 1929. (Pp. xii + 608, with 219 figures.) Price 45*s*. net.

New Views of Evolution. By George Perrigo Conger, Ph.D., Associate Professor of Philosophy, University of Minnesota. New York: The Macmillan Company, 1929. (Pp. ix + 235.) Price 10*s*. 6*d*. net.

The Sceptical Biologist (Ten Essays). By Joseph Needham, Fellow of Caius College, Cambridge, and University Demonstrator in Biochemistry. London: Chatto & Windus, 1929. (Pp. 288.) Price 7*s*. 6*d*. net.

The Chromosomes in Man. Sex and Somatic. By Herbert M. Evans and Olive Swezy, Berkeley. California: University of California Press, 1929. (Pp. 64, with 11 plates.) Price $1·50.

Fungi and Fungous Diseases. (Adolph Gehrmann Lectures of the University of Illinois College of Medicine 1926.) By Sir Aldo Castellani, M.D., F.R.C.P., K.C.M.G., Director of Tropical Medicine and Dermatology, Ross Institute, etc. Chicago: American Medical Association, 535 North Dearborn Street. (Pp. 203, with 44 figures.)

Climate and Acclimatisation. By Sir Aldo Castellani, K.C.M.G., M.D., F.R.C.P. (Reprinted from the *Journal of Tropical Medicine and Hygiene*, July 1 and 15, 1929.) London: John Bale, Sons, & Danielsson, 83 Great Titchfield Street, W.1. (Pp. 25.)

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The Chemical Aspects of Immunity. By H. Gideon Wells, Ph.D., M.D., Professor of Pathology, University of Chicago. Second Edition. Revised and Enlarged. New York: The Chemical Catalog Company, 419 Fourth Avenue, at 29th Street. (Pp. 285.) Price $6.

The Mechanism of Enzyme Action and Associated Cell Phenomena. By F. F. Nord, Fellow, Physiological Institute, Tierzerzliche Hochschule, Berlin. London: Baillière, Tindall & Cox, 8 Henrietta Street, Covent Garden, W.C.2, 1929. (Pp. x + 78, with 7 figures.) Price 9*s*. net.

Problems of Neurosis. A Book of Case-histories. By Alfred Adler, with a Prefatory Essay by F. G. Crookshank, M.D., F.R.C.P. Edited by Philippe Mairet. London: Kegan Paul, Trench, Trübner & Co., 68 Carter Lane, E.C., 1929. (Pp. xxxvii + 178.) Price 8*s*. 6*d*. net.

The Extrapharmacopœia of Martindale and Westcott. Revised by W. Harrison Martindale, Ph.D., Ph.Ch., F.C.S. Nineteenth Edition. In Two Volumes. Vol. II. London: H. K. Lewis & Co., 1929. (Pp. xxxviii + 759.) Price 22*s*. 6*d*. net.

Problems in Electrical Engineering with Answers. Edited by S. Parker Smith, D.Sc., M.I.E.E., Professor of Electrical Engineering, the Royal Technical College, Glasgow. London: Constable & Company, 1929. (Pp. viii + 206.) Price 6*s*. net.

Steam and Other Engines. By J. Duncan, Wh.Ex., M.I.Mech.E., Head of the Department of Engineering at the Municipal College, West Ham. Revised and Enlarged Edition. London: Macmillan & Co., St. Martin's Street, 1929. (Pp. xi + 536, with 338 diagrams.) Price 6*s*. net.

Overhead Power Lines. Elementary Design and Calculations. By Captain W. Morecombe, R.E., B.Sc., A.M.I.E.E. London: Chapman & Hall, 11 Henrietta Street, W.C., 1929. (Pp. xi + 236.) Price 15*s*. net.

Mechanics of the Gyroscope. By Richard F. Deimel, M.A., Associate Professor of Mechanics, Stevens Institute of Technology. New York: The Macmillan Company, 1929. (Pp. ix + 192, with 73 figures.) Price 17*s*. net.

Gauges and Fine Measurements. By F. H. Rolt, M.B.E., B.Sc. With Introductory Note by J. E. Sears, Jne. Edited by Sir R. T. Glazebrook, K.C.B., F.R.S. Vol. I, Standards of Length, Measuring Machines, Comparators. Vol. II, Limit Gauges, Measuring Instruments, General Methods of Measurement. London: Macmillan & Co., St. Martin's Street, 1929. (Pp. Vol. I, xv + 366, with 253 figures; Vol. II, viii + 357, with 349 figures.) Price 42*s*. net.

Manual of Second and Third Order Triangulation and Traverse Department of Commerce, U.S. Coast and Geodetic Survey. Special Publication No. 145. Washington: U.S. Government Printing Office, 1929. (Pp. v + 226.) Price 70 cents.

The Trauma of Birth. By Otto Rank. London: Kegan Paul, Trench, Trübner & Co.; New York: Harcourt, Brace & Company, 1929. (Pp. xv + 224.) Price 15*s.* net.

Photographic Emulsions. Their Preparation and Coating on Glass, Celluloid and Paper, Experimentally and on the Large Scale. By E. J. Wall, Hon.F.R.P.S., F.C.S. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1929. (Pp. viii + 245.) Price 21s. net.

Short Stories in Science. By J. G. Crowther. London: George Routledge & Sons, 68 Carter Lane, E.C., 1929. (Pp. viii + 213.) Price 5s. net.

Studies in the History of Statistical Method. With Special Reference to Certain Educational Problems. By Helen M. Walker, Ph.D., Assistant Professor of Education, Teacher's College, Columbia University, Baltimore; The Williams Wilkins Company; London: Baillière, Tindall & Cox, 1929. (Pp. viii + 229, with 10 plates.) Price 22*s.* 6*d.* net

Science and Thought in the Fifteenth Century. Studies in the History of Medicine and Surgery, Natural and Mathematical Science, Philosophy and Politics. By Lynn Thorndike. New York: Columbia University Press, 1929. (Pp. xiii + 288, with 10 plates.) Price 24*s.* net.

The World, the Flesh and the Devil. An Enquiry into the Future of the Three Enemies of the Rational Soul. By J. D. Bernal, M.A., Lecturer in Structural Crystallography, University of Cambridge, London: Kegan Paul, Trench, Trübner & Co.; New York: E. P. Dutton & Co., 1929. (Pp. 96.) Price 2*s.* 6*d.* net.

The Book of the Moth. Edgware, Middlesex: The De Haviland Aircraft Co. (Pp. 37.) Illustrated. Price 1*s.* 6*d.* net.

Select Methods of Metallurgical Analysis. By William Archibald Naish, Ph.D., A.R.S.M., B.Sc., and John Edward Clennell, B.Sc. With an Introduction by Sir H. C. Harold Carpenter, F.R.S. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1929. (Pp. xii + 495, with 32 figures.) Price 30*s.* net.

Letters from Rome on Certain Discoveries connected with Malaria. By T. Edmonston Charles, M.D., Q.H.P., and Addenda, consisting of an article by S. Calandruccio, letters from Robert Koch and A. Laveran, and a Statement by Lord Lister. Edited, with a Preface and Remarks by Sir Ronald Ross, K.C.B., K.C.M.G., F.R.S. (Pp. 78.)

The Future of the Earth. By Harold Jeffreys, M.A., D.Sc., F.R.S. London: Kegan Paul, Trench, Trübner & Co., Broadway House, Carter Lane, E.C., 1929. (Pp. 72.) Price 2*s.* 6*d.* net.

School Laboratory Management. By Arthur Sutcliffe, M.A., B.Sc., A.I.C. London: John Murray, Albemarle Street, W.I. (Pp. ix + 272.) Price 4*s.* 6*d.* net.

Studies in Commerce Refrigeration. London School of Economics and Political Science, University of London. Editors, Sir W. H. Beveridge, K.C.B., and Prof. A. J. Sargent. Vol. V, Refrigeration as Applied to the Transportation and Storage of Food Products. By E. W. Shanahan, M.A., D.Sc. London: Gee & Co., 6 Kirby Street, Hatton Gardens, E.C.1. (Pp. x + 96.) Price 5*s.* net; post free 5*s.* 4*d.*

The Scientific Examination of Pictures. An Investigation of the Pigments used by the Dutch and Flemish Masters from the Brothers van Eyck to the Middle of the Nineteenth Century. By Dr. A. Martin de Wild. With a Foreword by Prof. Dr. F. E. C. Scheffer, Professor of Inorganic Chemistry at the Technical University of Delft, Holland. Translated from the Dutch by L. C. Jackson, M.Sc., Ph.D. London: G. Bell & Sons, 1929. (Pp. xv + 106, with 46 plates.) Price 15*s.* net.

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SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

PHYSICS. By L. F. BATES, B.Sc., Ph.D., F.Inst.P., University College, London.

The Relation between Matter and Radiation.—In the issue of SCIENCE PROGRESS for July 1928, experiments were described in this section which supported the view that a moving electron behaved as a group of waves whose velocity and wave-length were determined by the speed and mass of the electron. We then saw that for an electron which had traversed a potential difference of 25,000 volts, the calculated wave-length was $0{\cdot}75 \times 10^{-9}$ cm., *i.e.* of the same order of magnitude as that of hard X-rays. Now, the de Broglie theory is not restricted to electrons, and we may extend the calculation to any moving particle. In fact, if we considered a beam of protons which had traversed the same potential difference, we should have an equivalent wave-length of $1{\cdot}75 \times 10^{-11}$ cm. In spite of the huge experimental difficulties, A. J. Dempster (*Nature*, **125**, p. 51, 1930, *Phys. Rev.*, **34**, p. 1493, 1929) has succeeded in showing that a beam of hydrogen canal rays produces a complex pattern on a photographic plate after reflection at almost grazing incidence from the cleavage plane of a calcite crystal. A regular pattern of curved and straight lines is formed ; it is not perfectly symmetrical about the undeviated spot, and, in fact, the lines resemble the secondary lines frequently observed when using the rotating crystal method of X-ray analysis. If it is assumed that the pattern is formed by the diffraction of the canal rays at the two-dimensional grating formed by the rows of atoms in the crystal surface, then the positions of the lines agree with those expected on the de Broglie theory. On the application of a magnetic field the whole pattern is shifted, showing that it is formed by protons. In Dempster's experiments, the maximum potential difference across the discharge tube was 40,000 volts, and magnetic analysis showed, as would be expected, that the canal rays possessed velocities distributed over a wide range. Now each velocity should be represented by a corresponding wave-length, and the lines in the pattern should correspond

to the diffraction patterns for these wave-lengths; in fact, should be a kind of velocity spectrum of the protons. Now, we stated above that the de Broglie theory does not allow us to differentiate between charged and uncharged particles, and we should expect charged and uncharged particles to form the same spectrum if they possessed identical velocities. We should, however, expect the two sets of particles to behave differently in a magnetic field. Actually, however, the whole pattern was observed by Dempster to shift on the application of a magnetic field, although considerable numbers of uncharged particles must have been present in the beam. This might, of course, have been due to successive capture and loss of electrons by the canal rays, but the possibility of this would depend upon the design of the apparatus, of which we await further details with considerable interest.

Prof. G. P. Thomson has continued his investigation of the diffraction of cathode rays by thin films. He finds that by using cellulose acetate as a base on which to sputter, that it is possible to make metal films of thickness of the order of 10^{-6} cm. which are surprisingly free from holes. In this way he has obtained films of silver, lead, iron, nickel, tungsten and aluminium. In the last two cases the diffraction rings were not good; tungsten showed only one very poor ring which was probably formed by a compound of tungsten, but the aluminium rings appeared to fit the usual structure. Nickel films behaved in an unexpected manner, for they showed hexagonal closest packing instead of the usual face-centred cubic packing. This is interesting, as it is the first case of the discovery of a new crystalline structure by means of electron diffraction (and incidentally it may throw some light on the question of the production of non-magnetic films of nickel). It was found that the lowest velocity electrons which would produce distinct rings with the metal films used were those having velocities corresponding to 10,500 to 15,500 volts. As a matter of fact, a film of some compound of lead, formed by sputtering lead on to a rock salt base, gave rings with an electron velocity corresponding to 5,800 volts. These results are in marked contrast with those of Rupp (*Ann. der Phys.*, **1**, p. 773, 1929), who obtained good rings with electrons of only 180 volts. The difference may be due to the possible occurrence of patches of exceeding thinness in Rupp's films. Again, however, Thomson found that the variation of the minimum voltage for the production of rings with increase in film thickness was surprisingly small. The intensity of the rings depends on a number of factors, such as the volume of the crystal, the number of planes giving rise to diffraction, the corresponding wave-length, the scattering per electron and the number of

scattering electrons per atom. The analogous problem has been thoroughly tackled in the case of X-rays, and Thomson shows, by photometric measurements of the diffraction rings in gold, that the curve giving the angular variation of electron scattering for individual atoms is similar to the " F " curves for X-rays.

Rupp (*Ann. der Phys.*, **5**, p. 497, 1929) has recently investigated the diffraction of electrons by ionic crystals, NaCl, KCl and KBr. A beam of electrons was directed upon the face of a crystal and a kind of Bragg method of electron reflection was employed. The angle of incidence was kept constant, and the electron velocity varied. The crystals all showed diffraction maxima which fitted the de Broglie relation. In the case of NaCl one maximum of unknown origin was observed. It was found that the index of refraction for electrons in such crystals did not appreciably differ from unity, so that no detectable internal lattice potential exists. The curves showing the distribution of velocity among the electrons in a reflected beam of maximum intensity were distinctly different from those obtained with pure metal crystals, the velocity losses being more distributed over the whole range of velocity in the former case.

So far the methods of investigation described here have required photographic exposures of several hours' duration or delicate electrometer measurements. Recently, however, Dauvillier (*Nature*, **125**, p. 50, 1930) has succeeded in making electron diffraction rings visible by causing a beam of electrons to pass through a thin film of zinc oxide and then to fall upon a fluorescent screen. The film was prepared by a method described by Ponte (*Compt. Rend.*, **188**, p. 244, 1929). The two principal rings of zinc oxide were easily visible when a current of less than a milliampère was passed through the tube under a potential difference of 8,000 volts. The diameters of the rings could easily be observed to vary with the potential difference applied to the tube.

Supraconductivity.—The interesting work of Kapitza on the effect of magnetic fields on the resistance of metals and the bearing of his results upon the problems of supraconductivity, recently described in this section, has stimulated W. Meissner and H. Scheffers (*Phys. Zeit.*, **30**, p. 827, 1929) to carry out measurements on very pure gold down to the temperature of liquid helium, using magnetic fields up to 13,000 gauss. Two resistances were made from single crystals of gold and a third was made from pure gold drawn as a wire. Let us for convenience call these resistances Nos. 1, 2, and 3 respectively. No. 1 was so pure that its residual resistance in liquid helium was only 0·0003 of its value at 0° C. The corresponding

figures for Nos. 2 and 3 were 0·001 and 0·035 respectively. For the measurements at liquid helium temperatures the gold resistance and a coil of 1,500 turns for producing the magnetic field were built directly into the liquefier. Measurements taken at the temperature of liquid nitrogen showed that the values of $\Delta R/R_0$ with H for the first two specimens could be represented by the same curve within the limits of experimental error, and the values for the third resistance were quite close to this curve, ΔR being the change of the resistance in a transverse magnetic field, H, and R_0 the resistance at 0° C. At the temperature of liquid hydrogen, however, the three specimens gave rise to three separate curves, the values of $\Delta R/R_0$ for specimen No. 1 being more than twice as high as those of No. 2. The values for No. 3 were not greatly different from those for No. 2, but if values of $\Delta R/R$ were compared there was no similarity between the two specimens. The authors consider that curves of $\Delta R/R$ against H are far too much influenced by the presence of impurities in the specimens, and therefore give a false picture of the effect of the magnetic field, and that since R_0 is not so much affected by residual impurities, the curves of $\Delta R/R_0$ are much more reliable. At 4·2° K the values of $\Delta R/R_0$ for No. 1 are more than twice as great as those for No. 2, although, unfortunately, the measurements for the first specimen are not so reliable as those for the second. At this temperature the resistance of No. 3 actually decreased on the application of a weak magnetic field, and this decrease was attributed to the presence of a small quantity of iron, probably introduced as a surface impurity in the drawing of the wire. The variation of $\Delta R/R_0$ with the field for temperatures between 1·3 and 4·2° K was comparatively small. On the whole, we may say that the greater the impurity of the specimen and the higher its temperature the greater the curvature of the graph of $\Delta R/R_0$ against H. This graph becomes approximately straight for high values of the magnetic field; at 20·4° K this was certainly the case for specimen No. 1 for fields of 8,000 to 13,000 gauss, although under similar conditions the graph for No. 2 was still curved. It will be remembered that Kapitza obtained straight portions in his curves even at the temperature of liquid nitrogen, with the intense fields at his disposal. The interesting deduction which the authors make from these experiments is that the quantity $\frac{1}{R_0} \cdot \frac{dR}{dH}$ varies very little with temperature, in marked contrast to the variation of the quantity $\frac{1}{R} \cdot \frac{dR}{dH}$ employed by Kapitza and other workers.

By extrapolation from curves obtained at the temperature

of liquid nitrogen, Kapitza obtained a quantity $\frac{1}{R_t} \cdot \frac{dR}{dH}$, where R_t represents the resistance possessed by an ideally pure metal. This quantity he used in his discussion of the behaviour of the various elements of the periodic table. The authors do not consider this quantity satisfactory for this purpose, and they suggest the use of the quantity $\frac{1}{R_\theta} \cdot \left(\frac{dR}{dH}\right)_\theta$, *i.e.* the value of $\frac{1}{R} \cdot \frac{dR}{dH}$ obtained at the characteristic temperature, θ, of the element under consideration. (*Cf.* Grüneisen, *Phys. Zeit.*, **19**, p. 382, 1918, and Siebel, "Elektrizität in Metallen," *Sammlung Goschen.*) The characteristic temperature of the metal is calculated from Debye's theory of atomic heats. They go even further, and suggest the use of the atomic resistance for the above purpose, using Simon's expression (*Zeit. für Phys. Chem.*, **109**, p. 136, 1924) for this quantity, *viz.*, $\rho_\theta \cdot V^{-1/3}$, where ρ_θ is the specific resistance and V the atomic volume, ρ_θ being measured at the characteristic temperature of the metal. The evaluation of $\frac{d\rho_\theta}{dH} \cdot V^{-1/3}$ showed that it was approximately constant for members of the same group of the periodic table, omitting certain obvious exceptions, such as the ferromagnetic metals and the elements arsenic, antimony, and bismuth.

The experimental results obtained by Meissner and Scheffer approximately fitted the hyperbola

$$H^2 = \frac{b^2}{a^2}\left(\frac{\Delta R}{R_0}\right)^2 + 2\frac{b^2}{a}\left(\frac{\Delta R}{R_0}\right)$$

for a given temperature, where a and b are the intercepts of the asymptote on the $\Delta R/R_0$ and H axes respectively, and correspond to Kapitza's $\Delta R_0/R_0$ and H_k. For specimen No. 1 the values of a for the temperatures 4·2, 20·4, and 78·5° K are respectively $1{\cdot}8 \times 10^{-4}$, $1{\cdot}2 \times 10^{-3}$, and $2{\cdot}8 \times 10^{-2}$, so that a cannot be regarded as independent of the temperature. The corresponding values of b were 370, 2,400, and 50,000 gauss, whereas Kapitza considered 3,000 gauss to be the probable value for gold. Now, since a varies so much with temperature, Kapitza's view that for an ideal metal—*i.e.* one with no residual resistance—the curve of $\Delta R/R_t$ against H ought to be a straight line even for small values of H, cannot be supported, for there is clearly no relation between a and the residual resistance. The fact remains, however, that the slope of the asymptote increases with decrease in residual resistance. It should be pointed out that the introduction of a hyperbola formula means that, even for comparatively small

values of H, we cannot write $\Delta R/R_0$ proportional to H^2, as the most recent theories appear to require, and the theoretical meaning of the formula is not clear.

Finally, is the establishment of the supraconducting state only prevented by the existence of residual resistance, *i.e.*, would all metals be supraconductors if we could obtain them so pure that their residual resistance was zero? Meissner and Scheffer answer this question in the negative. They quote the Leiden results for lead, and show that the resistance of this supra-conductor at 7° K is already very much less than the resistance of an ideally pure metal—as calculated from Grüneisen's formula for the variation of resistance with temperature below 20° K—would be at 7° K. They therefore consider that supraconductivity arises from some unknown mechanism. The writer of this article shares their conviction, and considers that the existence of supraconducting alloys of bismuth and gold, two metals which are themselves not supraconductors, affords very great support to this view. Again, considerable support is provided by Meissner's own experiments (*Zeit. für Phys.*, **58**, p. 570, 1929) on the supraconductivity of copper sulphide. He measured the resistance of one of the copper sulphide wires prepared by Fischbeck and Dorner (*Zeit. für Am. Chem.*, **181**, p. 372, 1929), who exposed a heated copper wire to the action of the vapour of boiling sulphur. Such a wire, about 5 cm. long and 0·9 mm. diameter, was used, current being supplied to the wire through clamps. It was found that, between the temperatures of 1·66 and 1·55° K, the resistance of the wire fell to such a small value that it could not be measured, whilst the actual residual resistance just prior to the establishment of supraconductivity was only $4{\cdot}4 \times 10^{-3}$ times that at 0° C., *i.e.* the residual resistance was of the order of magnitude as that obtained in the case of very pure metals—a very remarkable result. We may therefore safely conclude that no satisfactory picture of the process of supraconductivity yet exists.

Miscellaneous Contributions.—At the meeting of the German Physical Society in Prague last September, E. Brüche (*Phys. Zeit.*, **30**, p. 815, 1929) contributed a paper on his experiments on the passage of very slow electrons through methane, ethane, propane, and butane. He emphasised that, as the length of the hydrocarbon chain increased, the physical properties showed a corresponding marked change, and the object of the experiments now described was to determine whether the direct examination of these molecular fields by the passage of slow electrons would show parallel changes as the length of the hydro-carbon chains increased. The results obtained with the Ramsauer apparatus, particularly as set forth in the clear

schedule in Brüche's paper, provide a convincing answer in the affirmative. The curves representing the variation of effective molecular cross-section plotted against electron velocity all show the same characteristics, with a very well-defined maximum value for the same electron velocity in each gas. It is shown that a model similar to Longmuir's cube model enables the theoretical curve for each of the gases to be deduced if the curve for methane is known, Such calculated curves are in excellent agreement with the experimental curves, and the calculations on which they are based lead to a value of 2·3 Å for the distance between the carbon nuclei.

Incidentally, Ramsauer and Kollath (*Ann. des Phys.*, **3**, p. 536, 1929) have extended Ramsauer's measurements of the effective cross-sections of rare gas atoms, using electrons with velocities as low as that corresponding to 0·16 volt. Naturally, precautions were taken to exclude contact differences of potential and traces of moisture. It was found that with electrons possessing velocities of less than 1 volt, helium possessed a more or less constant effective cross-section, whilst argon, krypton, and xenon possessed effective cross-sections which fell to extremely low values, and then rose rapidly. The minimum for argon was at 0·3 to 0·4 volt, and those for krypton and xenon were in the neighbourhood of 0·6 volt.

A very neat and accurate method for the quick determination of magnetic susceptibilities of powders is described by W. Sucksmith (*Phil. Mag.*, **8**, p. 158, 1929). A phosphor-bronze strip is bent into the form of a ring or circular hoop. The ring is mounted vertically, its uppermost point being fixed. To the lower surface is attached a stiff wire carrying a glass phial, in which the powder is placed. A small scale-pan is fixed to the upper end of the wire. On placing the phial in a non-uniform magnetic field, a downward displacement is produced, and the ring is slightly distorted. This distortion is magnified by an optical system consisting of two plane mirrors suitably mounted on the ring, so that a line source of light may be focused on a vertical scale after reflection from the mirrors. The displacement of the lower surface of the ring may thus be magnified 150 times. A direct relation exists between the force and the displacement produced, and the apparatus may be calibrated by using a phial containing pure water or a powder of known susceptibility. The placing of slight loads in the scale-pan facilitates the accurate setting of the apparatus.

In these pages we recently discussed a simple method for the measurement of the ratio of the specific heats of a gas devised by Rückhardt. The apparatus consisted of a glass tube so accurately bored that a perfectly spherical steel ball would

slide in it almost without friction. The tube was mounted vertically and attached to a large closed reservoir of gas, and the period of oscillation of the ball was used to determine the above ratio. Now R. Rinkel (*Phys. Zeit.*, **30**, p. 805, 1929) shows that the same apparatus may be used in a more simple and accurate manner. It is only necessary to release the ball and to measure accurately its maximum distance of fall. The ratio is given by the expression $\frac{mg}{b} \cdot \frac{v_1}{a^2} \cdot \frac{2}{x}$, where mg is the weight of the ball, b the barometric pressure, v the initial volume of the gas, a the area of cross-section of the tube, and x the distance the ball falls. Friction produces an apparent diminution in the weight of the sphere of about 3 per cent.

The vexed question of the amount of energy, ϵ, necessary for the production of a pair of ions when a cathode ray passes through air has recently been investigated by Eisl (*Ann. der Phys.*, **3**, p. 277, 1929). As he points out, the number of ions produced per cm. of path of the ray is now known fairly accurately from the work of Buchmann (*Ann. der Phys.*, **87**, p. 509, 1928), but the experimental values of ϵ obtained by different workers vary from about 30 to 45 volts; moreover, it is quite impossible from these values to state how ϵ depends upon the velocity of the ray. Eisl used cathode rays with velocities up to that corresponding to 60 K.V., a fairly homogeneous beam of rays of known velocity being isolated by the magnetic spectrum method. The beam passed through a celluloid window into an ionisation chamber, due allowance being made for the retarding effect of the window, based on measurements of magnetic deflections recorded photographically in a special apparatus. The cathode ray and ionisation currents were found by an electrometer method. It was found imperative that the window through which the rays entered the ionisation chamber should be connected to the negative pole of a battery, and not earthed. The total ionisation was found to be directly proportional to the kinetic energy of the cathode ray. The energy required for the production of a pair of ions was therefore strictly independent of the velocity of the ray, and was found to be $32{\cdot}2 \pm 0{\cdot}5$ volt.

PHYSICAL CHEMISTRY. By R. K. SCHOFIELD, M.A., Ph.D. (Cantab.), Rothamsted Experimental Station, Harpenden.

Molecular Spectra.—The recent development of wave mechanics, by revolutionising the interpretation of molecular structure, has opened up new avenues of approach to the fundamental problems of chemistry. Besides leading to the recognition of a new kind of isomerism dependent on the symmetry of protonic spin, molecular or band spectra are

providing information about the spacial arrangement of the atoms in molecules and crystals, about the nature of the bonds that hold molecules together, about the electronic configuration of molecules in their normal and activated states, and about the mechanism and energy relations involved in chemical reactions.

According to the quantum theory of radiation, the emission of radiation from a molecule is accompanied by a change in the molecule from a state of higher to one of lower energy, the frequency of the radiation emitted being proportional to the energy given out. The reverse occurs when radiation of the correct frequency is incident and a quantum is absorbed. A molecule, like an atom, may be at one of a series of energy levels characterised by different electronic configuration. In most (but not all) cases the energy differences between these levels amount to several volts, so that spectra due to transformations between these electronic levels are usually in the visible or ultra-violet. A molecule also has a series of quantised vibrational levels. In a diatomic molecule the nuclei alternately approach and recede from one another, while in polyatomic molecules and crystals the motions are more complex. The energy differences here are usually small and the corresponding spectra are generally in the infra-red. They consist of bands which, under high dispersion, are found, more especially in the case of gases, to be made up of a series of lines. The energy differences between these lines are those of transformations between different quantised rotational levels.

From the spacing of the rotation lines the principal moments of inertia of the molecule can be calculated. For linear molecules two of these are equal and one zero, for atoms in a plane $I_1 = I_2 + I_3$. Thus it is possible to decide whether the atoms of a molecule are in line, coplanar, or distributed in three dimensions. A beginning has already been made in this direction by Mecke, Eucken, Rawlinson, and others. Further research will be necessary before we have much definite information from this source, but the outlook is very promising.

The rotation spectra of diatomic molecules, particularly of elements, have been of service in the study of isotopes. The molecule $Cl^{35}-Cl^{37}$ gives rise to lines with wider spacing than those due to $Cl^{35}-Cl^{35}$ owing to its greater moment of inertia. In this way O^{17} and O^{18} which are present with O^{16} in quantities too small for detection in the mass spectrograph have been discovered, although there is only one O^{17} to 10,000 of O^{16}. When the nuclei of a diatomic molecule are of equal mass the rotation lines sometimes alternate in intensity (H_2) while in other cases alternate lines are missing (He_2).

In the case of hydrogen, wave mechanics predicts the existence of two isomers distinguished by their nuclei spins. One can exist in rotational states 1, 3, 5, etc., the other in states 0, 2, 4, etc. Each isomer gives rise to half the lines. At ordinary temperatures the first kind is three times as abundant as the second. At very low temperatures, on the other hand, state O is the most stable, and if the change is catalysed by charcoal in liquid air or by an electric discharge, hydrogen passes over almost entirely to the second state. On returning to room temperature such a sample of hydrogen does not at once return to its normal 3 to 1 mixture, but, in the absence of a catalyst, only does so in the course of weeks. Its physical properties such as specific heat differ from that of normal hydrogen. The latter has long been known to give an anomalous variation of specific heat with temperature, and previous to the application of wave mechanics Dennison had suggested that it is a 3 to 1 mixture of two distinct gases. A similar prediction by wave mechanics is confirmed by an examination of the spectrum of ammonia, but there is a discrepancy in the case of nitrogen which remains to be cleared up.

While the gaseous state is the simplest one in which to examine rotational lines owing to the freedom of the molecules, it is convenient in dealing with vibrational bands to start by considering crystals on account of the regularity of atomic arrangement. In crystals of the type of KCl, X-ray analysis shows that each element is arranged in a simple cubic lattice, each lattice being face-centred with respect to the other. Modern atomic theory renders it certain that the units are the potassium and chloride ions. In executing vibrations it seems that the K^+ lattice remains rigid, as does the Cl^- lattice, but that they vibrate against one another. The "fundamental" frequency of this vibration is that of the powerful absorption band (63·4 for KCl) in the neighbourhood of which the crystal reflects the "residual rays" of Rubens. Similar vibrations occur with more complex crystals such as $CaCO_3$ and K_2SO_4, the anion behaving as a unit. With crystals of lower symmetry it is found that different absorption bands appear according to the direction in which the radiation traverses the crystal (giving rise to different refractive indices). Since this is due to a variation of the natural frequency according to the direction of the mutual vibration of the two ionic lattices, information is thereby gained about the variation of the interionic forces. Where groups such as CO_3'' and SO_4'' are present vibration occurs within these groups. The case of CO_3'' has been studied in detail by Schaefer. X-ray analysis by the Braggs showed that the three oxygen nuclei

are at the corners of an equilateral triangle with the carbon nucleus at the centre of gravity. Several vibrations are possible. The carbon nucleus can oscillate with respect to the oxygens at right angles to the plane of the group. Oscillations can also occur in the plane, one set of levels giving rise to doublets. This is attributed to a slight lack of symmetry, too small to be detected by X-ray analysis, so that the triangle is not strictly equilateral, but isosceles. This may prove to be of importance in deciding whether all three oxygen atoms are similarly linked to the carbon or whether the diagram $O = C\langle^{O^-}_{O_-}$ of the older structural chemistry has any significance. One mode of vibration is of special interest. Here the carbon is stationary and the oxygen triangle alternately expands and contracts. No infra-red band corresponding to this frequency has been found, and its presence had originally to be inferred from the existence of bands which did not correspond to combinations of other known frequencies. Support was given to the existence of this "inactive" frequency by the discovery of a similar state of affairs with NO_3'. Moreover its "inactive" frequency is identical with that of CO_3''. This is reasonable since the moving system (the oxygen triangle) has the same mass in each case; nevertheless it is interesting to find a change in nuclear charge having so little effect on the forces acting on the oxygens. Vibrations of central atoms, on the other hand, have frequencies proportional to the nuclear mass. A similar relationship is found between SO_4'' and ClO_4', frequencies due to the oxygen nuclei (1,092 and 1,080 cm.$^{-1}$ respectively) being nearly equal, those due to the central nuclei (627 and 720) differing considerably. The corresponding values of the former frequencies for CrO_4'' and MnO_4' (870 and 888) indicate a weaker binding of the oxygens in these cases. This is probably due to a wider spacing of the oxygens brought about by increasing the size of the central atom. A. M. Taylor has pointed out that MnO_4' has a lower frequency in aqueous solution than in the crystalline state. A decrease in frequency does not occur when $KMnO_4$ is dissolved in ethyl acetate, in which solvent the salt is but slightly dissociated. The effect in aqueous solution is considered to be due to the effect of hydrating water molecules on the field in which the oxygen atoms vibrate.

In the case of permanganate the frequencies are most easily determined from the visible absorption spectrum. The existence of an absorption spectrum in the visible region (giving to permanganates their characteristic colour) is due to the presence of two electronic levels of not very great energy

difference. As, however, a change in vibrational level usually accompanies an electronic change the spectrum consists, not of a single band, but of a series of bands, each frequency difference being equal to that of an infra-red band. The value of vibration frequencies can also be obtained from the recently discovered Raman effect. On passing a powerful beam of mono-chromatic light through a (transparent) solid, liquid or gas, the scattered light is found to consist not only of light of the incident frequency, but also of other bands of higher and lower frequency. The frequency differences between these bands and that of the incident radiation is (generally) equal to that of an infra-red absorption band. The bands of altered frequency are very feeble in comparison with the scattered light of unaltered frequency, and still more feeble by comparison with the incident radiation. This is why they were not discovered earlier. Although the relative intensity of these bands is not obtained so directly as in the infra-red, the frequency differences can be measured very accurately. Also the line bands are polarised to different degrees. The reason for this is not entirely understood at present, but it is likely to throw still further light on spacial arrangement in crystals. A particularly interesting discovery is the appearance of the "inactive" frequencies of CO_3'' and NO_3' in the Raman spectra. Cases are also known in which well-known infra-red frequencies do not appear in the Raman spectrum.

The vibration spectra of liquids and amorphous solids are least easy to interpret. This is because the molecules are in an intermediate condition, neither completely free as in gases nor held in fixed orientation as in crystals. They are subject to fields of force which are variable and consequently the bands are usually rather broad and diffuse. Nevertheless, on account of the relative ease with which they can be obtained, the infra-red absorption spectra of liquids have received considerable attention. Water shows well-defined bands at 1·5, 2, 3, 4·7, and 6μ. The 3μ band studied by the Raman method becomes sharper in strong aqueous sulphuric acid, sharper still in ice, and even sharper in gypsum (which contains water of crystallisation). It has long been known that the infra-red absorption spectra of members of a homologous series of organic liquids show remarkable similarities. It is only very recently, however, that it has been possible to identify bands with particular groups such as C—H, N—H, C=O, C—N, C—OH. These are all associated with the vibration of a single atom (or small group). Vibrations of carbon atoms in a chain are obviously more complex and have not yet been definitely identified. Since, in the former case, the masses of the atoms are known, the frequencies give information about the chemical

bond that unites them to the molecule. The similarity of the C–H and N–H bonds is seen in the closeness of the corresponding frequencies. R. W. Wood has recently discovered from Raman spectra a fine structure in the C–H bands, which differs from compound to compound. Thus it appears that differently situated H-atoms have slightly different frequencies. Further work along these lines will be of great interest when taken in conjunction with the known chemical effect of substitution in one part of a molecule on the properties of another part.

No extensive work has yet been carried out on the change in molecular spectra on passing from liquid to vapour, though a successful investigation along these lines might lead to important results, particularly as to the structure of molecular associates in liquids. One remarkable feature of recent progress in the interpretation of band spectra of gases is the definite identification of a number of diatomic molecules which have not as yet been obtained in bulk in a pure state. These include CH, OH, and C_2 (giving the bluish colour to coal-gas flames and the latter giving the " swan " bands); CO^+, CN, and N_2^+ (in the tails of comets); NH, CaH, BO and others (in sun-spots); Na_2, K_2, and NaK (in the metallic vapours), and He_2 (high-current discharge in helium at low pressure), to mention some of the more remarkable. It appears that OH takes a part in H_2O equilibrium at high temperatures, while the formation of CN is also favoured by high temperatures in carbon and nitrogen equilibrium.

Of no less importance to chemistry is the light thrown upon the nature of valence. It is found that the higher harmonics of the vibration of linked atoms are not exact multiples of the fundamental. Kratzer has shown that the anharmony varies with the nature of the valence bond. In general the older electron theories receive striking confirmation. Where the bond is an electro-valent attraction between oppositely charged ions there is very little anharmony, but with typical co-valent linkages the anharmony is considerable. The new method of approach indicated, however, that intermediate types probably exist. The essential correctness of the theory of Lewis that a co-valent bond consists of a pair of electrons has been brought out in the work of Heitler and London. It is now recognised that the electrons of an atom are spinning. Two conditions are possible according to the direction of the spin. In a completed electronic shell the electrons are paired and the resultant spin zero. In an incomplete shell some may be paired, but others not. The unpaired electrons are the valence electrons. In the formation of molecules electrons unpaired in the uncombined atoms become paired, each pair thus formed constituting a valence

bond. The lines of atomic spectra are singlets, doublets, triplets, etc., according to whether there are 0, 1, 2, etc., unpaired electrons in the outer shell. The case with molecules is analogous, so that from a study of band spectra the number of electrons still unpaired can be ascertained. Thus N_2 and CO have none, NO, CN, and N_2^+ have one, while O_2 has two and is therefore only linked by one bond. Unpaired electrons lead to para-magnetism, which is well known with NO and O_2.

Energies of dissociation can be found both from the limit of vibration bands in the visible and ultra-violet and from a consideration of electronic levels in simpler cases. The matter is involved owing to the possibility of both molecule and dissociation products existing in activated as well as normal states. Readers who are new to the subject and wish to pursue it should consult R. S. Mulliken, *Chemical Reviews*, December 1929. Those desiring further details will find the Faraday Society *General Discussion on Molecular Spectra* (September 1929) a mine of information and can obtain from it full references.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., Ph.D., University, Glasgow.

Petrography—Igneous Rocks.—In his study of the Bushveld Igneous Complex (Transvaal), Prof. R. A. Daly (*Bull. Geol. Soc. Amer.*, **39**, 1928, pp. 703–68) supplements the older petrographical data by no fewer than eighteen chemical analyses. The upper felsite, granite, and granophyre are shown to be strikingly similar in composition, and are regarded as the compound superficial part of a giant lava flow, an areal eruption, the lower part of which constitutes the main norite beneath. The salic layer and the norite are treated as the poles of a gravitative differentiation similar to that of many other igneous bodies. Many large slabs and blocks of quartzite and shale that are enclosed at the contact of the coarse red granite and main norite are regarded as gigantic xenoliths of Transvaal sediments. Prof. Daly provisionally outlines the history of the Bushveld Complex, and raises many questions which can only be answered after the accumulation of more data.

A study of the rock-association and chemistry of the Bushveld Complex has led Dr. E. Reuning (*N.J. f. Min., B.-B.* **57**, Abt. A, 1927, pp. 631–64) to reject the theory of differentiation *in situ* as the cause of its remarkable banded structure. He favours the view that the differentiated zone is due to the injection, as successive sills, of numerous magmatic fractions which originated in a deep-seated part of the Bushveld igneous hearth.

The Pilansberg, occurring within the Bushveld Complex,

and one of the largest alkaline igneous masses in the world, has been comprehensively mapped and studied by Prof. S. J. Shand (*Trans. Geol. Soc. S. Africa*, XXXI, 1928, pp. 97–156). It proves to be a huge ring complex, composed of concentric rings of foyaite and syenite, partially covered by a series of volcanic rocks consisting of trachytes and phonolites. The ring structure is explained by the subsidence of a solid cake of red foyaite into a laccolithic body, with the concomitant welling-up of successive rings of syenite and foyaite towards the exterior—a mechanism difficult to imagine, and very different from the mechanism invoked to explain the ring structures of the West of Scotland. The petrology of the rocks is very fully dealt with.

F. Dixey and W. Campbell Smith have described the rocks of the Lupata Gorge and of the North Side of the Lower Zambezi (*Geol. Mag.*, LXVI, 1929, pp. 241–59). There is a lower rhyolite series and an upper alkaline series. The Upper Lupata sandstones are unconformable to the rhyolites below, and overstep on to the Lower Lupata sandstones and even on to the Karroo lavas. Further petrographical descriptions of the igneous rocks are given. The rhyolite is of normal type with, however, a decided predominance of potash over soda. The alkaline lavas include phonolites, kenytes, " blairmorites " with large phenocrysts of analcite, and rhyolites.

In describing some Mesozoic and Tertiary Igneous Rocks from Portuguese East Africa (*Geol. Mag.*, LXVI, 1929, pp. 529–40), Mr. F. P. Mennell deals with Karroo basalts and intrusive dolerites, and further with the Lupata volcanics mentioned in the preceding paragraph. He describes in particular a nepheline-feldspar-porphyry or *lupatite*, which forms two huge sheets near the lower end of the gorge. A rock apparently identical with the " blairmorite " of Dixey and Smith, Mennell regards as having been originally of the nature of leucitophyre.

In his thorough-going comparison of the Keweenawan Sill Rocks of Sudbury and Cobalt (Ontario), which includes much new mineralogical as well as petrographical data, Dr. T. C. Phemister (*Trans. Roy. Soc. Canada* (3), XXII, 1928, pp. 121–96) suggests that these " diabase-gabbro " intrusions represent differentiation of a primary magma, the remainder of which has given rise to anorthosite and granophyre. These rocks are similar to the quartz-dolerites and gabbros of Scotland and elsewhere, and Dr. Phemister also points out a parallelism with the " Jotunnorite " series of Norway. The enormously greater, contemporaneous, neighbouring intrusion of Duluth, which shows the same rock association, receives, however, only a passing mention.

Prof. A. P. Coleman, associated with E. S. Moore and

Prof. T. L. Walker, has made another very thorough investigation of the Sudbury (Ontario) " nickel intrusive " (*Univ. of Toronto Studies: Geol. Ser. No.* 28, 1929, 54 pp.). By several detailed field traverses, aided by intensive microscopical and chemical work, and many determinations of specific gravity, the authors claim to have demonstrated (and the present writer is bound to say that he thinks the claim well founded) that the " nickel intrusive " is a unit formed by the segregation of a magma into a more basic portion, norite, passing without a break into a more acid phase, micropegmatite. Sulphides (pyrrhotite, pentlandite, chalcopyrite) occur abundantly as blebs in the norite with such relations to the other constituents as to show that they are of magmatic crystallisation. The theory of magmatic segregation for the origin of the nickel ores is the only one consistent with the facts. The authors admit secondary hydrothermal action and rearrangement of the ores in the " offset " deposits, but contend that the hydrothermalists completely fail to account for the immense quantities of sulphides scattered through the fresh norite.

A new interpretation of the famous diabase sill at Pigeon Point, Minnesota, is advanced by Prof. F. F. Grout (*Bull. Geol. Soc. Amer.*, **39**, 1928, pp. 555–78), consequent upon the discovery that it exhibits a roof phase with abundant phenocrysts of labradorite, grading into anorthositic gabbro. A few boulder-like masses of pure anorthosite have also been found. Grout favours the view that the labradorite phenocrysts, and the anorthosite aggregates, floated upward in the sill magma at an early stage, on account of their lower specific gravity. The well-marked further differentiation of intermediate and acid (" red rock ") types took place later. The " red rock " had a tendency here, as elsewhere, to stope away the chilled roof in places, and hence occasionally comes into contact with the quartzite roof of the intrusion. Grout believes that not more than a quarter of the granite, and probably much less, is due to assimilation of sedimentary rock. It was formed in great part by ordinary crystallisation-differentiation.

The Pre-Cambrian Saganaga granite of the Minnesota-Ontario border is described by Prof. F. F. Grout (*Journ. Geol.*, **37**, 1929, pp. 562–91) as an oval mass, fifteen by twenty-five miles across, with vertical walls. The main rock is a uniform sodic hornblende-granite. Local phases include syenite, shonkinite, and hornblendite. Table IV (p. 589), intended to show the similarity in chemical composition between hornblendite and basalt, really illustrates their extreme difference.

A composite stock at Snowbank Lake in N.E. Minnesota is, according to C. W. Sanders (*Journ. Geol.*, **37**, 1929, pp. 135–49), mainly syenite, with local phases consisting of nordmarkite,

ægirine-augite-syenite with allanite, quartz-diorite with 13 per cent. of magnetite, and a shonkinite with poikilitic texture. It is of Algonkian age, and is allied to a number of similar stocks in the same region.

The Birch Lake granite batholith (Ontario) is shown by C. Tolman (*Amer. Journ. Sci.*, XVII, 1929, pp. 403–24) to be late Pre-Cambrian in age, and probably a member of the Killarney group of batholiths. It is made up of two slightly differing cognate intrusions of the composition of alaskite. Both the granite and late Pre-Cambrian basic dikes which intersect it have suffered dynamo-metamorphic changes which indicate a hitherto unrecognised period of metamorphism subsequent to the intrusion of the late Pre-Cambrian dikes.

Near Libby, Montana, is a stock of pyroxenite, with nepheline-syenite, and small dikes of granite, which have been described by Prof. E. S. Larsen and J. T. Pardee (*Journ. Geol.*, **37**, 1929, pp. 97–112). The whole intrusion has undergone hydrothermal metamorphism. A large mass of biotite-rock, altered to vermiculite, also occurs.

Two volcanic necks and a number of sills have recently been discovered in Central Arkansas by C. Croneis and M. Billings (*Journ. Geol.*, **37**, 1929, pp. 542–61). These contain a number of remarkable alkaline rocks. The intrusions are of interest because they probably belong to the mid-Cretaceous igneous activity that has produced the diamondiferous peridotite of another Arkansas volcanic vent.

According to G. E. Goodspeed (*Journ. Geol.*, **37**, 1929, pp. 158–76), a small stock of quartz-diorite, intruding feldspathic schists, has reacted with schist inclusions with the production of plagioclase phenocrysts more calcic than those indigenous to the uncontaminated magma. The irruption of this magma into a fissure has formed what the author terms a "reaction porphyry dike."

In his valuable memoir on the Geology of the Marysville Buttes (California), Dr. Howell Williams (*Univ. of California Publ., Bull. Dept. Geol. Sci.*, **18**, 1929, pp. 103–220) has made numerous volcanological observations, and has described an interesting igneous series. The Marysville Buttes consist of an intrusive laccolithic core of andesite-porphyry surrounded by an inner ring of sediments, and an outer ring of andesite tuffs and breccias. After the sedimentary cover had been stripped from the laccolith a volcano arose by steam explosions through the centre of the intrusion. These explosions appear to have been connected with the intrusion of a series of rhyolitic necks into both the andesites and surrounding sediments.

Mt. Monadnock (Vermont), *not* the Mt. Monadnock of physiographic fame, is the most northerly member of the

Novanglian petrographic province, which is the southern continuation of the Monteregian petrographic province of Canada. Nineteen members of the combined provinces are now known. Prof. J. E. Wolff describes Mt. Monadnock (*Journ. Geol.*, **37**, 1929, pp. 1–16) as a stock of quartz-nordmarkite, with an interior older mass of essexite, and with associated bostonite and camptonite dikes.

Notwithstanding the enormous bulk of the basaltic lavas and doleritic intrusions of the Gondwana formations in southern South America, which constitute one of the world's greatest eruptive provinces, little petrographic work has been done on these rocks. Hence Dr. K. Walther's memoir (in Spanish, with German summary) is notable as the first full description, and also as containing no fewer than eighteen new chemical analyses of these rocks (*Inst. Geol. Perforac., Bull.* No. 9, 1927, 41 pp. See also *Geol. Zentralbl.*, **38**, 1929, pp. 305–7). The analyses show that the Gondwana basalts and dolerites are all oversaturated with silica, and are identical, both chemically and mineralogically, with the quartz-dolerites and associated lavas of other regions, especially the corresponding region in South Africa.

Extensive work on the volcanoes and Tertiary igneous rocks of Burma, by Dr. H. L. Chhibber, Prof. L. D. Stamp, A. E. Day, and E. S. Pinfold, has resulted in the publication of a long series of papers, as follows: H. L. Chhibber, "The Ancient Volcanoes of Burma," *Journ. Burma Research Soc.*, XVII, 1927, pp. 169–73; H. L. Chhibber and L. D. Stamp, "The Igneous and Associated Rocks of the Kabwet Area, Upper Burma," *Trans. Min. Met. Soc. India*, **21**, 1927, pp. 97–128; E. S. Pinfold, A. E. Day, L. D. Stamp, and H. L. Chhibber, "Late Tertiary Igneous Rocks of the Lower Chindwin Region, Burma," *Ibid.*, pp. 145–225; H. L. Chhibber, "The Igneous Rocks of the Mount Popa Region, Burma," *Ibid.*, pp. 226–310; H. L. Chhibber, "Some Intrusive Rocks of the Pegu Yoma, Burma," *Ibid.*, pp. 338–63. There appear to be four well-marked groups or lines of igneous activity. The rocks of the central and western lines (including the great volcanic pile of Mt. Popa) are of calc-alkaline or "Pacific" character, whilst other centres are partly calc-alkaline and partly "Atlantic." While Mt. Popa seems to be definitely an andesitic volcano, the absence of chemical analyses renders the diagnoses of other types, at any rate as regards their assignment to "Pacific" or "Atlantic" groups, somewhat less certain. Two further memoirs on Burmese igneous rocks are: H. L. Chhibber, "The Rhyolites and Rhyolite Tuffs of Thaton District, Lower Burma," *Journ. Burma Research Soc.*, **16**, 1927, pp. 166–75; H. L. Chhibber, "The Serpentines and

the Associated Minerals of Henzada and Bassein Districts, Burma," *Ibid.*, pp. 176–99.

Many of the newer igneous rocks of north-eastern China, of which little is known, have now been described by Prof. A. Lacroix (*Bull. Geol. Soc. China*, **7**, 1928, pp. 13–59) from his own collections supplemented by others. There are andesites and dacitoids of Jurassic age; dacitoids, rhyolites, granites, monzonites, and diorites of the Cretaceous; basalts, basanitoids, and limburgites of Kainozoic times. The paper contains no fewer than seventy-five new chemical analyses.

The problem of the occurrence of quartz and oligoclase in an olivine-basalt is discussed by T. Ogura on the basis of a Japanese example (*Geol. Mag.*, LXVI, 1929, pp. 68–71). The same quartz and oligoclase phenocrysts also occur in an associated dacite lava. Ogura pictures the subsidence of these minerals from an upper acid magma (dacite) into a lower basic zone, where they became constituents of a basalt.

The greater part of Miss I. A. Brown's paper on the "Geology of the South Coast of New South Wales: 1. The Palæozoic Geology of the Moruya District" (*Proc. Linn. Soc. N.S.W.*, LIII, pt. 3, 1928, pp. 151–92), is taken up by the description of a normal gabbro-diorite-granodiorite-granite plutonic complex. The differentiation, as shown by the ten chemical analyses, is remarkably similar to that of the classic series at Garabal Hill, Loch Lomond. The intrusion most probably took place in late Devonian times.

Prof. J. A. Bartrum has described a series of pillow lavas, probably of uppermost Cretaceous or Palæocene age, with an associated series of peridotite and gabbro intrusions, from northernmost New Zealand (*Trans. N.Z. Inst.*, **59**, 1928, pp. 98–138). A full discussion of the stratigraphical relations of the series and of the overlying Tertiary sediments is given, and the petrography of the igneous rocks is supplemented by three chemical analyses of dolerite, noritic gabbro, and basalt.

Sedimentary Rocks and Sedimentation.—In an important paper on the magnitude of the sediments beneath the deep sea, W. H. Twenhofel (*Bull. Geol. Soc. Amer.*, **40**, 1929, pp. 385–402) estimates that a volume of inorganic sediments approximating to eighty million cubic miles has been deposited in the deep seas. At the same time a large but unknown volume of calcareous sediments, silica, and other substances, has been laid down. Beneath these deep-sea sediments is buried an unknown but possibly large volume of shallow-water and continental-slope sediments, the total thickness of which at any place is a function of the length of time before there was deep sea at that place. Twenhofel suggests that the deep-sea sediments of early geological times were dominantly composed

of insoluble and undecomposed materials, in contrast to the period since the late Mesozoic, during which lime has been an important constituent. This lime is permanently removed from the continental masses.

An interesting paper by Dr. A. Hadding on the "First Rains and their Geological Significance" (*Geol. För. Förh. Stockholm*, **51**, 1929, pp. 19–29) discusses the formation of the earliest sediments. He pictures the earth's surface in pre-aqueous times as covered with a loose, ill-compacted mantle of volcanic ash. The first rains swept an enormous amount of ash into the depressions, where it formed bedded tuff. Later on, leaching of the highly saline material gave rise to limestones and iron ores. Still later began the differentiation of erosion products into clays, sands, limestones, etc. Dr. Hadding identifies the first sediments in the leptite and halleflintas formation of the Lower Archæan of Fennoscandia, which has the appropriate composition, although now highly metamorphic. It also contains limestones and banded ironstones. These are the oldest sediments in one of the oldest parts of the earth's crust, and the rocks beneath them exhibit no indications of sedimentary origin.

Dr. Hadding has also recently published a very comprehensive memoir on the Palæozoic and Mesozoic Sandstones of Sweden (*Lunds Univ. Aarsskrift.*, N.F. Avd. 2, Bd. 25, 1929, 287 pp.), which really amounts to a full treatment of the general petrology of sandstones. This work is a continuation his earlier memoir on the conglomerates of Sweden (see SCIENCE PROGRESS, April 1928, p. 588). The memoir is well illustrated and contains interesting discussions of various kinds of tracks and trails, and other bedding-plane markings in sandstones.

Miss D. L. Reynolds records new occurrences of authigenic potash feldspar in the Trias sandstone of the north of Ireland, the Keuper Marl of Charnwood, the Dolomitic Conglomerate from near Kells, and from the Magnesian Limestone of Roker (*Geol. Mag.*, LXVI, 1929, pp. 390–9). In the Trias sandstone the feldspar appears to have grown from kaolin; but all authigenic feldspars so far recorded have been obtained in limestones, in which kaolin, or its amorphous equivalent, may be present, and act as an absorbent for soluble potash.

J. T. Singewald and C. Milton describe authigenic albite feldspar in a limestone of Trenton age at Glen Falls, New York (*Bull. Geol. Soc. Amer.*, **40**, 1929, pp. 463–8). The limestone appears to be a chemical precipitate which was rich in organic matter. The authors suggest that the feldspar was formed by a slight rise of temperature due to chemical action amongst the constituents of the limestone.

Dr. W. Mackie's latest paper deals with the "Heavier

Accessory Minerals in the Granites of Scotland" (*Trans. Geol. Soc. Edin.*, XII, 1928, pp. 22–40). He lists and describes a large number of minerals : monazite, xenotime, zircon in many varieties, apatite, garnet frequently of phenomenal abundance, magnetite, allanite, epidote, tourmaline, anatase, brookite, rutile, topaz, fluorite, enstatite, hypersthene, staurolite, andalusite, sillimanite, kyanite, olivine, and dumortierite. Allanite and monazite appear to contra-indicate one another ; but, on the other hand, there is a correlation between topaz and fluorite.

Mr. H. C. Sargent returns to the study of chert in a further series of papers (*Geol. Mag.*, LXVI, 1929, pp. 399–413). The present paper deals with the cherts in the Yoredale Series of the North Riding of Yorkshire, and with certain minor occurrences believed to be of magmatic origin. The author believes the Yoredale chert beds to be original contemporaneous deposits of shallow seas. The silica was of inorganic origin, and was precipitated on the sea floor as a gel. In magmatic cherts a ferruginous character seems to be diagnostic in many cases.

Dr. A. Raistrick describes the petrology of the three boulder clays and associated sands, the Basement Clay, Purple Clay, and Hessle Clay, in the cliff section at Dimlington, Yorkshire (*Geol. Mag.*, LXVI, 1929, pp. 337–44). Two sets of distinguishing features were found : first, the actual mineral species present were different in abundance in the different clays ; and secondly, the minerals were strikingly different in size and degree of rounding of the grains. In the Basement Clays the minerals were large and irregular in shape, whereas in the Purple Clay the minerals were small, and often of perfect crystalline form. In the Hessle Clay the characters were to some extent intermediate between these extremes.

The ball clays which are fully described by Dr. A. Scott in a Survey Memoir (*Spec. Repts. Min. Res. Gt. Britain*, XXXI, *Mem. Geol. Surv.*, 1929, 73 pp.) are geologically simply transported china clay deposited in lake basins of Kainozoic age. They possess greater plasticity than ordinary china clay, and are thus of considerable value for certain ceramic processes. The memoir contains a full account of the mineralogy, chemistry, and physical properties of this type of clay.

Mr. J. B. Scrivenor's views on laterite (*Geol. Mag.*, LXVII, 1930, pp. 24–8) may be summarised in the following quotation : " I would suggest that the word 'laterite' be used as little as possible. In Malaya we have typical laterite as defined by Buchanan, but it is only weathered rock impregnated with iron oxide, and contains a large amount of hydrous silicate of aluminium or 'clay.' On the other hand the word is applied popularly to masses of iron oxide and red decomposition products which

are simply ordinary soils coloured by Fe_2O_3. The formation of large quantities of aluminium hydrate may be of importance mechanically in a soil, but it is unnecessary, and incorrect, to associate that with Buchanan's word 'laterite.'"

A. Reifenberg has described the "terra rossa" or red earth of the Mediterranean region, which is a residual product of the weathering of limestones, and has close relations to laterite and bauxite (*Kolloidchem. Beihefte*, Leipzig, 1929, 93 pp.). The memoir is mainly concerned with the colloid chemistry of the formation of terra rossa.

The petrography of Spitsbergen coals is dealt with comprehensively by G. Horn in a memoir published by the Norwegian Spitsbergen Department (*Norges Svalbard- og Ishavs-Undersökelser. Skrifter om Svalbard og Ishavet*, No. 17, 1928, 69 pp.). The most important coals belong to the Palæocene or Eocene. These are almost entirely durain coals with only subordinate amounts of vitrain and fusain. It is not possible to indicate adequately the excellent material on coal petrography contained in this memoir. We may note, however, a valuable section on the petrographic nomenclature of coals.

BOTANY. By E. J. SALISBURY, D.Sc., F.L.S., University College, London.

MISS SAUNDERS continuing her studies on carpel structure concludes that the gynœcium of *Butomus* consists of twelve carpels in two whorls, six sterile and six fertile, and two carpel types are also considered to be present in Hydrocharitaceæ, Aponogetonaceæ, Sparganiaceæ, and Typhaceæ. In the last two-named families the normal gynœcium is held to be bi-carpellary. Only one type of carpel is present in Alismataceæ, Juncaginaceæ, and Potamogetonaceæ (*Ann. Bot.*, July 1929).

M. Pilaud gives interesting details regarding the flowers of *Chlora perfoliata*. These open in the morning from 8 to 9.30 a.m. and close in the afternoon from 4 to 4.45 p.m., the coloration meanwhile intensifying from yellow to orange. In the dark the flowers show a similar rhythm, opening at the same time but closing from one and a half to two hours later. Differences in temperature seemed to have no effect on the time of opening. The mechanism of opening and closing is associated with a difference in structure of the lower and upper epidermis of the petals. The latter consists of isodiametric cells and the former of cells about three times as long as broad. The yellow pigment is not a carotinoid, and they contain an appreciable amount of cesium. Large colonies of this species are described which, however, disappeared at the end of the year and were not replaced by their offspring,

a feature recalling the behaviour of other members of the calcicole flora (*Bull. Soc. Bot. Fr.*, p. 541, 1929).

The embryo development of *Lotus corniculatus* has been studied by M. Sonèges, who finds that the pro-embryo becomes differentiated at an early stage into a terminal embryonic region, a median region giving rise to the hypophysis and a basal suspensor. This latter is of the massive type, consisting of several rows of cells (*Bull. Soc. Bot. Fr.*, p. 527, 1929).

Cryptogams.—An important monograph of 161 pages and 20 plates on the Helicosporous fungi imperfecti has just appeared in the *Annals of the Missouri Botanical Garden* (March 16, 1929) by D. H. Linder. All the species grow best in a neutral or acid medium, and pectin and cellulose proved to be the best source of carbohydrates in artificial cultures. Growth only takes place at high humidities (96–98 per cent.) and the optimum temperature appears to be between 21° and 26° C. The perfect stage *Lasiosphæria pezizula* was found by pure culture of single spores to be a phase of *Helicoma Curtisii.*

The interesting fungus *Neotiella crozalsiana* described by Grelet as parasitic on bryophytes has been recorded by E. J. Corner from the Chilterns growing on *Plagiochila asplenioides.* The fungus is regarded as representing a series of Humariaceous fungi characterised by the presence of carotin in the paraphyses. The normal spores contain three oil globules, a large central and two smaller terminal. The relation of this triguttate type to the biguttate (*e.g. Peziza*), the uniguttate (*e.g. Helvella*), and the eguttate (*e.g. Thelebolus*) is discussed (*Ann. Bot.*, July 1929).

A comparative study of four-spored and two-spored varieties of *Coprinus ephemerus* has been made by Sass (*Am. Jour. Bot.*, November 1929), who finds that in each case the bi-spored strain is normally homothallic and haploid, whilst the four-spored strain is heterothallic and diploid. In *C. ephemerus* the germ tube of the two-spored strain is cœnocytic.

In an account of the algæ of Hispaniola in the same journal Taylor and Arndt describe a new genus of Ceramiaceæ under the name *Actinothamnion* in which the main branches are born alternately and their ultimate ramifications bear whorls of reduced branches which are unicellular.

An account of the meadow, heath, scrub, and birchwood communities of Lapland with particular reference to the light relations is published by Lippmaa (*Acta Inst. et Horti. Bot. Univ. Tartuensis*, vol. ii, 1929). The constituent species are characterised by adaptation to a high light intensity by the presence of sap pigments, pigmented cell membranes, low chlorophyll content, and polished leaf surfaces. A

considerable body of data regarding the leaf pigments of the various species is furnished and also details as to the leaf-structure. The duration of the foliage of some species is considerable, two years in *Andromeda polifolia*, two to three years in *Linnæa borealis*, *Loiseleuria*, *Vaccinium vitisidæa*, eight to ten years in *Juniperus communis*, and from ten to fifteen years in *Lycopodium selago*. The possession of anthocyanin is regarded as a protection against excessive insolation, and the species studied are placed in four categories with respect to this feature. Species which do not form anthocyanin at any season of the year constitute 56 per cent. of the constituents of the birch-meadow (Wiesenbirkenwald) community, but only 19 per cent. of the heath-scrub (Zwergstrauchheide). The latter community is characterised by a high proportion of species which possess anthocyanin throughout the year (23 per cent.), such being absent from the meadow and birch meadow. Species which are green in summer but which develop sap pigments in the young leaves and in the autumn are best represented in the heath scrub. Species of this last category, and continuously green types, constitute the major part of the alpine grassland (Schneewiese).

A. Hakansson (*Hereditas*, XIII, 1, p. 1, 1929) reports that *Salix viminalis* and *S. caprea*, each of which has a haploid complement of nineteen chromosomes, produce a fertile hybrid. The offspring presented two aberrant types. One with giant leaves was triploid ($2n$ 57), the other with $2n = 82$–84 was *Salix laurina* which is markedly sterile. *Salix aurita*, which was also studied, was found to have $n = 19$, whereas the *S. aurita* studied by Blackburn and Harrison was tetraploid. This author in the same journal also records that *Scirpus palustris* from the south-east of Sweden had $n = 19$, whereas plants from Lund had $n = 8$. The only morphological difference between these two strains appears to be that those with the larger number of chromosomes have fruits which are nearly half as long again as those of the plants with the fewer chromosomes.

The extraordinary variability of the colour of dahlias is a fact familiar to everyone, so that the recent investigation of Lawrence (*Journ. Genetics*, August 1929) has particular interest. The colour is dependent on soluble pigments, namely two flavones, yellow and ivory, and probably two anthocyanins, geranium red and purple. The latter are only found in the presence of the former, but the flavones may occur independently, giving an ivory or deep yellow colour to the flower. The additional presence of anthocyanins yields tints ranging from orange and scarlet to purple and magenta. Both types of pigment are present in *D. variabilis*, which is an octoploid with

sixty-four chromosomes. The remaining species are either ivory-magenta-purple (*D. maximiliana*) or yellow-orange-scarlet (*D. coronata*); these have thirty-two or thirty-six chromosomes. It is suggested that *Dahlia variabilis* has arisen by doubling of the chromosomes in a hybrid between members of the two groups.

PLANT PHYSIOLOGY. By PROF. WALTER STILES, Sc.D., F.R.S., The University, Birmingham.

Protoplasm.—The subject of protoplasm has not so far been dealt with in these articles. As considerable attention has been devoted to its properties during recent years, some account of recent work on it will therefore not be out of place.

That protoplasm consists of a colloidal system is now generally admitted, and enquiries into the nature of protoplasm during the last decade have mainly aimed at acquiring information with regard to the organisation of this colloidal system: whether it is a sol or gel, what kinds of substances are involved in it, and how it reacts to various agents.

Information with regard to the question whether protoplasm is a sol or gel has been sought by observations on the viscosity of protoplasm and on Brownian movement within it. For the study of the viscosity of protoplasm the method of microdissection has been employed by two American investigators, R. Chambers and W. Seifriz. The latter's earlier observations ("Observations on the Structure of Protoplasm by Aid of Microdissection," *Biol. Bull.*, **34**, pp. 307–24, 1918) were made on a variety of different material: the plasmodia of Myxomycetes, pollen tubes, oogonia, ova, and embryos of *Fucus*, and the ova of *Echinarachnius*. He found that protoplasm exhibits a wide range of viscosity. In the plasmodia of Myxomycetes, in the young oogonia and embryos of *Fucus*, and in pollen tubes, the protoplasm is described as having the consistency of a liquid, while at the other extreme, in mature and resting eggs of marine organisms, it is said to be very viscous. Reversible changes in viscosity were observed in the ova of *Fucus*, in which there is a rapid increase in viscosity during the last stages of ripening, while on fertilisation there is a return to a more liquid consistency. Later ("Viscosity Values of Protoplasm as determined by Microdissection," *Bot. Gaz.*, **70**, pp. 360–86, 1920) the same worker assigned to different samples of protoplasm viscosity values varying from 1 to 10 on an arbitrary scale derived from observations of the viscosity of prepared gelatin dispersions varying from 0 to 2 per cent. of gelatin in water, the most concentrated of these being a rigid gel. He examined from this point of view a number of Myxomycetes,

Fucus, freshwater algæ (*Spirogyra* and *Vaucheria*), the moulds *Rhizopus* and *Zygorhynchus*, pollen tubes, protozoa, the sand-dollar *Echinarachnius*, and a sea-urchin *Tripneustes esculentus*. His observations serve to emphasise the fact brought out in his earlier work, namely, the very considerable differences in viscosity exhibited by different samples of protoplasm. Thus, " the consistency of Myxomycete protoplasm when in the active vegetative state is liquid," while in the mould *Rhizopus* the protoplasm " is of gel consistency, more usually that of a soft gel, is sticky, quite elastic, and very extensile, closely resembling bread dough in these physical properties." The reversible changes in viscosity that protoplasm undergoes are further described. A Myxomycete plasmodium increases in viscosity as it prepares to fruit. Changes in viscosity also appear to be associated with cell division, at any rate in the division following fertilisation of the eggs of the marine organisms examined, while changes during development of oogonia have already been mentioned. Further, different regions of a single protoplast may exhibit different degrees of viscosity. Thus, in the ciliate *Euplotes* the inner part of the protoplasm, the endoplasm, is very liquid, while the outer part, the ectoplasm, is described as possessed of the firmness of a rigid gel. Smaller localised centres of the protoplasm, the nucleus and chromatophores, also possess a different viscosity from the bulk of the protoplasm.

Some of the conclusions derived from microdissection studies are confirmed by observation of Brownian movement. In a quiescent *Amœba* Seifriz observed that the number of microscopically visible particles in Brownian movement is small and the amplitude of their movement short, while in an active *Amœba* all the microscopically visible particles except the largest exhibit the movement, the amplitude of which is long. This is in agreement with the general conclusion that young active protoplasm is more mobile than quiescent protoplasm, an increase in viscosity taking place as it matures and becomes less active. This conclusion is also supported by earlier observations made with the ultra-microscope. Seifriz's conclusion, that the absence of visible Brownian movement indicates a viscosity above a certain value, has been disputed by L. V. Heilbrunn (*The Colloid Chemistry of Protoplasm*, Berlin, 1928) on the ground that the particles may be so close together that the movement must be so small as to be hardly recognisable. This is the case, for example, in the egg of the sea-urchin *Arbacia*.

The microdissection method, as a method for determining the viscosity of protoplasm, has been severely criticised by Heilbrunn. He says : " For the measurement of viscosity the

microdissection method can at best give only indications of gross differences in viscosity. And even for these it is more or less uncertain." One of the arguments against the usefulness of the microdissection method is that the introduction of a needle into the cell, which it involves, produces injury, and this might alter the viscosity. There is also sometimes vagueness with regard to the criteria used for viscosity determination.

Heilbrunn himself used a centrifuge method for determining viscosity of protoplasm. When eggs of certain organisms are centrifuged it is observed that the granules move to one end of the egg under the influence of the centrifugal force. Since the rate of movement of the granules depends on the viscosity of the protoplasm, by observing the rate of movement of the granules under the influence of a known centrifugal force it is possible to calculate the viscosity of the protoplasm. Using this method Heilbrunn measured the viscosity of the intergranular material of the egg of *Arbacia* and found it to be only about twice that of water, a surprisingly low value. For the clam *Cumingia* a value of between 1 and 4·3 times that of water was found for the viscosity of the intergranular material of the eggs, while at 18° C. the viscosity of the protoplasm of *Amœba dubia* was found to be approximately twice that of water. (L. V. Heilbrunn, "The Absolute Viscosity of Amœba Protoplasm," *Protoplasma*, **8**, pp. 65–9, 1929.)

Earlier measurements of the viscosity of the protoplasm of endodermal cells of *Vicia Faba* made by A. Heilbronn in 1914, by comparing the rate of fall of starch grains under the influence of gravity in the cell and in water, also indicated a comparatively low viscosity of the protoplasm, namely, a viscosity about eight times that of water. Later, Heilbronn described a magnetic method for determining protoplasmic viscosity of Myxomycetes ("Eine neue Methode zur Bestimmung der Viscosität lebender Protoplasten," *Jahrb. f. wiss. Bot.*, **61**, pp. 284–338, 1922). Small iron rods were introduced into the protoplasm, which was then placed in the magnetic field of an electro-magnet. The extent to which a rod is twisted under these conditions depends on the viscosity of the protoplasm. The results, even if not very accurate, indicate that the viscosity of the protoplasm of the various Myxomycetes examined varies from about 9 to 18 times the viscosity of water.

According to F. Weber ("Plasmolyseform und Protoplasmaviskosität," *Osterreich. bot. Zeitschr.*, **73**, pp. 261–6, 1924) the form taken by some plant cells, for example those of *Spirogyra*, when plasmolysed depends on the viscosity. The

form of plasmolysis can then be taken as an index of the viscosity.

Seifriz's work has already indicated that the viscosity of protoplasm can undergo reversible changes. Several workers have attempted to demonstrate such changes in relation to temperature. Earlier work suggested that with increasing temperature viscosity increased until a maximum was reached, after which the viscosity decreased. The most recent work on this subject, that of Heilbrunn ("Protoplasmic Viscosity of Amœba at Different Temperatures," *Protoplasma*, **8**, pp. 58–64, 1929), indicates considerable variation in viscosity in the case of *Amœba dubia*. Up to about 12° C. the viscosity remains about constant with a value in arbitrary units of about 22. With increasing temperature above 12° C. the viscosity rapidly falls until it reaches a minimum of 3 units at 18° C. Above this temperature the viscosity again rises, reaching a maximum of approximately 9 at about 25° C., above which the viscosity falls to a low value (about 2) at 30° C.

There can be no doubt, therefore, that the viscosity of protoplasm varies. But Seifriz has pointed out that physical structure and not viscosity determines whether a system is a sol or a gel, so that observations on the question based merely on viscosity determinations alone require confirmation by other methods of enquiry. An experiment with this end in view is described by Seifriz ("An Elastic Value of Protoplasm, with further Observations on the Viscosity of Protoplasm," *Brit. Journ. Exper. Biol.*, **2**, pp. 1–13, 1924) in which a particle of magnetic metal was suspended in the protoplasm of the egg of *Echinarachnius* and subjected to the attraction of an electro-magnet. The particle returned to its original position after release from the magnetic attraction. This is held to show that the protoplasm is elastic, and from the distance the particle moves under a certain force a measure of the elasticity of protoplasm can be obtained. Seifriz regards this elastic property of protoplasm as of the greatest importance in affording information with regard to its structure ("Elasticity as an Indicator of Protoplasmic Structure," *Amer. Naturalist*, **60**, pp. 124–32, 1926), and he cites various investigations which indicate that colloidal solutions with rod-shaped particles are elastic while those with spherical particles are not. This leads to the conclusion that protoplasm has a fibrous structure. Moreover, the high elasticity it is supposed to display, its rigidity and capacity for imbibitional swelling, are held to indicate that it is in the gel state. This condition would, however, apparently include a system similar to that of a weak solution of gelatin which does not set, for Seifriz says that it "makes little difference what we call protoplasm, whether sol or gel,

so long as we remember that its physical properties are the properties of jellies. If the ability to flow is our criterion of the sol state, then protoplasm is usually, but by no means always, a sol; but there are other indicators of the colloidal state such as elasticity, rigidity, and imbibition, and these are gel characteristics."

Seifriz therefore pictures protoplasm as forming either an entangled mass of fibres or an orderly arrangement of chain molecules, and it is interesting to note that the food substances found in living cells, such as starches, fats, and proteins, are mostly characterised by possessing molecules made up of chains of atoms, whereas a cyclic structure is unusual. It is in the meshes of the fibrous network of the protoplasmic system that such of the food constituents of the protoplasm as are ultra-microscopic are to be found in the form of an emulsion. Seifriz's views are further elaborated in a later paper ("The Contractility of Protoplasm," *Amer. Nat.*, **63**, pp. 410–34, 1929).

Seifriz appears to assume that the fibrous ground substance of protoplasm is protein in character, and W. H. Pearsall and J. Ewing ("Some Protein Properties of Plant Protoplasm," *Brit. Journ. Exper. Biol.*, **2**, pp. 347–56, 1925) take the same view. They find that plant proteins tend to be isoelectric at about *p*H 4·0 to 4·5, and that aggregation and precipitation of the protoplasm of wheat take place at about *p*H 4·1. Tissues such as those of potato and bean cotyledons stain more strongly with basic dyes between *p*H 5 and 7 and with acid dyes at *p*H 3 to 4. If potato tuber tissue is allowed to swell in acid solutions, a marked depression in the amount of swelling is noticed at about *p*H 4·5. Again, in tissues of potato tuber and beetroot coagulation by heat is brought about most rapidly when the acidity is that corresponding to *p*H 4·4.

Heilbrunn, however, in his recent book cited earlier, holds the view that protoplasm is ordinarily a suspensoid. The low values obtained for its viscosity, its granular structure as indicated by ultramicroscopic observations, and its reactions to various agents can be adduced in support of this view. "It seems certain," says Heilbrunn, "that as the physiology of the cell becomes more clearly understood, there will be shown to be a definite dependence of many vital phenomena on the granular nature of protoplasm, on the properties which it possesses by virtue of the fact that it is a suspension."

ZOOLOGY.

By F. W. Rogers Brambell, Ph.D., D.Sc., Lecturer in Zoology, University of London, King's College.

A discovery of significance and far-reaching importance was made by Müller in 1927 when he produced mutations and translocations of the genes and non-disjunction of the

chromosomes in the germ-cells of Drosophila. These mutations produced by X-rays are similar in every way to those occurring in nature, except in their greater frequency. This led Müller to make the startling suggestion that natural mutations may result also from radiations, which are known to be present in nature. If gene mutations are admitted to be the chief cause of the variations in nature, then it follows from this hypothesis that the action of cosmic radiations on living organisms is of great importance in evolution. The fundamental nature of Müller's work is obvious. It is now supplemented by the scarcely less important work of J. T. Patterson on the production of mutations in the somatic cells of Drosophila by means of X-rays (*Jour. Exp. Zool.*, vol. 53, 1929). The sex-linked genes for eye colour were chosen as the most suitable for several reasons. First, because the compound eye is made up of a number of unit ommatidia, in terms of which the area affected by a colour mutation can be expressed numerically. Secondly, the series of multiple allelomorphs for eye-colour allow of different combinations of these colours being used for experimental purposes. Thirdly, the fact that these genes are sex-linked enables them to be combined in either the homozygous or the heterozygous condition in the female, whereas they are always homozygous in the male, as the gene in the single X-chromosome is unaccompanied by its allelomorph. This method of investigation therefore admits of determining the amount of mutation which occurs after a given dose of X-rays and of comparing the effects of different doses. Moreover, the time at which any given mutation occurs can be estimated from the size of the area affected, since the earlier in development the mutation occurs the larger will be the mutant area in the adult. Mutations were produced by irradiation of the eggs and larvæ, but not by irradiation of the pupæ. Moreover, the treatment of the eggs and larvæ was found to have little or no selective effect on the mortality of males or females.

It was found that males from treated cultures, carrying the dominant gene, showed white mutant areas due to gene mutation. Homozygous females from the same cultures showed no change in eye colour. Heterozygous females from treated cultures showed white mutant areas which were due in part to gene mutations and in part to breakage, followed by loss, of the pieces of the X-chromosome carrying the dominant gene.

Females homozygous for eosin and apricot gave light-eosin and light-apricot mutant areas, respectively, that were due to gene mutations. Males from the same cultures gave only white mutant areas, due to gene mutations.

Both sexes showed white areas, and some females showed dark-eosin areas in treated cultures of crosses between eosin females and white-forked males. The white areas in the males and a similar number of white areas in the females were due to gene mutations. The rest of the white areas in the females were due to breakage, followed by loss, in the eosin-bearing chromosome. The dark-eosin areas were interpreted as being due to breakage, followed by loss, in the white-bearing chromosome.

Gene mutations in the homozygous apricot cultures produced white areas in males and light-apricot areas in females. The light areas found in both males and females in crosses between apricot females and white-forked males were explained in a similar manner to those of the eosin cross. Dark-apricot areas and, in a few cases, twin areas, composed of dark apricot and white patches of about equal size and adjacent to each other, were found also in females. These could be explained by assuming either fractional translocation or non-disjunction to have taken place.

The data obtained in the experiments permitted of calculation of the rate of mutation in a given gene. It was found that with the average dose of X-rays employed mutation occurred in one gene in every 9,891. Breakage was found to occur once in every 713 X-chromosomes carrying the dominant gene. An attempt was made to produce reverse mutations in the somatic cells, but only one certain example was found in the eyes of 4,661 treated flies.

D. B. Casteel (*Jour. Exp. Zool.*, vol. 53, 1929) has carried out an histological investigation of the eyes of Patterson's X-ray mutants of Drosophila. He has examined the changes in the arrangement and distribution of the pigment in the ommatidia in the mutant areas.

W. E. Castle and P. W. Gregory have contributed an interesting study of the embryological basis of size inheritance in the rabbit (*Jour. Morph. and Physiol.*, vol. 48, 1929). Comparative study of different races showed more rapid cell multiplication and increase in mass in the embryos of the large races than in those of the small. This tendency toward rapid rate of growth was shown by the results of reciprocal crosses to be transmitted and influenced equally by both the sperm and the ovum. The rate of differentiation was found to be independent of the rate of growth, and to be unaffected by it. Consequently, embryos of a large race attain greater size than those of a small race at corresponding stages of differentiation. The fundamental difference in the rate of growth is evident already in forty-eight-hour embryos, and becomes increasingly clear at later stages. Embryos of

the large race were found to have undergone about one more cleavage than those of the small race at forty-eight hours post-coitum. The former were in the 32-cell stage at this time, while the latter were still in the 16-cell stage.

R. Cumming Robb has arrived at important conclusions concerning the nature of hereditary size limitation in the rabbit (*Brit. Jour. Exp. Biol.*, vol. vi, 1929). He has found that throughout post-natal life the relative weights of the pituitary body, thyroid, thymus, and adrenals in the rabbit may be expressed by the equation $y = ax^k + c$, where y is the weight of the organ in question, x is the body weight, k is the constant giving the differential growth ratio, a is a constant giving the relative initial values of x and y, and c is a constant giving the correction for absolute magnitude.

A similar relation is indicated for Donaldson's data of weights of eyeballs, liver, pancreas, hypophysis, thyroid, adrenals, submaxillary glands, kidney, and fresh skeleton in the rat.

The ultimate proportions of body parts in giant and pigmy rabbits are not the same, but, for any given body weight, corresponding tissues in the two groups tend to exhibit an identical relation to total body mass.

It was found that the adrenals and testes of the Polish rabbits are relatively much larger than those of the Flemish, but that in both the growth of the adrenals approximates to a constant power function of body weight. The growth of the testes in both Polish and Flemish rabbits and hybrids has an identical relation to the weight of the adrenals.

It is suggested that in a growing organism the magnitude of any part tends to be a specific function of the total body mass or of some portion so related to the whole. This relation is explained by surmising that each tissue is in equilibrium with the internal milieu with regard to the distribution of nutrient growth essentials. The equilibrium point in each case would be determined by the nature of the cell, and after differentiation would tend to remain constant. The relative enlargement of each tissue would be limited by the excess of the equilibrium value over the katabolic expenditure.

The equation $y = ax^k + c$ may possess a physical significance according to this hypothesis. Apparently eight types of growth relationships may exist, differing because of the apparent inactivity of one or more constants in this equation.

A. S. Parkes (*Annals Applied Biol.*, vol. 16, 1929) describes the results of an interesting experiment on the growth of young mice suckled by rats. The effect of rats suckling young mice is to provide the latter with a practically unlimited supply of nutrition. It was found that under these conditions the

growth of the young mice was extremely rapid. In one instance, where only two young mice were suckled by a rat, they attained the phenomenal average weight of 17·0 gms. at the age of three weeks. This increase is nearly double that of two young suckled by a mouse. Although increase in size was accelerated, development was not. The eyes opened at the normal time, and at three weeks old the animals were not sufficiently developed to cope with their abnormal body weight, and were in consequence practically immobile. It was concluded from these experiments that the variation in the growth of the various sizes of litters in the normal mouse is purely a question of differential nutrition, and that under conditions of unlimited nutrition the growth of young mice may proceed to a degree which is both unusual and unhealthy.

A. C. Redfield and R. Goodkind (*Brit. Jour. Exp. Biol.*, vol. vi, 1929) have studied the Bohr effect in the respiration and asphyxiation of the squid (*Loligo Pealei*). They measured the oxygen and carbon-dioxide content of the arterial and venous blood. The venous blood was estimated to be 0·13 *p*H unit more acid than the arterial blood. It was shown that the reciprocal effects of oxygen and carbon-dioxide upon the respiratory properties of the haemocyanin accounted for one-third of the respiratory exchange. Asphyxiation occurred when the oxygen and carbon-dioxide pressures were such that the arterial blood could combine with only 0·5 to 1·5 volumes per cent. oxygen. Carbon-dioxide was found to exert no toxic effect, except through its influence on the oxygenation of the blood. It was concluded that the haemocyanin of the blood is of vital necessity to the squid, because the amount of oxygen which can be physically dissolved in blood is less than the amount which is necessary for the maintenance of life.

A case of thyrogenous dwarfism in a Rhode Island Red pullet is described by W. Landauer (*Amer. Jour. Anat.*, vol. 43, 1929). The thyroid was enlarged, and consisted of aplastic tissue without any colloid. The thymus showed an advanced stage of involution. Growth was generally arrested, and the head was brachycephalic, with bulging eyes and swollen tissue surrounding them. There was a myxœdematous swelling on the left side between the eye and the nostril. The skin was dry and the feathers were relatively long.

The skeleton in particular exhibited striking differences from the normal. The perichondral bone, on account of the decreased number of Haversian canals, appeared very compact. Endochondral bone was almost entirely absent from the shafts of the long bones, and only a few bone trabeculæ were found in the epiphyseal ends of the bones. The marrow cells were

decreased in number or missing. The marrow in the humerus had degenerated into reticular connective tissue with a few elastic fibres in it. The condition resembled the human malformation so closely that it was considered to be a case of *myxœdema infantilis*.

The histology of the alimentary tract of the plaice (*Pleuronectes platessa*) is described by Ben Dawes (*Q.J.M.S.*, vol. 73, pt. ii, 1929). The pharynx was distinguished from the œsophagus as a definite region owing to (*a*) the absence of longitudinal musculature, (*b*) the definitely stratified epithelium rich in goblet cells, and (*c*) the presence of structures resembling taste-buds. The detailed structure of these taste-buds was found to be strikingly similar to that of the taste-buds of the mammalian pharyngeal epithelium.

The musculature of the œsophagus was extremely well developed, and was composed of striated fibres. The mucosa of the œsophagus was intensely folded, and contained many goblet cells, but no glands were present, thus clearly defining this region from the stomach.

The musculature of the stomach consisted of non-striated fibres. The glands at the cardiac end of the stomach were more shallow than those at the pyloric end ; otherwise there was no differentiation between them. The gastric mucosa was composed of three types of cells—superficial cells forming the epithelium on the surface and lining the crypts, mucous cells in the necks of the tubules, and granular cells in the basal secretory portions of the tubules. Neither parietal or oxyntic cells nor goblet cells occurred in the gastric mucosa.

The intestinal mucosa was folded, giving an impression of crypts and villi which did not actually occur. The only differentiated elements in the intestinal epithelium were goblet-cells. Leucocytes were always found in small but constant numbers in all zones of the intestinal epithelium, but probably they do not play an important part in food absorption.

The pyloric caeca exhibited the same structure as the intestine, and was in open communication with this region of the alimentary canal. It was thought to be secretory and absorptive, like the duodenum and intestine.

A valve was present at the junction of the intestine and the rectum, and was well developed. The wall of the rectum had a similar structure to that of the intestine, except that the folds of the mucosa were deeper and goblet-cells were more numerous in it, and the musculature was thicker. Goblet-cells were absent from the mucosa in the region of the anus.

The development and ultimate fate of the gill-chambers and the operculum which covers them are described in the frog (*R. temporaria*) in a recent paper by Gwendolen T. Brock

(*Q.J.M.S.*, vol. 73, pt. ii, 1929). She finds that the operculum develops as a single fold of skin stretching in a convex sweep from the sides of the head right across the ventral surface of the body. This fold grows backwards over the gills, the lateral portions growing more rapidly than the ventral. Consequently both the right and left gill-chambers are open posteriorly at first. The communicating channel, which joins the two cavities, is formed by the ventral portion of the fold, which grows for a time as a free flap, and then fuses with the body wall again behind the communicating channel. The completed opercular fold fuses with the body wall posteriorly along the whole of its free margin, except for a small space on the left side, which forms the funnel-like spiracle. The right gill-chamber therefore does not communicate directly with the exterior, but with the left chamber by means of the ventral communicating channel. The chamber in its turn opens to the exterior directly by means of the asymmetrical spiracle.

The anterior limb-buds form within the gill-chambers and, as they develop, they pierce the operculum covering them. The author does not make clear whether the left limb-bud emerges through the spiracle, as has been thought, or through a new perforation in the operculum. She merely states that in tadpoles in which the limbs were freed no spiracle could be distinguished, apart from the two apertures in front of the limbs caused by their bursting through the operculum. The author accepts Latter's view, that the newly metamorphosed tadpole continues to breathe by means of gills as well as by lungs until the tail is completely absorbed. It was thought that the openings in the operculum through which the legs emerged served as temporary exhalant apertures for the branchial current.

Ultimately the branchial cavity appears to be obliterated by the sinking inwards of the operculum and the fusion of its inner surface with the inner wall of the branchial cavity, that is, with the original body-wall of the tadpole. Thus the outer surface of the operculum persists as the permanent body-wall.

An interesting and important paper by Gwendolen T. Brock (*Q.J.M.S.*, vol. 73, pt. ii, 1929) describes the development of the skull of the snake, *Leptodeira hotamboia*. The cartilaginous cranium consists of the basal plate and trabeculæ, and the otic and nasal capsules. The crista sellaris of the basal plate is exceptionally far forward, as compared to *Lacerta*, and the basicranial fenestra is thus entirely within the anterior third of the basal plate and in front of the auditory capsules. The trabeculæ run forward parallel to each other

in front of the fenestra hypophyseos, and fuse to form the nasal septum. Each of the otic capsules is separated posteriorly from the basal plate by the fissura metotica, and has a large vestibular division and a smaller cochlear part. The fissure metotica is divided into a small anterior medial opening of the recessus scalæ tympani and a posterior jugular foramen, through which the vagus passes. The jugular vein, as in other reptiles, passes through the foramen magnum. The fenestra cochleæ of the cochlear capsule faces the recessus scalæ tympani. Comparison of this fenestra with the mammalian fenestra rotunda leads to the important conclusion that the two are not homologous. It is claimed that the apertura lateralis of the recessus scalæ tympani of the reptile is the homologue of the fenestra rotunda of the mammal. The secondary tympanic membranes in both reptiles and mammals correspond morphologically and physiologically. The columella auris has an oval foot-plate and a shaft, the distal end of which bends sharply back to make contact with the quadrate. A small nodule at the distal end ossifies separately from the columella as a process of the quadrate. It is suggested that this nodule is an intercalare and that the distal bent end of the columella represents the dorsal process of the lizard.

The interorbital septum and the cartilaginous side-walls of the orbito-temporal region are lacking.

A small basitrabecular process projects laterally from the trabecular plate, and supports a small bone, situated in the side-wall of the skull over the trigeminal incisure and the facial foramen. The relations of this small bone indicate that it is an epipterygoid. It is concluded that this basitrabecular process and epipterygoid correspond to the so-called alisphenoid of earlier workers.

The nasal capsules are delicate and incomplete, and there are large conchal infoldings. The cartilaginous cup of Jacobson's organ is isolated from the rest of the nasal skeleton. A small planum antorbitale is attached to the dorsal edge of the nasal septum by a posterior commissure.

The membrane bones are well developed, and the parietals, frontals and parasphenoids form a strong fulcrum for the slender palate and jaws to work upon.

R. A. R. Gresson, in two recent papers (*Q.J.M.S.*, vol. 73, pt. ii, 1929), describes the oogenesis of the saw-flies, *Thrinax macula*, *Thrinax mixta*, and *Allantus pallipes*. Extensive nucleolar budding with probable extrusion of the buds into the cytoplasm was observed in all three species. The nucleoli in the early oocytes of *T. macula* are basophil, but they develop an oxyphil margin as they increase in size. This oxyphil part separates and becomes rounded off. The basophil part then

consists of a small basophil body surrounded by a less basophil or slightly oxyphil zone. Vacuoles appear in this outer zone of the basophil nucleolus, or in large masses given off from it, and dark granules develop in some of them. These granules increase in size, and are finally liberated as separate basophil bodies surrounded by faintly staining material. These buds move towards the periphery, but were not observed passing through the nuclear membrane or in the cytoplasm. They disappear after yolk formation has begun.

Simultaneously, buds are being formed either from the oxyphil nucleolus or from a large mass constricted off from it. These oxyphil buds also move towards the nuclear membrane.

The nucleoli of the early oocytes of *A. pallipes* are basophil, but they gradually change over to oxyphil. The basophil material in oocytes about to enter on yolk-formation is only represented by small bodies containing dark granules. The oxyphil nucleolus becomes active at this stage, and in some oocytes appeared to be breaking up. Buds were formed in contact with the inner surface of the nuclear membrane, and later bodies resembling them were observed in the cytoplasm.

Both oxyphil and basophil nucleolar buds are formed in *T. mixta*, the latter being observed as extrusions in the cytoplasm. It was thought that the oxyphil buds also passed through the nuclear membrane. The nuclear phenomena in this species resemble those of *T. macula* more closely than those of *A. pallipes*. It is concluded that in these three species the basophil nuclear extrusions at least take part in the formation of albuminous yolk, but it is suggested that the oxyphil extrusions may do so also.

Small Golgi vacuoles are said to be present in the undifferentiated cells at the proximal end of the ovarioles in all three species. They increase in number and size after the differentiation of the nurse cells and oocytes. The Golgi vacuoles of the nurse cells pass into the oocytes at a certain stage of oogenesis. The Golgi vacuoles in the oocytes are converted into fatty yolk by the deposition of fat within the vacuole. The mitochondria were not considered to play an important part in oogenesis.

H. W. Beams (*Anat. Rec.*, vol. 42, 1929) has described and figured osmiophilic structures at the poles of the nuclei in insect muscle cells. It is suggested that these structures probably represent the Golgi apparatus of other types of cells.

A remarkably interesting paper by G. H. Faulkner (*Q.J.M.S.*, vol. 73, pt. ii, 1929) describes the early prophases of the first maturation division of the oocyte in *Obelia geniculata*. The chief interest of the work lies in the fact that it was carried out entirely on the living unstained oocytes within the parent

medusæ. The tissues of the medusa and the yolk in even the larger oocytes was so transparent and colourless that even high powers could be employed to examine the nuclei. It was found that the nucleolus of the resting oocyte represented the condensed chromatic spireme which constituted the chromosomes. This nucleolus elongates and breaks up during the early growth stages of the oocyte into pairs of homologous chromosomes closely united. Subsequently the individual chromosomes of these bivalents separate. The two components of the largest bivalent pair were found to be unequal in size, suggesting that they represented an XY pair of sex chromosomes. Chromosome counts gave the number of bivalents as 17 and of univalents as 34. The chromosomes in later stages break up into numerous small globules, which become evenly distributed throughout the nucleus.

The details of the conjugation of a triploid individual of the holotrichous ciliate, *Chilodon uncinatus*, are described by Mary Stuart MacDougall (*Q.J.M.S.*, vol. 73, pt. ii, 1929). These do not differ much from those of the diploid and tetraploid forms, except that the behaviour of the odd chromosomes is abnormal. In the first maturation division the endobasal body of the micronucleus divides, and a spireme in the form of a parachute appears. This spireme breaks up into strings of granules, which later condense into six chromosomes. These chromosomes split, and at the metaphase six go to each pole of the spindle. The chromosomes then pair, preparatory to the resting stage, as has been observed in diploid and tetraploid forms. After the resting stage one or both of the daughter nuclei enter on the second maturation division, which is reductional. The initial stages are similar to those of the first division, except that the granule stage is not found. Six chromosomes are formed, and three go to each pole. Two chromosomes then pair, leaving the third unpaired, preparatory to the resting stage. After the resting stage the pronuclei move into the region of the protoplasmic bridge. Then the endobasal body divides, and a very dense spireme is formed. Three short rows of granules appear and condense into three chromosomes. These divide, the spindle pulls out, and three daughter chromosomes go to each pole. Two of the chromosomes at each pole pair and one remains unpaired. Then the migrating and stationary nuclei touch, and the membranes at the point of contact disappear, so that two sets of chromosomes come to be side by side. A resting stage follows this fusion of the nuclei, and the animals separate and reorganise.

Woodcock described in 1906 an interesting Gregarine occurring in the respiratory trees of Cucumaria. Helen

Pixell Goodrich (*Q.J.M.S.*, vol. 73, pt. ii, 1929) has reinvestigated this form and distinguishes two species. One of these, *Lithocystis cucumariæ n. sp.*, was found in the respiratory trees of *Cucumaria saxicola*. It was characterised by the trophozoites being neogamous and encysting in the wall of the respiratory tree. The spores are approximately 18μ long, and are provided with an epispore produced into a double funnel at one end and at the other into a long tapering tail. The sporozoites are short and transparent, and are arranged around a conspicuous residuum of refringent granules.

The other species, *Lithocystis minchinii Woodc.*, was characterised by lateral and neogamous association. The host attaches the paired parasites to its cœlomic wall throughout the greater part of their lives, and tries to embed them in its connective tissue. The spores have peculiar episporal projections, including a short flattened tail.

Quantitative observations on the molluscs and polychætes made in the intertidal zone in various localities on the Scottish coast are described by A. C. Stephen (*Trans. R.S. Edin.*, vol. 56, pt. ii, 1929). Consideration of the variations in the composition and density of this fauna in different places led to the conclusion that the beaches near Cumbrae, Firth of Clyde, were the richest, St. Andrews (West sands) coming next. In some places, such as Findhorn and Aberdeen, the results were almost negligible, while the Nairn coast and much of the Firth of Forth were very poor. Each species has its own area of distribution and region of maximum density. The shores examined fell into two types: those where *Tellina tenuis* and *Nephthys caeca* predominated, and those where *Cardium edule* and *Macoma baltica* predominated, but transition stages between the two occurred.

E. Percival and H. Whitehead (*Jour. Ecology*, vol. 17, 1929) have carried out a quantitative investigation of the fauna of different types of stream-beds, and have arrived at some interesting conclusions. They found that the bulk of the population was made up of a few types of organisms which varied somewhat with the environment. A very considerable variation in numbers was observed which could only be explained by reference to the conditions. Many of the organisms which have been considered as the chief source of the food of fish, such as *Limnaea peregra*, *Gammarus pulex* and *Ephemera spp.*, were found to constitute, in many places, little or no part of the total fauna. The Microphyta, the Ephemeroptera, and the Chironomidae were found to play a very important rôle.

The method of attack affords a useful means of studying fluviatile conditions. It showed the variation in species and

numbers which could be correlated with changes in the physical character of the stream, and admitted of those variations being presented numerically. It showed which organisms were of importance in the economy of the stream, and provided facts bearing upon the problem of maintaining or increasing the population for the purpose of feeding fish.

Sir Aldo Castellani, in an interesting paper (*Jour. Trop. Med. and Hygiene*, July 1929), deals with the problems of Climate and Acclimatisation. The importance of the effect of climate *per se* on Europeans living in the Tropics is emphasised and dealt with at length. The various factors which constitute climate are defined, and the influence of each on the native and the foreigner is described. A valuable section deals with acclimatisation and the measures which should be adopted by Europeans on entering the Tropics, with a view to becoming acclimatised as quickly and easily as possible. This paper should be of interest and value to many who intend going to the Tropics.

PEDOLOGY. By Professor G. W. Robinson, M.A., University College of N. Wales, Bangor.

The past year has been distinguished by an unusually large number of conferences of soil workers. The International Society of Soil Science has been responsible for most of these gatherings. Commission I (Physics) met at Praha and discussed mechanical analysis and kindred subjects. Commission II (Chemistry), meeting at Budapest, was mainly occupied with problems connected with soil acidity and alkalinity and the methods for determining plant nutrients. In Commission III (Biology and Biochemistry), held at Stockholm, nitrogen fixation and the microbiological decomposition of organic matter were discussed. Commission IV (Soil Fertility) at Königsberg discussed the determination of the nutrient requirements of soils with special reference to the methods of Mitscherlich and Neubauer. Commission V (Mapping and Classification) met at Danzig, and Commission VI (Drainage) at Praha. In addition, soil problems played a prominent part in the programme of Section M of the British Association for the Advancement of Science in South Africa.

The Brown Earths.—A. Stebutt (*Z. Pflanz. Düng.*, 1929, **15A**, 134–67) has made a useful contribution to the study of this somewhat loosely defined group of soils. The pedogenic processes may be considered as consisting of three phases, namely: (1) decomposition of the primary silicates into their principal components, silica, sesquioxides, and bases; (2) resynthesis of hydrous alumino-(ferro)-silicates from the

products of the first process; and (3) degradation of the secondary silicates formed by the last process into free silica and sesquioxides. The principal soil groups are characterised by the relative preponderance of these processes. The degradation phase is favoured on the one hand by excessive leaching, as in the podsols, and on the other hand by high temperature as in the laterites, where the hydrolytic action of water is reinforced by the relatively high concentration of carbonic acid formed from the aerobic decomposition of organic matter. In the brown earths, the occurrence of the degradation process is indicated by the brown colour of the soil, but the conditions are not sufficiently humid for the bleaching out of the surface horizon. The presence of free sesquioxides and silica in such soils may, however, be demonstrated by extraction with 5 per cent. potassium hydroxide, as proposed by Gedroiz. The brown earths form an intermediate stage between the tshernosems, in which the pedogenic processes have not proceeded past the formation of complex zeolith-like silicates, and the podsols and laterites in which degradation has been followed by translocation of the products and differentiation into horizons characterised by excess of silica or sesquioxides. The red earths represent an intermediate stage between brown earths and laterites.

Terra Rossa (Red Earth).—A. Reifenberg (*Koll. Chem. Beihefte*, 1929, pp. 1–93) gives an exhaustive discussion of the occurrence and mode of origin of the Mediterranean red earth. It occurs as a product of weathering of limestone in a region characterised by winter rainfall and summer drought. The humus content is low, the salt content relatively high, and the reaction alkaline. The sesquioxides liberated by hydrolysis are peptised by colloidal silicic acid and tend to move upwards to the surface where they are irreversibly precipitated as a consequence of the high concentration of electrolytes. The sesquioxides undergo further physical and chemical changes after precipitation. The same author, in a discussion of the weathering of sandstone and basalt in Palestine, examines the cause of the difference in colour between the terra rossa and the product of basalt weathering, and considers it to be due to the hydrated ferric oxides in the latter case having been formed from ferrous hydroxide sols.

H. Harrassowitz (*Chem. der Erde*, 1928, iv, 1) describes certain South European red soils which are to be considered as illuvial horizons, since in some cases they occur associated with an overlying A horizon. Such soils are quite distinct from the red earths derived from and invariably associated with limestone.

Climate and Soil.—H. Jenny (*Soil Research*, 1929, 1, 139–

189) has examined the soils of the United States of America in their relationship to climate as expressed by the rainfall (N–S) quotient of A. Meyer (*Chem. der Erde*, 1926, 209). This quotient gives a better measure of the relative humidity than the rain factor of Lang, and for each of the principal soil groups there is a characteristic range of rainfall quotient. The results agree well with those calculated for the corresponding European soil groups. Meyer's rainfall quotient considered together with mean annual temperature may serve to give a complete definition of climate from the point of view of soil formation. The extreme variety in soils may be attributed to the joint effect of humidity, which affects the degree of leaching, and temperature. Two localities with the same rainfall quotient will only be pedogenically identical if the temperature conditions are the same. When the complications introduced by the parent material are also considered it is only to be expected that an immense variety of soils should result.

Weathering of Felspars.—O. Tamm (*Medd. Stat. Skogsforsoksanstalt*, 1929, **25**, pp. 1–28) has attacked the problem of the weathering of felspars by a method based on the classical work of Daubrée, consisting in the self-grinding of particles of 5–15 mm. diameter by rotation in water. The minerals used were microcline and oligoclase, respectively. As in Daubrée's experiment, the aqueous medium rapidly became alkaline owing to liberation of hydroxides, but decomposition stopped at *p*H 10·7 in the case of microcline, and *p*H 11·1 in the case of oligoclase. The products of disintegration, principally of dimensions 2μ–0·2μ, were appreciably hydrated, the amount of water thus taken up increasing with fineness of division. Within *p*H range 10–6, the reaction with microcline particles appears to be a reversible base exchange with hydrogen-ions; whilst over the range *p*H 6–3, more pronounced decomposition occurs with liberation not only of alkali, but also of aluminium ions, a fact which is of significance for weathering processes under acid conditions. Oligoclase is decomposed irreversibly even over the range *p*H 10–6.

Mineralogy of Soils.—R. Hart (*J. Agri. Sci.*, 1929, **19**, 90–105, 802–13), from a mineralogical examination of the fine sand fraction of certain Scottish soils, has found a close correlation with the local geology. The local rocks appear to exert a preponderating influence on the matrix of their overlying drifts.

Colloidal Behaviour in Soils.—S. Mattson (*Soil Sci.*, 1929, **28** 179–220) has examined the effect of different ions on the properties of soil colloids. He refers the observed phenomena to the formation round the colloidal particle of an atmosphere of ions whose density and thickness are governed by the specific

characters of the ions comprising it. In the absence of free electrolyte, potential difference, swelling, and viscosity are governed by the degree of dissociation of the exchangeable cations, depending on such specific ionic characters as valence, hydration, and potential. Imbibition increases with degree of dissociation, and proceeds until there is an equilibrium between osmotic and electrostatic forces. The presence of free electrolyte results in a suppression of potential difference, swelling, and viscosity, depending solely on the valence of the ions. A distinction is drawn between the micellar solution which is an integral part of the colloid and the imbibed solution which participates in the phenomena of swelling and viscosity changes. The same author (*Soil Sci.*, 1928, **26**, 372–409) has also studied cataphoresis and flocculation in electrolyte solutions in the case of bentonite, and soil colloids with different silica-sesquioxide ratios. Charge and stability depend on the degree of dissociation of the exchangeable cations. Changes produced in the stability of colloids by added ions are the direct consequence of ionic exchanges. Thus, if the replacing ion is more highly dissociated in the complex than the ion replaced, the charge, osmotic hydration, and dispersion tend to increase. Free electrolyte exerts a suppressing effect, although dissociation is increased consequent on a decrease in ionic activity. Particle size is due to a balance between cohesion on the one hand and dispersion as determined by ionic density on the other, and there is for each colloid a size of maximum stability depending on the nature and quantity of the exchangeable ions. Aggregation is distinguished from flocculation. Whilst the former is a continuous process operating under all conditions, the latter is a discontinuous process in which the micelles are linked, apparently by electrostatic attraction. Osmotic hydration is the most important factor in stability. Since hydration depends on the number of dissociated ions, the generally observed connection between high charge and stability is to be considered as an indirect effect.

Base Exchange and Soil Reaction.—F. Menschikovsky and S. Ravikovitch (*Soil Sci.*, 1929, **27**, 50–68), from a study of the exchangeable bases and soil solutions of Palestine profiles, conclude that the composition of the soil solution in a highly mineralised soil conforms with and reflects the character of the colloidal complex.

G. Wiegner (*Trans. Comm. II, Int. Soc. Soil Sci.*, 1929, Vol. B, 92–144) has made an important contribution to the theory of the determination of the hydrogen-ion concentration of soil suspensions. He shows that the measured hydrogen-ion concentration of a suspension is made up of the hydrogen-ion

concentration of the dispersion medium in equilibrium with the disperse phase, and the hydrogen-ion concentration due to the hydrogen-ions in the cationic shell of the suspended particles. In a settling suspension, the hydrogen-ion concentration at a given point gradually decreases as the particles settle out, and finally reaches the hydrogen-ion concentration of the medium, which is independent of the soil-water ratio. The hydrogen-ion concentration of a suspension can be represented by the relationship $C'_H = C^\circ_H + km$, where C'_H is the observed hydrogen-ion concentration, C°_H, the hydrogen-ion concentration of the dispersion medium, m, the concentration of the disperse phase, and k, a constant. The equation has been experimentally verified for a number of soil suspensions, and holds so long as there are no changes in the state of aggregation of the disperse phase. The same principles apply to the measurement of hydroxyl-ion concentration in alkaline suspensions. The disperse phase in soil suspensions is held to consist of particles each composed of a nucleus surrounded by an inner shell of anions, including hydroxyl-ions, and an outer shell of cations, including hydrogen-ions. Both hydrogen- and hydroxyl-ions are exchangeable. The inner shell ions are held to the nucleus by lattice forces and residual valencies, whilst the outer shell ions are held by electrostatic attraction. The author also describes suspensions in which the suspended particles are characterised by an inner cationic shell and an outer anionic shell. Here again it is possible to verify the exchangeability both of hydrogen- and hydroxyl-ions.

M. Trénel (*Trans. Comm. II, Int. Soc. Soil Sci.*, 1929, vol. B, 144–153) has studied the properties of permutite gels bereft of their bases by electro-dialysis. He considers the exchange acidity developed in the presence of neutral salts to be the consequence of an actual decomposition of the complex into sesquioxides and silicic acid and not to the presence of exchangeable hydrogen-ions. The desaturation of soils is not considered to be completely reversible.

Manganese Dioxide in Soils.—W. O. Robinson (*Soil Sci.*, 1929, **27**, 335–350), in a study of the occurrence of manganese dioxide in soils, shows that this substance occurs to some extent in the sand, to a greater extent in the silt fraction, and not in the clay fraction. The solubility of manganese depends on soil reactions and in the relative prevalence of oxidation and reduction. The presence of calcium carbonate plays an important part in the precipitation of manganese dioxide. The presence of manganese dioxide in the surface is peculiar to certain soil series, which show a characteristic chocolate-brown colour. A low silica-sesquioxide ratio is also correlated with the presence of manganese dioxide in the surface soil.

Oxidation-Reduction Potential of Soils.—N. P. Remesov (*Z. Pflanz. Düng.*, 1929, **15A**, 34–44) has broken fresh ground in a suggestive discussion of the application to soils of measurements of oxidation-reduction potentials. The data presented demonstrate clearly the variations in oxidation-reduction potential in the different horizons of profiles. The oxidising status decreases with depth, parallel with the increasing tendency to reducing conditions. It is perhaps not too optimistic to suggest that oxidation-reduction potential measurement may prove as valuable an instrument in soil research as the measurement of hydrogen-ion concentration.

Organic Matter and Peat.—A. F. Heck (*Soil Sci.*, 1929, **27**, 1–48) has studied the decomposition of the nitrogenous compounds of fungous tissues in soils. They are for the most part fairly simple and readily decomposed. S. A. Waksman and K. R. Stevens (*Soil Sci.*, 1929, **27**, 271-82), in a profile study of a Florida Everglade (low moor) peat, found small amounts of cellulose in the upper but none in the lower layers. Ether-, alcohol-, and water-soluble constituents were low, hemicelluloses and ash, medium, and protein and lignin, high in the upper layers. The lower layers showed a low hemicellulose and high ash content. In the decomposition of this type of peat there is a rapid mineralisation of the nitrogen. A sedimentary peat from a Florida lake showed a general similarity to the lower layers of the Everglade profile. The same authors (*ibid.*, 1929, **28**, 321–340) show that the increased decomposition in samples of peat treated with ether and toluene is due, not to changes in microbial balance, but to the removal of waxy substances.

A. P. Dachnowski-Stokes (*Soil Sci.*, 1929, **27**, 379–88) and S. A. Waksman and K. R. Stevens (*ibid.*, 1929, **27**, 389–98) have carried out further studies on high moor peat profiles in Maine, U.S.A.

H. C. Jensen (*J. Agri. Sci.*, 1929, **19**, 71–82) discusses and examines the effect of carbon-nitrogen ratios of organic materials on their mineralisation in the soil.

Temperature and Soil Nitrogen.—From a study of the soils of the United States, H. Jenny (*Soil Sci.*, 1929, **27**, 168–88) infers a correlation between the mean annual temperature and the average total nitrogen content of upland prairie, forest, terrace, and bottom soils. The logarithm of the nitrogen content varies inversely with the temperature, every 10° C. decrease in temperature corresponding with a twofold or threefold increase in nitrogen content. The carbon-nitrogen ratio decreases with increasing temperature.

Biochemistry of Waterlogged Soils.—V. Subrahmanyan (*J. Agri. Sci.*, 1929, **19**, 627–48), in continuation of earlier

work, has shown that losses of nitrogen on addition of nitrate to waterlogged soils do not occur in the absence of decomposable organic matter. Addition of small quantities of a fermentable substance such as glucose is followed by rapid depletion of nitrate and oxygen without denitrification, and increase in acidity, carbon dioxide, and bacterial numbers. The soluble nitrogen under such conditions is mainly assimilated by micro-organisms, and the carbohydrate mainly transformed to lactic, butyric, and acetic acids.

Physical Properties.—B. A. Keen and G. W. Scott Blair (*J. Agri. Sci.*, 1929, **19**, 684–700) have studied the viscosity and pseudo-viscosity of clay-water pastes. The static rigidity of pastes in the pseudo-viscous state, a measure of the energy required to induce plastic pastes to flow, is shown to be correlated with other physical properties connected with the tillage character of soils.

Single-value Soil Properties.—J. R. H. Coutts (*J. Agri. Sci.*, **19**, 325–341) presents some interesting and valuable data on Natal soils for the properties characterised by single-value determinations.

Soil Structure.—G. J. Bouyoucos (*Soil Sci.*, 1929, **28**, 27–38) proposes to determine the ultimate "natural" structure of soil by using simple shaking in water as a dispersive treatment preliminary to mechanical analysis by the hydrometer method. It is believed that the results obtained correspond with a definite stage of stability, and form a suitable basis for the study of physical properties such as percolation, penetration, etc.

A. T. Tiulin (*Perm. Agr. Chem. Publication*, **8**, 1929) discusses the determination of non-capillary pore space in soils, and shows that where water is used the swelling of the soil colloids results in low results being obtained. The use of xylol is recommended.

Mechanical Analysis.—The method of mechanical analysis recommended by the International Society of Soil Science has been subjected to some criticism on the ground of its inadequacy in certain cases. G. B. Bodman (*Soil Sci.*, 1928, **26**, 459–70) questions the advisability of subjecting calcium-saturated soils to the drastic pre-treatment involved in the International method, and shows further that in some cases the correlation of the colloidal content as determined by the International method with that of the so-called absorption method is imperfect—a result which does not necessarily impugn the method of mechanical analysis. A. F. Joseph and O. W. Snow (*J. Agri. Sci.*, 1929, **19**, 106–20), on the other hand, find that the International method does not secure adequate dispersion in the case of certain heavy alluvial soils.

Difficulties are also encountered with gypseous soils, but these can be analysed by the International method if a modification recommended by Prescott (*Australian Council for Sci. and Ind. Res., Pamphlet* 8) is adopted. The determination of soil organic matter naturally supplements mechanical analysis, and G. W. Robinson, W. McLean, and R. Williams (*J. Agri. Sci.*, 1929, **19**, 315–24) have developed a simple routine method based on the well-known Kjeldahl reaction which enables organic carbon and nitrogen to be determined in one procedure.

METEOROLOGY. By E. V. Newnham, B.Sc., Meteorological Office, London.

The Atmosphere as a Colloid.—It would not occur to the average individual that the peculiar properties of colloids—liquids containing numerous very small suspended particles—deserve to be studied by anyone seeking to explain the phenomena of rain and cloud more fully than has yet been done, but that appears to be the view of Schmauss and Wigaud. These meteorologists have recently completed a work entitled *Die Atmosphäre als Kolloid*, which is briefly reviewed by F. J. W. Whipple in the *Meteorological Magazine* for September 1929. This is certainly a branch of study that might well receive the attention of English meteorologists, who have tended to concentrate on the application of the simple thermodynamical processes connected with rarefaction of air in their explanations of rainfall. Aitken, however, began pioneer research work many years ago in Scotland in the study of the action of minute particles in the atmosphere in relation to condensation of water vapour, and Owens has extended his researches, with interesting results, connected more with the formation of fog than of rain. The early meteorologists established almost beyond question [1] that a very large proportion of the rainfall of the world is due to dynamical cooling of air beyond its dew-point, but were unable to explain why under certain conditions large clouds will form and retain great quantities of water for a long period without the formation of drops large enough to produce rain; nor, so far as the present writer is aware, has anyone explained the converse phenomenon, namely, an exceptional facility for the formation of rain.

[1] A well-known French meteorologist—Guilbert—entirely disagreed with this view. The positive correlation between the altitude of places and their average rainfall that appears to be invariable when a region affected by similar meteorological conditions is considered, fits in very well with the orthodox view. Guilbert, however, uses it in support of his own theory, and would have it that rain evaporates to such an extent in its descent that the valleys receive much less than the hills, owing to the drops having to travel farther to reach the valleys.

Sometimes frequent showers occur out of a sky only partly covered by clouds, these being of small opacity and failing to exhibit the towering structure of typical shower clouds (cumulo-nimbus). There is another phenomenon which may need more than simple thermodynamical reasoning for its explanation—the apparent tendency for fluctuations of long period in the average amount of rainfall associated with a given type of synoptic chart: it appears to have been the view of most of those meteorologists who have had a long experience of weather forecasting in this country that in certain years depressions appear to be singularly shy of giving rain. One is generally referred to peculiarities in the physical state of the upper atmosphere—in particular to its temperature—for an explanation, but nobody appears to have collected any information in support of this view; there remains therefore a distinct possibility that the minute nuclei on which condensation takes place may be involved. It may be pointed out that the reality of this third phenomenon does not yet rest upon more than personal opinion, and would be difficult to prove. Thus, an exceptionally dry summer is generally associated with more than the usual number of anticyclonic spells (periods of light wind and high barometric pressure), or of spells of easterly or northerly wind, and to prove the reality of the tendency just described it would be necessary to show that during the intervals between these situations that are favourable for fine weather there was less rainfall than would normally be expected, having regard to the types of meteorological situation prevailing at these times. This would be difficult to do because of the infinite variety of situations that can arise, which makes satisfactory grouping of them into types almost impossible.

Before leaving the subject of the atmosphere as a colloid, reference may be made to a short article by R. F. T. Granger, in the December number of the *Meteorological Magazine*, which appeared as a result of the review referred to above. The writer of that article points out that in smoke, which he regards as a typical colloid, the smaller particles are kept in suspension and hindered from coagulating largely by Brownian movements, which keep them in continuous agitation, but that questions of electrical charge are involved. Positively or negatively charged particles will repel one another, and neutralisation of these electrical charges will facilitate coagulation. More information about the proportions of drops of different size in clouds, and in regard to the electrical charges that they bear, is obviously desirable, for although water clouds are not colloids in a strict sense, that is to say they are not composed of particles in a fluid medium, the particles and medium

being mutually insoluble, they are liable to contain droplets with diameters as small as 0·4 micron, and such droplets are liable to Brownian movement. It is suggested that the possibility of rain-drops being formed may depend partly upon the existence of droplets above a certain size, and that when many of the drops exceed that limit condensation into still larger drops may be very rapid.

The Transfer of Heat by Radiation and Turbulence in the Lower Atmosphere.—An important paper under this title by D. Brunt appeared in the *Proceedings of the Royal Society* for 1929, A, vol. 124. In it the author uses Hettner's measurements of the absorbing power of water vapour for long-wave radiations as a basis for calculating the vertical flow of heat that results in the lower atmosphere from the continuous process of radiation and absorption by successive superimposed layers of water vapour.

It has long been known that the absorptive power of the component gases in the atmosphere is in general small, and is in fact negligible, at least near the earth's surface, except for the few absorption bands of carbon dioxide and for the much more powerful absorption by water vapour. The effect of the latter is observable in the greatly reduced fall of temperature at night, with a clear sky, on occasions when the atmosphere carries a large amount of water vapour. Hettner's experiments have been summarised by Simpson (*Mem. R. Met. Soc.*, No. 21, 1928), and stated in a convenient approximate form which makes possible their application to meteorology without the use of very advanced mathematical analysis. It is shown that a column of air that contains water vapour equivalent to a depth of 0·3 mm. of liquid water will absorb completely all radiations of wave-length 5·5μ to 7μ, and greater than 14μ, and will partly absorb radiation between 4μ and 5·5μ, between 7μ and 8μ, and between 11μ and 14μ. For the range 8μ to 11μ the vapour is transparent. It may be noted that this last range includes the wave-length of maximum intensity for average terrestrial surface temperatures, which is about 10μ. The main absorption band of CO_2 is centred around 15μ. Simpson's simplifying assumption is that within the restricted regions of wave-length 5·5μ to 7μ and above 14μ the essential facts are sufficiently well expressed by saying that the emission of radiation from water vapour is equivalent to black-body radiation at the same temperature, and that the radiation of these wave-lengths is completely absorbed by a layer of air containing 0·3 mm. of precipitable water vapour. Brunt uses this same approximation, and confines his application to cases of clear sky, when the radiation between 8μ and 11μ can escape into space and so cease to be of any account for

terrestrial purposes. He uses the term W-radiation to describe radiation over the range of wave-lengths within which 0·3 mm. of precipitable water radiates like a black body. The length of a column of air (l) that will contain exactly 0·3 mm. of precipitable water will depend upon the temperature and the aqueous vapour pressure; the exact relationship can be shown to be: $l = 139 \frac{T}{p_w}$ centimetres, T being the absolute temperature of the air and p_w the aqueous vapour pressure. At ordinary temperatures, and with a vapour pressure of 10 millibars—a common value in temperature latitudes—l will be about 40 metres. We see therefore that the W-radiation from the earth's surface will be completely absorbed by a layer of remarkably moderate thickness, which layer will re-emit radiation appropriate to its own temperature. During the daytime the mean temperature of this first layer will generally be lower than that of the earth's surface, but by night it will often be higher, when the sky is clear, as is assumed in this paper. A point to which attention is drawn is in regard to the action of the dry air in the atmosphere. Although it plays no direct part in the absorption and emission of the W-radiation, it shares with the water vapour any change of temperature arising from these processes, and may be regarded as endowing the water vapour with a vastly greater specific heat. The assumption is made that l is sufficiently small for temperature to be regarded as constant throughout each layer of thickness l, and equal to its mean value throughout the layer, when calculating the radiation from any such layer. When it is recalled that on an average temperature falls between 5° and 6° C. in 1,000 metres of height, *i.e.* in a height of about 25 l, it will be seen that this assumption is hardly likely to lead to serious error.

We come now to the first of the two problems with which this paper deals—the derivation, from the approximate representation of Hettner's absorption data, of an equation for radiative transfer. For this purpose the atmosphere is regarded as made up of layers of thickness l; each of these contains 0·3 mm. of precipitable water, but the normal percentage decrease of vapour pressure with height being greater than the percentage change in the absolute temperature, the higher layers will be thicker than the lower. Consider the nearly horizontal surface separating the rth and $(r+1)$th layers above the earth's surface. The amount of radiation passing upwards across it may be denoted by E_r, and all of this must necessarily have originated within the rth layer, seeing that all radiation from points more than l below this surface is by hypothesis absorbed in the rth layer. In the same way it is

concluded that the downward radiation will all have originated within the $(r+1)$th layer, and can according to the same notation be denoted by E_{r+1}.

Calling the net upward flux F_R

$$F_R = E_r - E_{r+1}$$

then since $\quad F_R = -\Delta E_r$

$$F_R = -\frac{\Delta E}{\Delta T}\left\{\tfrac{1}{2}\left(\frac{\Delta T}{\Delta z}l\right)_{r+1} + \tfrac{1}{2}\left(\frac{\Delta T}{\Delta z}l\right)_r\right.$$

where z represents height.

If now we may replace the finite difference ratio $\frac{\Delta T}{\Delta z}$ by $\frac{\partial T}{\partial z}$, a proceeding which implies that over heights of the order of 40 metres the lapse-rate will not show very large variations, then the above may be written :

$$F_R = -\frac{\partial E}{\partial T}\cdot\frac{\partial T}{\partial z}\times l$$

$$= -\frac{\partial E}{\partial T}\cdot\frac{\partial T}{\partial z}\frac{bT}{p_w}$$

$$\text{or } F_R = -k\frac{\partial T}{\partial z} \quad \ldots\ldots \quad (1)$$

The constant b, as we have seen, has the value 139 in C.G.S. units.

The value of $\frac{\partial E}{\partial T}$, according to a table given by Simpson (*loc. cit.*), is practically a linear function of temperature between 200° and 295°*a*, given by the equation

$$\frac{\partial E}{\partial T}\times 1000 = 3{\cdot}0 + 0{\cdot}2\,(T - 270)$$

Brunt works out the value for k when the temperature is 275°*a*, the vapour pressure 5 millibars (corresponding with a relative humidity of 70 per cent.), and finds it to be 23, the units being the gramme calorie and the minute. If now the lapse-rate be equal to the dry adiabatic, which is the rate that results from thorough churning up of the air as happens normally over a land surface in the first few thousand feet on a cloudless mid-summer day, then $\frac{\partial T}{\partial z}$ becomes -10^{-4} in degrees centigrade per centimetre and the vertical flux of radiation will be $2{\cdot}3 \times 10^{-3}$ gramme calories per minute. This is compared with the average flux of solar radiation for the earth as a whole : assuming that the solar constant is 2 gramme calories per minute

and the average value of the earth's albedo 0·43 (Aldrich's figure), this is shown to be 0·275 gram/cal. per square cm. per minute. Under the specified conditions, therefore, less than one-hundredth of the incoming radiation can pass out through the lower layers as W-radiation—an important result. An even more important point to notice in connection with the foregoing equations for radiative transfer of heat is that this transfer can be expressed by an equation similar to that for the conduction of heat in a solid: $\frac{\partial T}{\partial t} = k\frac{\partial^2 T}{\partial z^2}$, as the following considerations should make clear.

A disk of air of unit horizontal area and thickness dz will according to equation (1) gain a quantity of heat $-\frac{\partial}{\partial z}\left(k\frac{\partial T}{\partial}\right) dz$ per minute. Making the second the unit of time, and calling C_p the specific heat of air at constant pressure, this must be equal to 60 $\rho C_p \times \frac{\partial T}{\partial t}\,\partial z$, and we get

$$\rho C_p \frac{\partial T}{\partial t} = \frac{1}{60}\frac{\partial}{\partial z}\left(k\frac{\partial T}{\partial z}\right) = \frac{1}{60}\frac{\partial}{\partial z}\left(\frac{T}{p_w}\frac{\partial E}{\partial T}\frac{\partial T}{\partial z}\right).$$

Now $\frac{\partial T}{\partial z}$ is the lapse-rate, a quantity that is known to vary with height and time over a wide range, but an examination of the possible variations of k shows that this will vary very slowly with height and time. It is therefore permissible to regard k as a constant when considering the stream of W-radiation at a given moment, anywhere in the lower layers of the atmosphere, and to put the equation just given into the form—

$$\frac{\partial T}{\partial t} = \frac{b}{60\rho C_p}\frac{T}{p_w}\frac{\partial E}{\partial t}\frac{\partial^2 T}{\partial z^2} = K_R\frac{\partial^2 T}{\partial z^2}$$

where K_R is practically constant for a given height.

Taking again the case where $T = 275a$ and $p_w = 5$ m.b., we get for K_R the value $1·3 \times 10^3$, the corresponding coefficient for the molecular conduction of heat being about 0·16 in the same units. The expression radiative diffusivity is suggested by Brunt for K_R.

The author completes this section of his paper by considering the case where z is so small that there is not a complete layer l with 0·3 mm. of precipitable water between the point in question and the ground. Under these conditions the same type of equation as (1) is found to be applicable.

An expression for the vertical transmission of heat by turbulence in an incompressible atmosphere was found nearly fifteen years ago by G. I. Taylor. Brunt finds that a slight

modification of Taylor's analysis, and the introduction of absolute temperature in place of potential temperature, enables him to express the flux of heat by turbulence as follows:

$$\rho\frac{\partial T}{\partial t} = \frac{\partial}{\partial z}\left\{K\rho\left(\frac{\partial T}{\partial z} + a\right)\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where K is an essentially positive quantity that measures the activity of the eddies at height z, and a is the dry adiabatic lapse-rate. Space does not permit the repeating of the ingenious analysis leading up to this result. Under circumstances where K does not vary with height, equation (2) can be reduced to the same form as that for diffusion of heat by radiation, namely—

$$\frac{\partial T}{\partial t} = K\frac{\partial^2 T}{\partial z^2}$$

and the combined effects of radiation and turbulence by

$$\frac{\partial T}{\partial t} = (K + K_R)\frac{\partial^2 T}{\partial z^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where both K and K_R are positive.

If they both remain constant through small ranges of height (z), then temperature will rise when $\frac{\partial^2 T}{\partial z^2}$ is positive, and will fall when $\frac{\partial T^2}{\partial z^2}$ is negative. It is shown that both components of (3) will tend to smooth out any bends in a temperature-height curve.

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ARCHÆOLOGY. BY E. N. FALLAIZE, Royal Anthropological Institute, London.

As time gives perspective to the meeting of the British Association in South Africa, it becomes increasingly apparent that, so far as the study of archæology is concerned, this is one of the most important meetings to be held overseas, and the papers

read surpass in interest almost any that have been presented to the Anthropological Section in recent years. Reference has already been made in these pages to the results of Miss Caton-Thompson's excavations at Zimbabwe and Mr. Leakey's Stone Age discoveries in Kenya. In relation to the latter, special interest attaches to Mr. A. Leslie Armstrong's cave exploration at Bambata and Mr. v. Riet Lowe's investigations on Sheppard's Island and in the Vaal Valley. The latter demonstrated the evidence for pluvial periods with which are associated the various types of stone implements occurring in South Africa, and possibly to be linked up with the pluvials found by Mr. Leakey in East Africa; while Mr. Armstrong discovered in the Bambata Cave an apparently contemporary association of Mousterian and Aurignacian implements pointing to a similarity of conditions to those in Kenya where the appearance of an Aurignacian type of implement in early strata also points to the contemporaneity of the two cultures.

In his Presidential Address to the Anthropological Section Mr. Henry Balfour spoke of the progress in archæological studies which he had noted in the interval between his first and his then fourth visit to South Africa. What is now the position of these studies in South Africa may well be gauged from Vol. XXVII of the Annals of the South African Museum which consists of a monograph on the Stone Age industries of the subcontinent by Mr. v. Riet Lowe and Mr. A. J. H. Goodwin, entitled *The Stone Age Cultures of South Africa*. The publication of this volume coincided with the visit of the Association to South Africa and gave those members who were fortunate enough to obtain a copy, a comprehensive view of the archæological problem. This publication now supersedes in classification and nomenclature that of Mr. Peringuey—whose work, however, will always remain classical in South African archæology. The influence of Mr. Burkitt's archæological tour in South Africa in 1928 and of his subsequently published *South Africa's Past in Stone and Paint* is to be noted, but South African archæologists are still cautious, and perhaps wisely, in adopting the European nomenclature for the various types of South African implements.

At Johannesburg members of the Association had an exceptional opportunity for the study of the so-called Bushman paintings and engravings. A large collection of copies of these had been brought together for exhibition, together with some of the drawings and paintings collected by the Frobenius expedition, which was then in Rhodesia investigating the Zimbabwe ruins and other antiquities. It is interesting to note that a growing body of opinion which then found expression is leaning towards the view that this art, in its older manifesta-

tions at least, may be the product, not of the Bushman, but of an older race. The papers on the physical anthropology of the older types of man in South Africa and the incidental discussions of the ethnological problem, though not bearing directly on this point, showed what might be regarded as tendency to lend some support to this view. Adequate consideration must, however, await fuller publication of the papers in question.

The interest aroused by Col. Lindberg's experimental aeroplane flight over the Maya country in Central America and in the South Western States, where excavations are being carried on in Arizona, has again called the attention of the general public to the immense value of this new arm of archæological exploration. Arrangements for the flight were the result of co-operation between the Pan-American Airway (Inc.) and the Carnegie Institution of Washington. Good work was done in Arizona and New Mexico, where sites not previously known were discovered in places which in the ordinary course would never have been attained by explorers on account of their aridity and difficulty of access. The results obtained in the Central American area were even more striking. Belize was made the base and several thousand miles were covered. Not only were known sites visited, but country untraversed by a white man was viewed and a number—at least four—of quite unknown cities and groups of monuments in the jungle were discovered. The indication is that the aeroplane will add vastly to our knowledge of Central American antiquity by making possible a survey of the country and the monuments. From the topographical information thus gained, deductions of no little importance in their bearing upon questions of racial movement and cultural distribution cannot fail to be drawn. At present the general " lie " of the Maya country is unknown. In view of the tremendous mass of tropical vegetation surrounding the monuments—it grows to an almost impenetrable thickness immediately after the season's clearing for excavation—Dr. Kidder is perhaps optimistic in his view of the possible use of the aeroplane as a means of transport for the excavator. Landing would present difficulties unless under exceptional circumstances. Transport, however, is one of the greatest difficulties with which archæological expeditions now have to contend. The value of the 'plane, if its use became practicable, is indicated by the fact that the flight from Tikal to Uaxactun took six minutes, whereas for mule transport it is a journey of at least a day.

The British Museum expedition to explore Maya antiquities in British Honduras left England on January 23. The leader

of the expedition this season is Capt. Gruning, Capt. Joyce, who has hitherto acted as leader in previous expeditions, being detained at home by his official duties. The expedition will continue the work of last year at Pusilhá, studying the sculptured stelæ which still await decipherment, and exploring the ruins as yet unexamined, of which a number lie along the river bank. The work of last year was described for the first time after the return of the expedition by Capt. Joyce and Capt. Gruning at a meeting of the Royal Anthropological Institute held in December last. The expedition worked on a site to be known as Pusilhá, after the adjacent river. It is the centre of a number of ruins, and a number of important stelæ have been brought to light in the area. The most interesting result of the season's work, however, took the form of some thousands of potsherds which the Museum staff has been engaged for some months in laboriously sorting, classifying, and piecing together. As a result of this work a number of pots, of great interest both in their form and in their decoration, have been reconstructed. The sherds came from a cave in which the deposits were in the nature of a midden. A funnel in the roof of the cave apparently served as a shoot for rubbish from the village situated above. This had formed a mound, but, owing to the method of deposit, stratification was irregular and difficult to gauge. As a whole the pottery is of a remarkable fineness, smooth, thin and of a delicate texture, while the colours of the painted designs are exceedingly beautiful and well preserved. Both in form and still more in design, the pottery is remarkable, including types not previously known. It is hoped that further examination may elucidate the stratification and provide a clue to dating and the origin and progression of the designs. In order that the work may continue without interruption the Director of the British Museum, Sir Frederic Kenyon, has issued an appeal for funds. The expedition is dependent upon voluntary contribution entirely, the only assistance from public funds being a subvention from the government of British Honduras.

The season has again opened well for the British Museum's excavation at Ur, and although the two monthly reports which Mr. Woolley has already contributed to *The Times* in January and February contain nothing as sensationally interesting to the general public as the discoveries in the Royal Tombs of last season, they promise to be of not less moment for the early history of civilisation in Mesopotamia. One point of interest may, however, be mentioned before passing to what will no doubt prove to be the major objective. The view that Sumerian noblemen wore wigs is now confirmed by the discovery of one of these articles still adorned with gold ornaments in one

of the Royal Tomb Series. Attention has now been turned in the cemetery to the mounds of rubbish which have accumulated in the course of time. Starting from the stratum belonging to the First Dynasty of Ur (circa 3100 B.C.) which lies over the Royal Tombs, with a layer possibly of older date beneath it, eight strata have been laid bare. The Royal Tombs lie in the third, a layer of considerable thickness. In the succession of the lower layers, Mr. Woolley notes that it is possible to mark the gradual development of the art forms as they pass from naturalism to conventionalism. What is most interesting, however, is that the tablets and seals show linear designs in combinations previously unknown in Mesopotamia, so much so that their Sumerian origin might be doubted if other objects did not place it beyond question. While of a number of the inscriptions, some contain known elements from the scripts combined with others unknown, others show script in combination with figures such as a bird or bull's head. This has suggested a transition from pictorial to script writing. In the sixth layer copper bulls' hoofs show a technique equal to that of the copper sculpture of the Royal Tombs. The fact that they come from a level twenty feet lower than the Royal Tombs points to a long apprenticeship in the art of metal working, of which the beginning has not been reached even at this early stage. On the town site a second excavation has been opened in which the stratification is closely related to that of the cemetery. Here, in a depth of twenty-nine feet, eight houses built one upon the top of the ruins of another have been unearthed. The fourth from the top is already older than the Royal Tombs and the sixth is contemporary with the stratum which produced the bull's feet and the linear seals in the cemetery. In the eighth were sherds of the pottery painted in three colours, hitherto discovered only at Jemdet Nasr, near Kish, and below again begin to appear the black and green (buff) sherds of the al 'Ubaid type. In this same stratum was found a remarkable example of Early Sumerian art in the form of a wild boar in the round of steatite. Although the oldest example of Sumerian art yet found, its pose and vitality are remarkable.

Among the events of major importance of last year in connection with the problem of Early Man must be counted further discoveries of the remains of Peking Man. In the summer parts of the jaw, skull, and teeth of an immature individual were found, while at the beginning of December a skull was brought to light by a Chinese geologist, Mr. Pei. This was exhibited at the Geological Society in Peking later in the month. A notice appeared in the *Daily Telegraph* of December 16 last. The story of Peking Man will not be the least romantic

in the history of discoveries relating to Early Man. It will be remembered that the first hint of the existence of *Sinanthropus Pekinensis*, as he is now called, was the purchase in 1903 of certain fossil teeth in an apothecary's shop in Peking. Their presence there was due to the fact that it is the custom in China to grind up fossil bones and use them as medicine. On examination certain of these teeth showed characters which indicated that they belonged to a type of Early Man. In 1920 further teeth of a similar type were found in material from the deposits of Choukoutien discovered by Dr. J. C. Andersson. From these and later discoveries the existence of Early Man in China was established, the associated fossilised remains of mammalia, such as the sabre-toothed tiger, establishing their age. The parts of skull and jaw which have been described bear out the indications of the teeth that an early type of man lived in China in the middle, perhaps in even the early, Pleistocene. While this type shows, in form of skull, jaw, and teeth, certain characteristics which recall in some respects the anthropoids, it may, nevertheless, be regarded as close to the direct line of descent of modern man. An illustration of the skull with description by Professor Elliot Smith appeared in the *Illustrated London News* on February 8. It is the skull of a young adult, highly mineralised, and with its left lateral surface buried in a hard block of travertine, not yet removed. The right side has been cleared and shows that, though the facial region is missing, the brain case is complete.

Owing to Miss Caton-Thompson's absence in South Africa while excavating at Zimbabwe, the Royal Anthropological Institute's expedition to the Fayum was intermitted next year. Nor has any expedition been sent out to Egypt this season, as Miss Caton-Thompson is engaged in preparing the results of her Zimbabwe expedition for publication. A concession for next season has been obtained on a site which promises to provide valuable material for elucidating the relation between the early culture of the Fayum and that of Badari. Miss Caton-Thompson will again act as leader of the expedition in the field. An influential committee has been appointed under the Council of the Royal Anthropological Institute, which has issued an appeal for funds. The cost of the expedition will be somewhere in the neighbourhood of £1,200. Contributions should be addressed to the Secretary of the Committee, Miss Gardiner, Bedford College, Regent's Park, N.W., or to the Treasurer, Royal Anthropological Institute, 52 Upper Bedford Place, W.C.1.

ARTICLES

MODERN SEED TESTING

By Prof. R. G. STAPLEDON, M.A.
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The world's commerce in agricultural seeds is of inestimable importance in relation to the production of human food; it is in the aggregate extensive, and is intricate to quite an extraordinary degree. The intricacy results from a number of causes inherent in the very nature of the commodity we are considering. Few commodities pass through so many hands in their chequered career from producer to consumer—in this case from the farmer who sows seed to produce a seed crop to the farmer who sows seed to grow a crop to be marketed as such, or to be converted by his animals into a marketable product. It would be hard to conceive of any other goods in which there was so much backwards and forwards movement between all the countries of the world. From east to west, from Japan to Britain, countries are both importing and exporting agricultural seeds, and this, moreover, frequently in respect of one and the same species. Red clover, for example, is harvested for seed and grown as a crop or in seeds mixtures throughout practically all parts of the temperate world, and it would not be in the least surprising if, on occasion, it has been the fate of a particular parcel of seed to cross the Equator more than once.

Seeds, that is to say if they have any value, are living things, and as such are peculiarly sensitive to conditions. From the moment seeds are sown to produce a seed crop until that crop is itself sown in the ordinary business of farming, the quality of seed aimed at and finally produced will be determined by the methods of growing and handling the parent crop, of harvesting, thrashing, and "conditioning" the seed; the methods employed in cleaning and "dressing"; the conditions under which the seed was stored and despatched to its final destination; and over and above all the factors under the control of grower, wholesaler and retailer, the influences of climate and weather to which the growing crop was subjected and to which the seed may have been exposed until finally sown by the ultimate user.

In addition to this it is by no means easy to distinguish and discriminate between all the different kinds and sorts of agricultural seeds; it is a difficult matter to grow and harvest a clean and pure seed crop, and it is no less difficult to eliminate weed seeds and other extraneous bodies from seed as harvested. The ensuring therefore that agricultural seeds of sound quality may be placed on the market demands not only trade integrity of the highest order, but also the exercise of considerable technical knowledge and skill. Unfortunately it has to be admitted that agricultural seeds are still largely handled by vendors who are in no sense of the word seedsmen and who can pretend to no proper knowledge of seeds.

Great Britain has always taken a foremost place in the seed trade of the world, for as well as being a not inconsiderable exporting country she has functioned, largely through London, as one of the chief clearing houses for agricultural seeds—the amount of imported seed that is exported being at all times very large.

Although, speaking generally, questions connected with seeds were early to the fore when it began to be realised that science could be applied with great advantage to matters agricultural, it was not in Britain that scientists or administrators or the great agricultural societies first began seriously to consider what steps might be taken either to regularise such a fundamentally important international trade or to give the farmer sound and accurate advice relative to the purchase of his seeds.

It was largely on the initiative of Germany that the first International Congress on Seed Testing ever held took place in Hamburg in 1906—for Hamburg, like London, had long been an important clearing-house for seeds.

Great Britain, despite her leading position in the seed trade of the world, took no important part in this Congress. She was, however, officially represented by Dr. Güssow, then assistant to the Consulting Botanist of the Royal Agricultural Society. In the *Journal* of the Society (1906) only two pages, however, were devoted to the question of the important matters discussed at the Conference, and it was perhaps typical of the times that Mr. Carruthers (the Consulting Botanist of the Society) in his annual report for 1906 states that he was unable to attend the Conference, "but that he sent his assistant to represent him." The Conference was disposed of in the *Journal of the Board of Agriculture* (October 1907) in one short paragraph in minute type under "Miscellaneous Notes." In view of what happened later it is significant that Ireland was represented at the Conference.

The second Congress, held at Münster and Wageningen

in 1910, would appear to have passed entirely unnoticed in England; no reference was made to it in either the *Journal of the Board of Agriculture*, the *Journal of the Royal Agricultural Society* or the *Journal of the Bath and West and Southern Counties Society*. It is an interesting fact that the only British representative at that Conference was Dr. G. H. Pethybridge, who was in attendance on behalf of Ireland.

Such was the position prior to the war, a position that was largely reflected in both the official and scientific attitude towards seed testing as a service valuable for farmers, independent of its commercial and international aspects which prevailed in this country for so many years.

Notwithstanding the fact that Official Seed Testing Stations had been established in Copenhagen and in Zürich in the seventies—the latter station having issued annual reports since 1877—it was not until 1895 that the Consulting Botanist to the Royal Agricultural Society published anything in the nature of a detailed report on the quality of the agricultural seeds on sale in England, although it is true that in his reports from about 1881 onwards passing comment was sometimes made on seed questions. It was not until much later that the Board of Agriculture for England took any striking official action with reference to informing itself as to the quality of the seeds offered to the farmer, or as to matters connected with seed testing and control. Samples were, however, collected by the Board's Inspectors during the years 1912, 1913, and 1914, and on behalf of the Board these were tested at Cambridge and subsequently formed the subject of an illuminating report by Prof. (now Sir) R. H. Biffen. At about this time the Board also instructed Mr. H. C. Long to visit some of the more celebrated Seed Testing Stations on the Continent, and his report was issued by the Board in August 1914.

In the meantime, however, things had been taking a more concrete shape in Ireland and in Scotland. The Department of Agriculture and Technical Instruction for Ireland had set up a Seed Testing Station in Dublin in 1900, and in 1910 seeds legislation came into force in that country. The Board of Agriculture for Scotland established an official station in Edinburgh in 1914, but this was not at that time supported by seeds legislation.

Evidence was thus accumulating and interest was being aroused in the question of the quality of seeds: this interest was quickened by the appearance of Sir R. H. Biffen's report in 1916, and by reports which had appeared on the condition of the seed trade in Wales in 1914 and in 1916.

These were not, however, the only influences at work. Most of the larger and more responsible of the seed houses had

for long been interested in seed testing, and Messrs. James Hunter of Chester had started selling seeds on a guarantee of germination and purity in 1883, while the quality of the seed supplied by old-established and reputable firms was tantamount to a guarantee of excellence. The institution of a definite system of explicit guarantee supported by actual figures was, however, a progressive move of the first importance. Some of the houses had set up their own seed-testing equipment considerably earlier than this; thus Mr. Martin H. F. Sutton has informed the writer that "Suttons" had set up such equipment by 1840. To meet the growing need of the trade, private seed-testing establishments were also coming into existence in England. The methods of seed testing conducted by private stations and by the seed firms themselves were, however, not co-ordinated, and in the main were conducted by methods which differed from those employed by the Irish Station when it was established. On this account, and in view of the fact that there was no official station for England, it came about that reports bearing the hall-mark of an official station, particularly in cases of large-scale transactions or of arbitration, were very generally obtained from the station in Zürich, which had rapidly attained to a world-wide reputation, and which conducted its tests by methods to which the seed trade had become accustomed and had always regarded as most equitable from its, the trading, point of view. Thus in 1914 no less than thirty-four British seed houses were on the contract list of the Zürich Station, while an increasing number of tests for British firms were also being made in Copenhagen.

This was the state of affairs, and not a very dignified one from the British point of view, when the war broke out. The tendencies we have briefly sketched must inevitably have led to the setting up of an Official Seed Testing Station in England. But it was, however, the stress of war which was finally responsible for the action that was taken in 1917—a year which was the turning-point in the official attitude of Great Britain towards seeds. The seven years period 1917 to 1924 witnessed a complete revolution in this attitude, a period during which England awoke from a state of apathy to take a leading place in the international deliberations and developments relative to seed testing and seed control.

The manner of the awakening, like the previous slumber, was typically English. The accumulating tinder was fired by the energy and enthusiasm of a far-seeing layman, the late Sir Lawrence Weaver, who had been called to assist in the gigantic task of safeguarding the nation's food supplies. The setting up of an official station for England at the Food Production Department in 1917—a relatively simple matter—

was a necessary preliminary to what followed. Far more difficult was the introduction of some method of seed control which the trade would accept and conform to loyally. To this end Sir Lawrence Weaver applied himself with such a rare combination of energy, patience and tact, that the views of the trade—views not necessarily always in its own best interest; the views and ideals of the scientist—ideals not always of practical application to the rough and tumble of actual farming conditions; and the views of administrators—of officials trained to foresee difficulties, and who are therefore singularly liable to make them in advance—were brought to a common denominator in the Testing of Seeds Order that came into operation in January 1918.

The permanent achievement was to follow in 1920, when the war-time order was replaced by the Seeds Act. Sir Lawrence Weaver's custodianship of Britain's seed interests, for in very truth it may be so described, did not, however, end with the placing of the Act on the Statute Book. In 1921 the official station was moved to Cambridge, and became a part of the National Institute of Agricultural Botany, and there at the headquarters of the Institute at long last it came into its own and was established with well-equipped laboratories built for the purpose. The English house having been placed in order with an Official Station worthy to perform its responsible duties and with a Seeds Act, Sir Lawrence Weaver was now in a position to perform what will probably come to be regarded as his greatest service in the cause of providing the British farmer with "Better Seed: Better Crops."[1] He had realised the fundamental necessity of taking all possible steps to rationalise the international trade in seeds, and it was very largely as a result of his endeavours, supported enthusiastically by the late Dr. W. J. Johannsen and Dr. Dorph Petersen (the Director of the Danish Station), that the Danish Government invited a third International Seed Testing Congress to meet in Copenhagen in 1921. At this Congress two exceedingly important steps forward were taken. In the first place it led to the foundation of the European Seed Testing Association, with the setting up of strong committees, now with adequate British representation, to study the whole question of co-ordinating and codifying methods of seed testing; in the second place joint meetings were held with trade associations. The good work started in Copenhagen was not allowed to languish, for in 1924 the first Seed Testing Congress held on British soil, and probably the most representative international gathering which had assembled in England since the war—the Fourth International Seed Testing Congress—took place

[1] The motto of the National Institute of Agricultural Botany.

at Cambridge. At this Congress the last corner-stone was laid in the cause for which Sir Lawrence Weaver had so consistently laboured since 1917—the foundation of the International Seed Testing Association to take the place of the European Association founded in 1921.

It was now left for the Association, ably led by Dr. Dorph Petersen and Dr. W. J. Franck of Wageningen and supported by the directors of all the leading national stations, to tackle seriously the problem of setting up international rules for seed testing—and at the fifth congress held in Rome in 1928 this all-important matter received the detailed attention of sixty delegates from all over the world.

By 1928 thirty-one countries had joined the International Association, which had a membership representing 170 stations, so that the meeting in Rome was of a truly international character, and like the two previous congresses was symbolic of something far greater than the desire of scientists to meet together and discuss matters merely of interest to themselves.

That agreement should have been reached on all points was hardly to be expected, having regard to the complicated nature of the technicalities of seed testing, the particular needs and points of view of the different countries, and the wide range in the degree of efficiency of the large number of official stations interested. The foundation upon which complete agreement will assuredly grow has, however, undoubtedly been laid—a very great achievement indeed, and an achievement the significance of which far transcends matters connected merely with the seed trade of the world.

Practically all the controversies that have arisen in connection with seed testing and seed control have been due in the last resort to two fundamental difficulties: firstly, the difficulty of arriving at a proper and generally acceptable definition of what constitutes a seed; secondly, that due to the fact that a method of testing and reporting which gives the most reliable, or at all events the most acceptable, information to the trade in connection with large-scale transactions may not necessarily be equally suitable for giving the farmer the information which he requires or which he can most readily understand.

Before we can usefully discuss some of the more important issues with which modern seed testing is confronted, we must, therefore, devote a little space to a consideration of germination and purity, since comparable and reliable figures can only be obtained if all stations conform strictly to the same conventions and rules in the conduct of their tests.

In so far as the definition of a seed is concerned, the grasses present difficulties peculiar to themselves. The fruit of the

ordinary grass is a caryopsis, but the recognisable "seed" of commerce nearly always consists of more than this (in some samples of timothy and *Agrostis* naked caryopses freely occur). Thus the "seed" of the rye-grasses and fescues include the caryopsis surrounded by a pair of paleæ, while that of meadow foxtail, for example, is the whole unifloral spikelet.

It would be manifestly absurd and altogether too laborious when making a purity test to shell every caryopsis and to place all the chaff (paleæ and glumes if any) with impurities, and to regard only naked caryopses as "pure seed." Consequently it has come to be generally accepted that the "seed" of a grass species should be regarded as the caryopsis, together with such parts of the spikelet as normally surround it.

The matter would be perfectly straightforward if these "normal surrounds" always contained a properly formed caryopsis—in practice, however, such is altogether too frequently not the case. What then is to be the definition of a "seed"? The pure botanist would immediately say "a caryopsis and its surrounds," and this was the view taken at an early stage by the pioneer Continental seed testing stations. On this basis pairs of paleæ devoid of a caryopsis or only containing a rudimentary caryopsis are regarded as, and placed with, impurities. To conduct a test by this means, however, places a considerable responsibility on the analyst, for in practice it is no easy matter to discriminate between caryopsis-bearing and non-caryopsis-bearing paleæ; obviously, therefore, it is not easy to attain to uniformity in testing when the purity test is made on this basis. Partly for this reason the Irish Station when first established counted as "pure seed" the appropriate part of the spikelet, whether it contained a caryopsis or not. This latter method was employed by the English Station when first founded, and by stations in the British Dominions.

The "Continental method" and the "Irish method," however, give very different results, especially in the case of samples containing excess of "light seed" (= appropriate portion of the spikelet not containing a fully developed caryopsis). Thus, for example, a sample of rye-grass containing 50 per cent. by numbers of light seed could not possibly germinate over 50 per cent. if tested by the "Irish method" (since the light seed would have been counted into the germination test)—while, if the light seed was removed (Continental method) the germination might be over 80 per cent., and since "light seed" is not heavy the percentage purity would not be anything like proportionally reduced.

Very strong arguments can be adduced in favour of both methods of testing, looking at the matter solely from the point

of view of the information supplied. A report based on an "Irish" test would undoubtedly emphasise the inferiority of a very poor sample more crushingly than would one based on a Continental test. The former method of testing is also more intelligible to the farmer, since when sowing or handling a sample he would be unlikely to discriminate closely between "light" and "heavy" seed. The "Continental method" on the other hand gives results of greater value to the seedsman, not necessarily because it shows a higher germination figure, but rather because it enables him the better to estimate the potential worth of a bulk sample after it has passed through appropriate cleaning machinery.

International uniformity in testing, with correlated rationalisation of the trade in grass seeds, was out of the question until agreement could be reached on this long outstanding question. Finally, the views of the trade prevailed, as was inevitable in such a matter, and the English, the Scotch, and the two Irish Stations agreed to adopt the "Continental method." In the meantime methods for separating "heavy" from "light" seed had been improved and standardised, and every year shows great advance in the agreement reached between tests conducted on referee samples by the various stations of the world.

On the balance there can be little doubt that the adoption of the so-called "Continental method" by this country was a proper step to have taken, and it is a step which has already enormously assisted in the standardisation of methods of testing throughout the world, and what is equally important, it is bound to react upon the standard of equipment and technical skill deemed to be consistent with the responsibility accepted by an official station. There can be little doubt that unification in testing has been greatly hampered, and is still hampered, by the fact that there are far too many more or less official stations testing seeds under altogether too heavy financial difficulties, which inevitably leads to poor equipment and an insufficiency of thoroughly trained assistants.

The first essential of any country which aspires to build up an export trade in seeds, or adequately to check its own imports, is to provide facilities for testing by approved, modern, and standardised methods, and this is a fundamental fact which it is to be feared has not yet been properly appreciated by all the countries concerned, and some of the British Dominions are by no means blameless in this respect.

The commercial "seed" of the great majority of the clovers, roots, and vegetables is also a seed in the botanical sense of the word, so at first sight there should be no difficulty in arriving at a definition of "pure seed" acceptable to all countries and to all seed analysts. This is, however, very far

from being the case, for what view should be taken relative to broken, cracked, shrivelled, and other damaged seeds? At the present time it is the sharp divergence of opinion on this question which has been chiefly responsible for such lack of agreement as occurs between tests conducted by different stations, and for the failure on the part of the International Seed Testing Association to have introduced a code of rules acceptable in all particulars to all the countries of the world.

Broadly speaking there are two schools of thought on this question: the European (now endorsed by Britain and Ireland) and the American. The view taken by the Association of Seed Analysts of North America and that officially adhered to by the United States is that all seed, whether shrivelled, damaged or broken, should be regarded as pure seed (save that in cases of seeds broken in two, the larger portion should constitute the seed) and consequently take its chance of being counted into the germination test. In broad principle the European *cum* British view is that only seeds that are prima facie capable of germination should be regarded as pure seed. Obviously in the case of samples containing much damaged seed the American method of testing will give much lower germination figures than the European, and the trade in seeds across the Atlantic has undoubtedly been definitely hampered by the divergent views so tenaciously held, which lead to results of tests made in Europe and in America frequently showing disconcerting discrepancies.

The question of broken seeds presents further difficulties, for many seeds are internally broken or fractured, resulting from damage during the process of thrashing and cleaning; such internal damage frequently cannot be recognised except during the progress of the germination test. Experience and skill are needed to differentiate between "permissible" and "unpermissible" germinations (germinations competent to produce a growable seedling and those which are not). A great deal of investigation has been conducted at the Official Station at Cambridge, and at the leading European Stations on "broken growths" supported by growth tests, and very definite and satisfactory rules can now be laid down as to the methods of testing to be adopted, and the conventions to be adhered to. But until all nations come into line on this question, and until all analysts are sufficiently well trained to appreciate the necessary distinctions between seeds variously broken when making a purity test and between "good" and "bad" germinations when making germination counts, the widest discrepancies in the results of tests are liable to occur, and bad feeling, or at least uncertainty, is likely to be engendered between the seeds merchants of different countries.

The present time, when England and the United States of America are endeavouring to solve problems of the greatest world importance in a spirit of good-will, would seem appropriate for reaching agreement on the relatively insignificant but none the less important question of damaged seeds, and it would be a graceful act on the part of America to accept the European view—a view that has been reached as the result of extensive research at splendidly equipped stations over a great number of years, and in parenthesis it may be added that it behoves our own Dominions to come rigorously into line on this question.

The United States of America are large importers and exporters of seeds, and until agreement can be reached between America and Europe on the question of what constitutes a seed, and until the equipment of all the seed testing stations which act in an official or quasi-official capacity on both sides of the Atlantic conforms to the same general principles, the rationalisation of the world's seed trade is impossible.

Important questions arise relative to the actual conduct of germination tests. Ideally it is to be desired that such tests should be quantitatively informative as to the ability of the seeds to establish themselves and grow in the soil. This is, however, an ideal which is impracticable to reach, having regard to the immense range of soil and climatic conditions under which seed is sown, and having regard to the wide range in the potential capacity for establishment of seeds which are viable. Thus for the purposes of seed control and trade transactions an official station can only seek to ascertain the percentage viability of the seed, and cannot in its reports attempt to discriminate between seeds of high and seeds of low "establishing power."

Differences of opinion therefore exist as to what steps are permissible in order to ensure that every viable seed does in fact germinate during a test of reasonable duration. Formerly very rigid rules were laid down as to the optimum temperatures at which the seeds of different species should be germinated. It is now realised more fully that different samples of seed of one and the same species differ widely as to the optimum conditions favouring germination. The temperature, degree of moisture, and amount of illumination, if any, required are a function of the conditions under which the seed was produced and harvested, and therefore apart from the vagaries of the weather from year to year in any one country, are in a broad way likely to differ in respect of seeds harvested in different countries. Pretty general agreement has now been reached on this question and very considerable latitude is allowed. It is a common practice to test the more difficult

seeds by one or more methods and to report the results obtained from the tests which the event proves to give the highest viability.

At the commencement of the season it is also desirable to conduct experimental tests to ascertain the conditions for germination most likely to suit the current harvest. This is a practice largely adopted amongst the leading stations, accompanied by a ready exchange of experiences. Recently the point has been raised as to the legitimacy or otherwise of applying artificial stimulation (*e.g.*, chemicals) to seeds when under test. The prevailing view, and we think the proper view in the present state of our knowledge, is that stimulation should not be allowed.

Rather different is the case of " conditioning " in respect of seeds which are obviously not " germination ripe." The phenomenon of delayed germination due to " unripeness " is largely met with in cereals tested soon after harvesting and required for autumn sowing, and it occurs also in cocksfoot and some of the grasses, as well as in other species.[1] " Conditioning," accompanied by testing under extreme conditions, is practised by nearly all stations in respect of cereals. In the case of these seeds artificial drying is sometimes practised as a pre-treatment, as is subjecting to low temperatures, and the germination tests are conducted at considerably lower temperatures than are applicable to normal " germination ripe seed." It is difficult to standardise methods of " conditioning " because delayed germination is frequently due to different causes, but in our view conditioning is an absolute necessity, and should therefore be regarded as perfectly legitimate, provided the methods employed at all stations are the common knowledge of all.

Once having decided what constitutes a pure seed the routine purity tests are straightforward and permit of no ambiguity. Questions do arise, however, as to the degree of separation that is desirable and practicable; and in this connection the tendency of official stations is to give increasingly detailed information in their reports. The chief categories in which impurities can be placed are: (1) other useful seeds; (2) inert and harmless matter; and (3) weed seeds. But each of these categories allows of amplification. Thus in respect of inert matter in the case of grasses it is of great convenience to the seeds merchant to be informed of the actual percentage of light seed—for this he can easily remove. As to weed seeds it is usually the practice to particularise the percentage of

[1] The question of delayed germination and delayed seedling growth has been under observation for some time at the Welsh Plant Breeding Station, and is undoubtedly a very widespread phenomenon.

"injurious or noxious" separately from "total" weeds, but this is by no means always done unless the amount of noxious weeds exceeds a certain minimum laid down in a particular country's regulations.

There can be little doubt that the question of reporting on tests is just as important as the methods of conducting them. A report can be either advisory in character or merely supply the information demanded by a country's system of seed control—in practice most reports are a compromise between the advisory and legally necessary.

The directors of all the leading seed testing stations are coming more and more to realise the great value of issuing advisory reports. To do so, however, to the best advantage means increased clerical and analytical staff and, as will be shown hereafter, considerably augmented facilities for research.

To confine ourselves for the moment to questions of germination and purity. In our view an advisory report should cover at least the following points (explanatory notes are added as seems necessary) and should be issued in connection with every sample tested.

(1) (*a*) Percentage gross impurity (always reported but perhaps most usually as percentage "purity"). (*b*) For grasses, percentage "light" seed (information so valuable to the trade). (*c*) Percentage weed seeds. (*d*) Percentage "injurious" weed seeds, regardless of whether this exceeds a legal minimum (1–2 per cent., according to species, in this country for purposes of declaration). (*e*) The number of seeds of each injurious weed seed per lb. Less than 1 per cent. of seeds of weeds like docks in red clover would be sufficient to have a serious land-fouling effect. Thus, Sir R. H. Biffen has shown that a sowing of 16 lb. per acre of red clover with 1 per cent. of docks would distribute 11 dock seeds per square yard over the field. Certain of the weeds scheduled as "injurious" or "noxious" may be far more serious in some districts than others of the same country. For example, soft brome grass in perennial rye-grass is not nearly so harmful in samples sown in regions of high rainfall as in the drier parts of this country, while Yorkshire fog is the more objectionable of these two impurities under the former conditions. Similarly the seeds of wild carrot are, generally speaking, more serious in samples intended to be sown in the east and south of England than in Wales or in many parts of the west of England. The station for the North of Ireland we understand has adopted this admirable method of reporting relative to weed seeds, the educational value of which is bound to be very great. (*f*) Presence of dodder (this is reported when the quantity exceeds a prescribed minimum). The importance of dodder in samples,

however, varies for different districts; none of the clover dodders appear to thrive in Scotland, while the large or "Chilian Dodder" is very seldom seen in clover fields in Wales. (*g*) Percentage of seeds so broken as to be regarded as impurity (valuable information to the grower and to the trade in regard to the proper handling of seeds when thrashing and cleaning). (*h*) Percentage hard seeds in clovers (this is invariably reported).

(2) (*a*) The percentage germination of pure seed (this is always reported). (*b*) The energy of germination of pure seed, that is to say, the germination in a prescribed number of days less than that allowed for a full test (this is usually reported). (*c*) The percentage of "broken growths" (*i.e.*, non-seedling producing germinations). This information, like information relative to obviously broken seeds, is valuable to grower and merchant for the reason stated above. (*d*) In certain grasses (*e.g.* cocksfoot) the percentage by weight of the sample which consists of a number of "seeds" held together (*i.e.*, a part or the whole of the spikelet instead of the spikelets broken up into single caryopses each with its surrounding paleæ). Samples with a high percentage of whole spikelets are not as satisfactory as those where all the "seeds" are separate, as this condition reacts against proper distribution over the land, and probably denotes incomplete maturation and ripening.

All of the points narrated above come under the eye and enter into the consciousness of the trained analyst, and are usually recorded, so that to report upon them would only entail more weighings, more countings, and more clerical work.

The foundation of seed testing and of seed control has been germination and purity: that is to say, the edifice, such as it exists to-day, has been built on the quality of seed *qua* seed. Obviously the quality of seed is of the greatest importance—for it is exceedingly uneconomic to ship large parcels of seed full of impurities and of low germination from country to country, and little less than disastrous for the farmer to sow on his land seeds incapable of producing a good crop or containing excess of land-fouling weeds.

The agricultural usefulness of seeds is, however, by no means only a function of their freedom from weed seeds and their power to germinate. Modern knowledge now teaches us that seeds should be above suspicion in three further respects, namely: (1) they should have the maximum potentiality for soil germination and establishment; (2) they should represent the variety or strain, and (or) have been grown in the particular locality or country which renders them most suitable for use in the particular locality or country where they will be sold for sowing; (3) they should not be in any way contaminated with seed-borne diseases.

It must be freely admitted that up to the present time seed testing and seed control (despite the fact that the stations in varying degree have devoted attention to the above points, and despite the fact that seed legislation in most countries is now taking cognisance of place of origin and variety) have tended in the direction of causing both farmer and merchant to place too much importance on germination and purity and altogether too little on these other equally essential attributes of seeds. Of what avail is it if the seed (though viable) is unable to establish a " plant " in the soil ; or if it represents a variety or strain incapable of producing a proper crop ; or if it carries within or upon itself the germ of its own destruction ?

The present state of affairs is understandable, for germination and purity represent tangible things which can be expressed in figures and are intelligible to the least imaginative of merchants and of farmers. Small wonder then that the money value of seeds is frequently influenced by differences in percentages of germination or purity which are often too slight to be of any real crop-producing significance. This method of evaluation inevitably tends either to undue blending of seeds or to excessive machining and cleaning, which latter practice frequently leads to waste of valuable seed. Of greater significance is the fact that the prominence given to germination and purity has retarded the general awakening of the seeds merchants and farmers of the country to the importance of the less immediately showy, more subtle, and in many ways more far-reaching properties of seed from the crop-producing point of view.

The position created by our own Seeds Act is highly instructive. Briefly the basis of the British Act is declaration. The vendor, *inter alia*, has to declare (1) germination and purity (or in certain cases the figures need not be declared if they are at or above a prescribed standard) ; (2) the variety or, in the case of grasses and clovers, country of origin of the seed ; (3) the percentage of injurious weed seeds : (*a*) in grasses if over 2 per cent. and (*b*) in clovers if over 1 per cent. ; (4) the percentage of yellow suckling clover (*Trifolium dubium*) and other small annual clovers when present in wild white clover in excess of 2 per cent.

None of the above clauses were intended to be penal ; they were framed in order that the farmer should know precisely what he was purchasing, and it was to be hoped and supposed that both the merchant and the farmer would form an estimate of what standard of germination and purity was appropriate and reasonably to be expected relative to each variety and strain or source of origin as such of seed. This

has not, however, proved to be the case, and to a certain extent therefore the Act has undoubtedly not borne its full fruit of usefulness, and in some respects has probably completely defeated its own ends.

The larger seed houses have undoubtedly regarded some of the clauses as being tantamount to penal, and have deemed it as not compatible with their reputations or with their dignity to sell seed that would have to carry anything approaching a " damaging " declaration, either in respect of germination or purity—no matter how super-excellent and valuable the strain or variety of seed.

This is well exemplified in the case of wild white clover, which from old pastures is exceedingly liable to contain much more than 2 per cent. of yellow suckling clover—valuable in itself, but not of course as valuable as the wild white clover. The clause was introduced simply that the farmer should know what he was buying, and be able to form his own estimate of the value of the seed. The knowing farmer, however, should rather welcome well over 2 per cent. of yellow suckling clover, for this " impurity " speaks of the genuineness of the wild white clover.

The reaction of the big seed houses has, however, shown itself in an aversion to purchasing wild white clover containing much suckling (including some of the most reliable of wild white that is harvested), and (or) in the adoption of very wasteful cleaning operations to reduce the percentage of this innocent little clover to below 2 per cent. The net result has unfortunately been to keep up the price of the seed of this all-important herbage plant and further to have wasted really considerable quantities of it in cleanings. This is the more unfortunate, because speaking generally the weed seeds in wild white clover are not of a very serious character, and the greatest of all needs is that the seed should be cheap and genuine. The writer has had ample opportunities of seeing excellent results obtained from cheap relatively uncleaned wild white clover, and although he must take his share of responsibility for the introduction of the " yellow suckling " clause in the Seeds Act, he would now most strongly advocate the repeal of that clause, or alternatively he would advocate increasing the amount that renders declaration necessary from 2 per cent. as at present to 10 per cent. The modification or the repeal of this clause is now the more to be desired because an official and voluntary registration scheme is to be introduced to safeguard the dealings in the genuine wild white clover, and it is much to be feared that the continued operation of the 2 per cent. bar will tend to react against a desire on the part of some of the larger merchants to deal in what will most

certainly prove to be amongst the very best of the registered stocks.

To attach too high an importance to germination, and to regard it as an attribute of seeds uninfluenced by other considerations has had the effect moreover of keeping up the price of certain varieties or nationalities of seed which on the average tend to have rather low germinations, and it is to be feared in certain instances also of causing the blending of higher germination seed of another strain or nationality with that of the variety or strain demanding the higher price. The position as to Montgomery extra-late red clover is an excellent example. In some years a germination of over about 70 per cent. can hardly be expected, and if seed of a higher germination than this is to be marketed on any considerable scale it can only be done by excessive and wasteful dressing or by blending with some other stock of an altogether higher germination.

It is because the trade has been in a sense educated to assess germination and purity as of supreme importance that it has not conducted through its catalogues a propaganda in favour of variety, strain, and source of origin, comparable to the propaganda it has so long and so successfully conducted in regard to germination and purity. There is very little hope of the importance of quality in this latter connection being at all properly appreciated until all concerned are prepared to regard very low germinations—even on occasion as low as 50 per cent.—as being all that can be reasonably expected of certain important varieties and strains in years which may have been unusually adverse to seed production in the localities concerned.

The recognition and active appreciation of the significance of variety, strain, and source of origin can, however, be enormously hastened by two further activities, in both of which the seed testing stations will have to participate to an ever-increasing extent. The one is the extension of schemes of registration of stocks of important varieties, and the other is the establishment of tests which will reveal variety, strain, and source of origin by an examination of the seeds and (or) of the young seedlings. The farmer is naturally averse to paying the high price demanded for super-varieties and strains unless he can feel assured that he will in fact get what he requires.

Great Britain may fairly claim to have taken a pioneer part in matters closely related to the question of registration. Thus a very large number of samples of wild white clover and of late-flowering red clover are grown annually to maturity in connection with both the English and Scotch Seed Testing Stations in order to test the accuracy of the descriptions under

which they are sold,[1] and it is symptomatic of the trend of British opinion that the former station is associated with the National Institute of Agricultural Botany and the latter with the Plant Registration Station for Scotland. Developments no less pregnant with promise have been the founding of the Cornish Marl and Montgomery Red Clover Growers' Associations on an entirely voluntary basis—the members in each case submitting to inspection of their standing crops by a competent botanist. It is also interesting to record that the question of the advisability of adopting some scheme of registration in respect of the more important species and strains is now under serious consideration in New Zealand. It follows that the developments in connection with the official scheme of registration of wild white clover in this country, previously referred to, will be watched with the greatest interest, and it is to be hoped that every endeavour will be made to simplify the working of the scheme as experience is gained. The great risk of all innovations planned to safeguard the quality of seeds is that they are likely to increase rather than reduce price. The ideal to be aimed at must always be twofold—to ensure adequate production of genuine seed of the most desirable varieties and at the cheapest possible price.

The institution of schemes of registration makes it doubly important that the seed analyst should be able to establish tests for the identity of variety, strain, and country of origin. On this account, and relative to seed-borne diseases, the urgent necessity of research is immediately manifest. A great deal of work is at present in progress, more particularly in relation to the first and perhaps the most urgent of these matters; but few, if any, countries have fully realised the immense scope for valuable research that exists in connection with seed testing in its wider and more far-reaching aspects.

The writer having been associated with seed testing in its routine aspects, and having also conducted research on seed questions, and being now intimately connected with breeding and other work bearing upon variety and strain, makes bold to assert that it would pay a country that thinks it worth while to establish and maintain a well-equipped seed testing station, to place at the disposal of its station funds, quite as considerable as are necessary for the conduct of routine tests, for the sole purpose of research. This statement is not lightly made, for it is the undoubted part of the applied scientist to

[1] As the result of growing-on tests conducted in Scotland in 1926, it was shown that of a large number of samples of white clover sold as wild white only 60 per cent. were undoubtedly genuine, while of 137 samples of late-flowering red clover 18 per cent. gave rise to plants which were wholly not of this valuable type.

leave no stone unturned to make practicable what is possible, for such is and always has been the sure high road to progress.

Enough research has been done to show that it is possible to devise tests as to variety, strain, and origin, and tests which could ultimately be made of practical application on a basis of reliability quite as secure as that upon which germination and purity depend.

We have the evidence as to contained weed seeds concerning which every year more data are being accumulated. We have the colour tests applied to germinating seedlings, optical and other tests applied to seeds, and a variety of critical methods of examining seeds and young seedlings for minute differential characteristics. More recently we have evidence to suggest that in some cases seedlings may be made to show their varietal character by their response to different periods of illumination.

Less research has been conducted on the question of the prognostication of a sample's ability to produce established seedlings, but there is sufficient evidence to show that detailed investigation relative to pre-treatments, accompanied by germination tests conducted at extreme temperatures and under extremes of moisture content of the germinating medium, checked by growth tests under varied conditions, would provide the necessary body of information for the establishment of routine tests of practical applicability.[1]

These considerations bring us back again to the desirability of greatly extending advisory reporting—of extending the educational value of seed testing. In the present state of our knowledge, reports on the matters immediately under review can hardly be absolute, and can for that reason only be advisory in character. It would probably be advantageous therefore to differentiate definitely between " official reports " —such as could be regarded as legal evidence in cases of disputes—and " advisory reports " which would not be regarded as evidence under the law.

In time, and as the necessary body of information accumulated, the distinction would not have to be made, since the reports on all aspects of seed testing discussed in this paper would come to be of equal reliability.

From the advisory point of view, the system of control adopted in this country has in one rather unexpected direction been of immense value and a great success. We refer to the fact that tests for the purpose of legal declaration have not

[1] Since writing this paragraph an important paper by Dr. Franck has appeared on this subject, in which he takes up practically the same position as that adopted in the present paper.

necessarily to be conducted at an official station, but can be undertaken by the seed houses themselves, provided their seed testing stations are conducted in such a manner as to qualify for a licence. By a system of referee samples the results obtained at the licensed stations are checked by those obtained at the official station, and further to this, courses in seed testing are held at the official station and certificates of competency are granted to the trade analysts attending those courses. The result is, that the eighty odd seed houses having their own licensed stations have in effect an advisory department which is based on scientific methods and a scientific point of view, and consequently perhaps the greatest benefit which British seed legislation has produced has come about through a mode of procedure which at its inception was looked upon with decided misgivings—shall we say—by the writer and some of his scientific colleagues deeply interested in seed testing and seed control.

We have endeavoured in this article to emphasise the great work in the cause of agriculture that can be accomplished through seed testing and reasoned seed control, but we fear a considerable amount of research remains to be conducted before the scientist working in this and kindred fields can win to his side " the public that liked him not, whom yet he had laboured for," and before the world's trade in seeds can be rationalised to the best advantage of the trader, the farmer, and the great consuming public.

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THE APPLICATION OF GOLDSCHMIDT'S HYPOTHESIS TO THE DIFFERENTIATION OF SEX IN VERTEBRATES

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A SATISFACTORY explanation of the mechanism which determines whether a living organism will develop into a male or a female is generally admitted to be provided by the sex-chromosome theory. It has been observed in a large number of plants and animals that there are a pair of chromosomes, the X-chromosomes, present in one sex, which are represented in the other sex by a single X-chromosome, with or without a small partner, the Y-chromosome. The sex with two X-chromosomes is homogametic, since all the gametes produced by it contain one X-chromosome. The other sex is heterogametic, since only half the gametes produced will contain an X-chromosome, while the other half will either be without a sex-chromosome or will contain the small Y-chromosome, according to whether the chromosomal constitution of the sex is XO or XY. It is known that, in the majority of animals, the male is the heterogametic, XO or XY, sex and the female the XX or homogametic sex. The converse, however, is true in butterflies, moths, birds, and probably a few other forms. It is frequently convenient to call the two similar sex-chromosomes occurring in the males of these forms the Z-chromosomes, and the small partner of the single Z-chromosome of the female the W-chromosome. This distinctive symbolism merely implies that the sex-chromosomes of these forms should not, in the present state of our knowledge, be assumed to be strictly comparable to those of forms in which the male is heterogametic. It is customary, however, to assume that X = Z and Y = W, for the purposes of general discussion of the sex-chromosomes, when it is not necessary to distinguish between male and female heterogamety.

The sex-chromosome theory, in this simple form, thus postulates that the sex of every individual is determined at fertilisation by the chromosomal constitution. This constitution depends on that of the gamete from the heterogametic parent, since all the gametes of the homogametic sex have a

similar constitution. Thus in the majority of animals, in which the males are heterogametic, the presence or absence of an X-chromosome in the sperm which effected fertilisation is the determining factor, while in birds and lepidoptera it is provided by the presence or absence of a Z-chromosome in the ovum.

The direct observation of the occurrence of sex-chromosomes in the cells of a large number of animals and plants is supported by breeding results. Thus the mode of hereditary transmission of certain characters, known as sex-linked characters, can only be explained by the assumption that the factors or genes which determine them are borne by the X- (or Z-) chromosomes. A large number of characters, in many forms of animals, including man, have been shown to be sex-linked.

The Y-chromosome, in some cases where it is present, appears to be entirely functionless and does not carry genes. This is the case in the male Metapodius, in which it is normally present. This is shown by the fact that the occasional absence of the Y-chromosome does not result in any visible abnormalities. In other cases, however, there is no doubt that the Y-chromosome is functional and does carry at least a few genes. These show themselves as hereditary characters which are transmitted through the male line only. One such character is the web-toed character in man. It is clear, therefore, that the Y- or W-chromosome may, in certain forms, play a part in heredity. It has been suggested that it may, in some forms, participate in the determination of sex—an idea which will be referred to again.

The sex-chromosome theory, in its essentials, is firmly established. Cytology and genetics combine to provide an overwhelming body of evidence, which is steadily increasing, in its favour. It must be realised, however, that the sex-chromosome theory does not require that the X-chromosomes are the only factors in determining sex, or even the most potent. It is necessary only to assume that they are normally the decisive factors. This distinction is of the utmost importance and should never be forgotten.

The nature of this normally decisive influence of the X-chromosomes in determining sex is an important problem. Obviously this influence must be either qualitative or quantitative. A little reflection will show that the action of the X-chromosome cannot be qualitative, since it is the X-chromosome of the male, in forms in which the male is heterogametic, which, when passed on, determines that the offspring shall be female. The genetics of the fruit-fly, Drosophila, provide positive evidence in favour of the view that the action of the X-chromosomes in determining sex is quantitative. The strongest of

this evidence is provided by the chance occurrence of individuals with an abnormal number of chromosomes. Such individuals are produced when the members of some or all of the pairs of homologous chromosomes fail to separate at the reduction division, and germ-cells are produced in consequence which contain either too many or too few chromosomes. This non-disjunction of the chromosomes may involve either the sex-chromosomes or the autosomes or both. Bridges obtained females with three sets of chromosomes, which had arisen by chance, presumably through non-disjunction. These provided strains of Drosophila which had from one to four X-chromosomes variously combined with diploid, triploid, or even tetraploid sets of autosomes. The various combinations of X-chromosomes and sets of autosomes obtained are summarised in the following table.

No. of haploid sets of autosomes.	No. of X-chromosomes.	Sex.
		Super-female.
		Female.
		Female.
		Normal female.
		Intersex.
		Normal male.
		Super-male.

Those individuals which had three X-chromosomes accompanied by three sets of autosomes, or four X-chromosomes and four sets of autosomes, were practically normal females. In other words, those in which the ratio : No. of X-chromosomes : No. of sets of autosomes was one of equality, as in the normal female, were themselves females. The examples with a greater proportion of X-chromatin, which had three X-chromosomes but only two sets of autosomes, were abnormal sterile females. They were in fact super-females. Similarly, the examples with one X-chromosome and three sets of autosomes, a smaller proportion of X-chromatin than in a normal male, were super-males. Finally, the group with two X-chromosomes and three sets of autosomes, a ratio intermediate between that of the normal male and normal female, was found to be composed of intersexes. These results show clearly that the action of the X-chromosomes is quantitative. Moreover, it is not the absolute quantity of X-chromatin which determines sex, but the quantity relative to that of the autosomes. Apparently the autosomes as well as the sex-chromosomes participate in the mechanism of sex determination. It is because normally they are equally distributed in the two sexes that they do not play a decisive part, as the sex-chromosomes do, in determining sex. Further evidence of the influence of the autosomes on sex in Drosophila is provided by the work of Sturtevant, who

found that the presence of a recessive gene, situated in the second chromosome (autosome), resulted in individuals with two X-chromosomes developing into intersexes of a definite type instead of becoming normal females.

These considerations enable us to conclude that the X-chromosomes are normally the decisive factors in determining sex and that their action is quantitative. The autosomes are concerned, and their action is quantitative also. The quantitative action of the X-chromosomes and autosomes is mutually relative, not absolute. It is possible that other parts of the cell, such as the cytoplasm or the Y-chromosome, are involved also in the mechanism of sex-determination.

Morgan suggested a useful symbolic method of representing these factors. He represented the factor for maleness by M and that for femaleness by F. It was assumed that M was borne by the autosomes, and was present equally in all germ-cells, in forms in which the male is heterogametic. The factor F was assumed to be present in all X-chromosomes. These assumptions imply that all the ova will carry the factors FM. The X-chromosome-bearing sperms will be FM, while the others will be M. This will result in half the fertilised ova being FFMM and half FMM. The former will produce females and the latter males, assuming that $F > M$ but $F < MM$.

It is necessary to employ the converse of this formula in dealing with forms in which the female is the heterogametic sex. The factor M will then be borne by the Z-chromosomes and F by the autosomes. All the sperms and the Z-chromosome-bearing ova will then be FM, while the W-chromosome-bearing ova will be F. The resulting offspring will be FFMM = male and FFM = female, where $M > F$ but $M < FF$. It must be remembered that these formulæ are purely symbolic, but they are convenient and they fit the observed facts remarkably well. Goldschmidt has adapted them to account for the occurrence of intersexes in the offspring of certain racial crosses of the Gipsy moth (*Lymantria dispar*). The principles involved in this application are far-reaching in importance. It is becoming increasingly evident that Goldschmidt's hypothesis has a much wider application than that of providing an explanation of intersexuality in moths. Before proceeding to consider the hypothesis in its more general aspects it is necessary to examine the evidence, provided by Lymantria, on which it is founded.

It is well known that crosses between species or different geographical races of the same species of moth frequently result in the production of intersexes in the F_1 generation. This fact led Goldschmidt to investigate the results of crosses of different geographical races of the Gipsy moth (*Lymantria*

dispar). This moth is particularly suited to the purpose on account of its wide geographical distribution, and the very marked differences between the sexes, which render the identification of intersexes easy. It was soon found that certain crosses invariably produced intersexes. Thus European females mated with Japanese males produced normal males and intersexual females. Those intersexual females which were still capable of reproduction produced an F_2 generation in which half the females were normal and half were intersexual. It is clear, therefore, that the factors concerned are Mendelian in character, since typical segregation occurs in the F_2 generation. The reciprocal cross of Japanese females with European males produced only normal males and females, as did pure strains of either race.

It was found that many other crosses also produced intersexes. Moreover, the degree of intersexuality, or amount in which the male and female characters were combined in the intersexes, was constant for any given cross but varied in different crosses. It was possible, therefore, by making different crosses, to produce a series of intersexes of different grades between the normal female and the normal male. Such a series in which the female offspring show an increasing degree of development of male characters is summarised in the following table of crosses between different local races.

Cross.	Female offspring.
Gifu ♂ × Kumamoto ♀	Slightly intersexual. Fertile.
Gifu ♂ × Hokkaido or Schneidemühl ♀	Weak intersexes. Sterile.
Gifu ♂ × Fiume ♀	Moderate intersexes. Secondary sexual characters male. Ovaries undeveloped. Sterile.
Japanese (unknown origin) ♂ × Schneidemühl ♀	Strong intersexes. Externally completely male. Ovaries more or less transformed into testes.
Ogi or Aomori ♂ × Schneidemühl, Fiume or Hokkaido ♀	All females completely transformed into males. Fertile.

The proof of the complete transformation of all the female offspring of the last-mentioned cross into males is shown, not only by the fact that only males are produced, but also embryologically, by showing that the gonads were ovaries which transformed into testes, and genetically, by the results of crosses with normal females of other races.

Such a series of intersexes results when males of a given race are crossed with females of a number of different races, and also when females of a given race are crossed with males of a number of different races. Further similar series showing stages in the transformation of males into females can also be produced by making suitable crosses.

A number of conclusions may be drawn from these experi-

ments. 1. The inheritance of the factors concerned is Mendelian. This can be seen from the first example of Japanese ♂ × European ♀ in which the females of the F_1 generation were intersexes, while half of those of the F_2 generation were normal and half intersexes; segregation having taken place. 2. This example shows also, bearing in mind that the Z-chromosome of the female is derived from the male parent, that the factor for maleness is located in the Z-chromosome. 3. The factor for femaleness is of purely maternal inheritance. This is shown, for instance, by the occurrence of male intersexuality. It becomes clear if we apply Morgan's symbolism FFMM = ♂ FFM = ♀, where $M > F$ but $M < FF$. Males inherit the factor F from both parents with the Z-chromosomes. If they inherit the factor F from both parents, intersexes will not be produced because the sum of the two factors FF will always be less than the sum of the two factors MM. This will be true no matter what the actual values of these factors may be in the two races, since M must necessarily be greater than F in any given race. It must be assumed, therefore, that F is purely maternal in inheritance. Goldschmidt suggests for these and other reasons that the factor F is located either in the W-chromosome or else in the cytoplasm of the egg. 4. The effect of both these factors is quantitative. Females of a given race mated with males of different races result in the production of different grades of intersexes. Therefore the value of the factor for maleness, located in the Z-chromosome, must vary in different races. Conversely, the fact that males of a given race mated with females of different races give different results, for each cross shows that the factor for femaleness also varies according to the race. 5. The values of both factors are constant for any given race and both vary in different races.

Goldschmidt has adapted Morgan's symbolic formula to combine these conclusions. The female factor, located in the cytoplasm of the ovum or in the W-chromosome, is represented by F. The factor for maleness, located in the Z-chromosomes, is represented by M. Then the formula is (F) MM = ♂ and (F) M = ♀. Since the factor F is derived from the mother only and is handed on equally to both male and female offspring, it is represented in brackets. It is necessary to assume that $F > M$ but $F < MM$ to account for the fact that in any given race males and females are produced in approximately equal numbers. Since the action of both F and M is strictly quantitative, they must admit of numerical representation. It was possible to arrange the different races in a series, according to the degree of intersexuality in the different crosses, and, consequently, to assign numerical values to the factors F and

M in each race. These values were, of course, purely arbitrary, in that they did not represent actual values but only the relative values of the factors in the different races. Employing these values, the degree of intersexuality that would be produced by any given cross could be estimated. A simple example is provided by the crosses of the European and Japanese races already described. Let it be supposed that F = 80 and M = 60 in the European race and that F = 100 and M = 80 in the Japanese race. The cross between a European male and a Japanese female may be represented as follows :

	European Male.		Japanese Female.
Parents :	$(F_1)\ M_1\ M_1$ $80 : 60 + 60$		$(F_2)\ M_2$ $100 : 80$
Gametes :	M_1 60		$(F_2)\ M_2$ (F_2) $100 : 80$ 100
Offspring :	$(F_2)\ M_2\ M_1$ $100 : 80 + 60$	(normal male)	$(F_2)\ M_1$ $100 : 60$ (normal female)

It can be seen from this diagram that the offspring should be normal males and females. This is in accord with the observed results, as stated. The reciprocal cross can be expressed similarly.

	Japanese Male.		European Female.
Parents :	$(F_2)\ M_2\ M_2$ $100 : 80 + 80$		$(F_1)\ M_1$ $80 : 60$
Gametes :	M_2 80		$(F_1)\ M_1$ (F_1) $80 : 60$ 80
Offspring :	$(F_1)\ M_1\ M_2$ $80 : 60 + 80$	(normal male)	$(F_1)\ M_2$ $80 : 80$ (inter-sex)

It can be seen that the male offspring will be normal, but the female offspring will be intersexes, because there is no excess of either factor, since F exactly equals M. The theoretical result of this cross is, therefore, the same as that which has been observed. It can be seen that the results of any given cross can be predicted in a similar manner once the relative values of F and M in the two parent races are known. The numerical application of the formula provides a complete description of the facts observed.

The detailed examination of the structure of the intersexes themselves leads to another important conclusion. It might be expected that the male and female characters of intersexes would be completely blended so that every part of the body would exhibit a condition intermediate between that of the corresponding parts of a normal male and a normal female. This expectation is not fulfilled, for some parts of the body of intersexes are entirely male, others entirely female. The intersexes are, in fact, sex-mosaics of male and female parts. By comparison of a number of intersexes of different grades it is possible to arrange the various parts of the body in a definite series according to whether they exhibit intersexuality only in slightly, moderately, or strongly intersexual individuals. This seriation is then found to be exactly the opposite of the order of differentiation of the parts in the individual. The parts that develop and differentiate first are the last to be modified and only exhibit the characters of the opposite sex in strongly intersexual individuals. Similarly, the parts that differentiate last exhibit the characters of the opposite sex even in weak, as well as in moderate and strong intersexes. It can be deduced, therefore, that in intersexes the characters of one sex only differentiated up to a certain point in development, after which the characters of the other sex differentiated. The degree of intersexuality thus depends on the degree of development attained when this switch over from maleness to femaleness, or *vice versa*, occurs. The earlier the switch over occurs in development the more completely the characters of the opposite sex will be developed. This leads to the important conclusion that there is a time factor concerned in development. This time factor determines the point in development at which reversal of sexual differentiation takes place. The parts that differentiate in a female intersex before that point in development is reached are female in character, those that are differentiated after are male in character. The converse is the case in the development of male intersexes. The earlier this point occurs in development the more complete will be the reversal of sex. Thus, if it occurs before the differentiation of the gonads the resulting individual, with the chromosomal constitution of one sex, will develop all the sexual characters of the other sex.

Since the degree of intersexuality produced by any given cross depends on the quantitative action of the factors F and M, it is apparent that the time factor is an expression of their action. These considerations have led Goldschmidt to make the interesting suggestion that the action of the factors F and M follows the laws of mass action, and that the factors themselves may be of the nature of enzymes.

However, space does not permit of following these speculations further.

Goldschmidt's hypothesis provides a remarkably complete explanation of the occurrence of intersexes in racial crosses of Lymantria. Moreover, this explanation is capable of numerical expression. The hypothesis, divested of all but essentials, involves three fundamental concepts: 1. The Mendelian factors determining sex are quantitative. 2. Racial variations occur in them. 3. A time factor is involved in their expression during development.

There is good reason to believe that these three principles of Goldschmidt's hypothesis are applicable to vertebrates as well as to moths. There is, however, one important difference in the development of the sexual characters in Insects and Vertebrates respectively. The gonads and the other sexual characters in the former both develop and differentiate under the direct control of the same factors. The sex of the accessory organs and the secondary sexual characters is not determined indirectly through the influence of the gonads, but is entirely independent of them, being determined directly by the same stimulus which causes the gonads to become either ovaries or testes. This is shown by the fact that the removal of the gonads of insects at an early stage of development does not affect the subsequent development of the other sexual organs or secondary characters, which become typically male or female, as the case may be, in a perfectly normal manner. The mechanism of sexual differentiation in Vertebrates is totally different. The factors which determine the development of the gonads into either ovaries or testes do not directly influence the differentiation of the other sexual characters. The development of these into male or female structures is determined by hormones secreted by the ovaries or testes. The factors which determine the sex of the gonads in vertebrates thus only influence the development of the other sexual organs indirectly through the medium of the specific male or female hormones secreted by the differentiated testis or ovary respectively.

The most important evidence, derived from vertebrates, in favour of Goldschmidt's hypothesis is provided by the occurrence of juvenile hermaphroditism or sex-reversal in various races and species of frogs. Pflüger discovered in 1882 that the sex-ratio of young frogs (*R. temporaria*), recently metamorphosed, of certain European local races was abnormal. The sex-ratio of young frogs from the neighbourhood of Utrecht, whether captured wild or bred in captivity, showed a remarkable preponderance of females, while frogs of the same age from Königsberg had a normal sex-ratio of one male to one female.

The sex-ratio of adult frogs from both localities was normal, as can be seen from the following table.

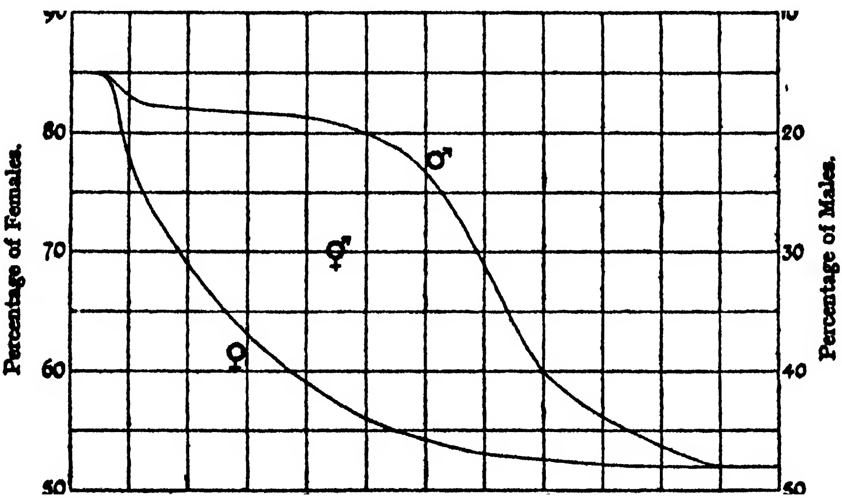
Local race.	Percentage of males.	
	Young frogs recently metamorphosed (6 months old).	Adults.
Utrecht	13	48·8
Königsberg	48·5	50
Bonn	36	49·4

The change in the sex-ratio of the Utrecht race was so great that it scarcely could be explained by assuming a differential mortality falling most heavily on the females. Pflüger therefore concluded that the excess of young females transformed into males, thus making the adult sex-ratio a normal one. Pflüger's discovery has been confirmed by R. Hertwig and his pupils, Schmitt-Marcel, Kuschakewitsch and Witschi, whose brilliant embryological and experimental investigations have added so much to our knowledge of the problems of sex-determination in vertebrates.

Schmitt-Marcel examined the sex-ratio of a local race from Dorfen at definite intervals from the time of metamorphosis until twenty-two months after. The first month showed a sex-ratio of 85 females to 15 males. Then the female percentage fell, hermaphrodites appeared, and the male percentage gradually began to rise. The largest number of hermaphrodites occurred at a year after metamorphosis, when the sex-ratio was 54 females : 24 hermaphrodites : 22 males. By twenty-two months after metamorphosis the female percentage had fallen to 52 and the male had risen to 48, no hermaphrodites being found. Evidently the transformation of the females into males was completed and the definitive sex-ratio was attained at this age. These results may be conveniently represented in diagrammatic form, as in Fig. 1. It is interesting to note that in this race the transformation of females into males takes place between the 2nd and 18th months after metamorphosis. In other races it may take place at a later stage of development.

The testes of frogs present, in fact, two different types of development: direct and indirect. Direct development of the males predominates in some geographical races, while in other races indirect development is the rule. The undifferentiated gonad in the former differentiates directly into either an ovary or a testis according to whether the animal is a female or a male. The gonads first differentiate into ovaries, in the latter type, irrespective of whether the animal is male or female, and later transform into testes in the males.

Witschi has described the development of the gonads of the frog. The gonad, before differentiating into an ovary or a testis, consists of a hollow ridge of peritoneal or germinal epithelium, in which the germ-cells are embedded. Five to seven strands of cells, the rete cords, grow down from the base of this genital ridge into the primitive central cavity. These rete cords are arranged in a series from the anterior to the posterior end of the genital ridge. The first stage in the direct differentiation of the indifferent gonad into a testis is marked by the more rapid growth and greater thickness of the rete



Months after Metamorphosis.

FIG. 1.

cords. The germ-cells and a number of epithelial cells then migrate from the germinal epithelium, across the primitive cavity of the gonad, to the rete cords, around which they become arranged in nests. These nests develop into the spermatic tubules, which acquire a lumen and open into the efferent tubules formed from the rete cords. Meanwhile, the germinal epithelium, deprived of its germ-cells, ceases proliferating and remains as a simple peritoneal investment around the developing testis. In the young ovary the rete cords grow more slowly and become hollow at their distal extremities. The germinal epithelium continues to thicken, chiefly owing to the multiplication of the contained germ-cells. After a time the germ-cells, or oogonia, become arranged in nests in the thickened epithelium. They enter then on the prophase of the reduction division, and, when this is completed, each becomes

surrounded by a layer of epithelial cells which form a follicle. Meanwhile the spaces in the rete cords increase in size, until each cord forms a hollow ovarian sac, the secondary cavities of the ovary. These ovarian sacs are thin-walled and completely fill the primitive cavity of the ovary. The gonads of males exhibiting indirect development definitely differentiate into ovaries first. The subsequent transformation of these ovaries into testes is marked by the migration of germ-cells from the base of the ovarian cortex to the walls of the ovarian sacs, where they form nests, which develop into the spermatic tubules. The ovarian cortex degenerates and undergoes involution, finally leaving only a thin peritoneal epithelium covering the gonad. The ovarian sacs also shrink and transform into the efferent ducts of the testis. During the transformation the basal and central parts of the gonad become testicular before the peripheral portion has lost its ovarian character. The actual stage of development at which this reversal of sex takes place varies widely in different races, and may even take place after the animal is adult.

It may be concluded that, in male frogs which exhibit the normal or direct form of development, the male-determining stimulus becomes effective while the gonads are still in the indifferent stage, before they have had time to differentiate into ovaries. The indirect form of development appears to depend on delay in the appearance of this stimulus, so that it does not become effective until after the gonads have differentiated, to a greater or lesser degree, into ovaries. It is probable, therefore, that a time factor is involved in the production of Pflügerian sex-reversal. Since the direct and indirect forms of male development are characteristic of different races it may be concluded that this time factor is the expression of hereditary factors which vary quantitatively in different races. These conclusions are identical with the major concepts involved in Goldschmidt's hypothesis, as applied to Lymantria.

There is, however, one important difference between Lymantria and Rana, for the female is heterogametic in the former and the male in the latter. The sex-chromosomal constitution of the frog has been demonstrated by Witschi by means of an adult hermaphrodite which produced both mature ova and spermatozoa. He was able to effect self-fertilisation of some of the eggs of this hermaphrodite and also to cross it with a normal male and a normal female by means of artificial fertilisation. (*a*) The eggs of the hermaphrodite fertilised by sperms from the normal male produced 135 males and 132 females. (*b*) The self-fertilised eggs produced 1 hermaphrodite and 45 females. (*c*) The eggs of the normal female fertilised by the sperms of the hermaphrodite produced 182 females

only. It is clear from these results that the hermaphrodite and the normal female were homogametic, or XX, and that the normal male was heterogametic, or XY. The observed results of the three crosses are almost identical with the theoretic expectations which demand that the first cross (*a*) should produce an equal number of males and females, and that the other two (*b*) and (*c*) should produce only females.

The sex-chromosome theory postulates that XX and XY individuals will be produced in approximately equal numbers. To meet this requirement with races of frogs which exhibit the indirect mode of development of the males, it is necessary to assume that those individuals which at first develop ovaries that then pass through an hermaphrodite stage and transform into testes are chromosomal males. We arrive, then, at the remarkable conclusion that juvenile or Pflügerian hermaphrodites are genetically male (XY), while adult hermaphrodites are genetically female (XX). Put in other words: the ovary of a frog, whether XX or XY in chromosomal constitution, may transform into a testis.

Since the male is the heterogametic sex in the frog the factor for femaleness (F) will be located in the X-chromosome, while the factor for maleness (M) will be located in the autosomes, or cytoplasm. The formula will be, therefore, FFMM ♀ and FMM ♂, where $F > M$ but $F < MM$. Witschi considers that the factor M is in the autosomes, and that the factor for femaleness is present in the Y-chromosome as well as in the X-chromosome, but less potent. He denotes the Y-borne factor by f. The formula then reads FFMM ♀ and FfMM male, where $F > M$ but $MM > Ff$. He has applied Goldschmidt's hypothesis, on the basis of this formula, to Hertwig's results of racial crosses of frogs. He divides the various races of frogs into five groups, according to the proportion of indirect development of males exhibited by each. Thus Group V contains those races in which the development of the male is almost or entirely direct, while the other groups exhibit increasing numbers of males which develop indirectly. Group I includes those races in which the development of the males is almost or entirely indirect, and the sex-ratio for the first month after metamorphoses is consequently over 90 per cent. females. Hertwig and Witschi made many crosses between the different races. It was shown by dividing the eggs of a single female into several parts and fertilising them with sperms from males of different races that different results could be obtained. For instance, a female of the Bonn race, which exhibits only a slight amount of indirect development (Group IV) was crossed with four different males. The results are summarised in the following table.

	Parents.		Offspring.
Bonn ♀ (Group IV) ×	Freiburg ♂ (Group I)	. .	366 ♀ : 14 ⚥ : 1 indifferent.
	Alsace ♂ (Group I)	. .	302 ♀ : 29 ⚥ : 2 ♂.
	Berlin ♂ (Group IV)	. .	235 ♀ : 16 ⚥ : 82 ♂.
	Davos ♂ (Group V)	. .	135 ♀ : 2 ⚥ : 139 ♂.

It will be seen that the offspring by the Group I males exhibited almost entirely indirect development of the males, whereas those of the Group V male exhibited practically exclusively direct development. The results of the cross with the Group IV males were intermediate in this respect between the Group I and Group V males. Conversely, the sperms of a single male were used to fertilise the eggs of several females of different races. The different results obtained showed that the females also influenced the sex of the offspring. Thus a Group V male mated with a Group V female gave only differentiated males and females, but mated with a Group III female it gave only males and hermaphrodites. Evidently in the latter case the male factors so outweighed the female that the chromosomal males differentiated directly while the chromosomal females underwent indirect development into males. It is clear from these crosses that both male and female parents participate in the transmission of the sex-determining factors to the offspring. Witschi was able to assign arbitrary numerical values to the factors in the different races and to predict from them the approximate results of any given cross. His formulæ differ somewhat from Goldschmidt's, but need not be given here, since it is sufficient that the principle is the same.

Environmental, as well as hereditary, factors appear to play a part in determining sex in frogs. Hertwig succeeded in modifying the sex-ratio in cultures by keeping them at high or low temperatures. The weak Irschenhauser race (Group I), which exhibits a high degree of indirect development of the males, produced 35 ♂ : 438 ♀ when cultured at 10° C., but produced nothing but females at 20° C. *R. esculenta* produced 250 ♂ : 85 ♀ at 15°–18° C., but produced 344 ♂ ; 319 ♀ at 30° C. Witschi cultured *R. sylvatica* at 32° ± 2° C., and obtained 53 females transforming into males and 62 normal males, but no normal females. This species normally exhibits direct development of the males and the control cultures gave a sex-ratio of 100 ♀ : 96 ♂. These results show that temperature plays a part in controlling the differentiation of sex in frogs.

Kuschakewitsch modified the sex-ratio by delaying fertilisation. Normal cultures gave approximately equal numbers of males and females, but a culture 89 hours over-ripe, when fertilised produced 300 males only. This experiment is quite

conclusive, since the mortality experienced was extremely small.

It must be admitted, on account of these experiments, that abnormal environmental conditions, of temperature and delayed fertilisation at least, may modify sex in frogs as determined by the purely genetical factors concerned.

Goldschmidt's hypothesis provides a remarkably complete explanation of the complicated problem of sex-determination in frogs. The observations of Hertwig and his pupils on the occurrence of Pflügerian hermaphroditism in the various races and racial crosses of frogs are extremely difficult to understand on any other hypothesis. The evidence which they provide in favour of Goldschmidt's hypothesis is almost as complete as that provided by Lymantria itself. Our knowledge of the mechanism of sex-determination in other vertebrates is not nearly so complete as that supplied by Rana, or by the Gipsy moth. Nevertheless, if Goldschmidt's hypothesis is applicable to one vertebrate it is reasonable to suppose that it may be applicable to all. The difficulty in applying it rests in the absence of sufficient genetical data, for in no vertebrate, other than the frog, have intersexes been produced at will by racial crosses. Critical examination of the developmental conditions involved in the production of various types of intersexuality and hermaphroditism, even in mammals, provides strong evidence that a time factor is involved. It is the purpose of this article to show that Goldschmidt's conception of a time factor in development, in the form of delayed appearance of the male-determining stimulus, will account for two of the best-known examples of hermaphroditism in vertebrates, namely, that exhibited by the male toad and by intersex pigs.

Toads possess a small rudimentary ovary, known as Bidder's organ, which is situated at the anterior end of each functional gonad, whether ovary or testis (Figs. 2 and 3). It is identical in structure in both males and females. It resembles an immature ovary and contains many small oocytes, which sometimes develop yolk. Bidder's organ can usually be distinguished from the functional ovary by its more compact form and golden-yellow to red colour. It persists throughout life in the male toads, but appears to gradually atrophy or merge with the functional ovary in adult females of some species. Its position at the anterior end of the gonad, to which it is closely attached, is constant, but it varies considerably in size. Harms, and also Ponse, have shown that the operative removal of the gonads, from either a male or a female toad, causes the Bidder's organs to hypertrophy and ultimately become functional ovaries. Bidder's organ, therefore, must be considered to be a true ovary in a state of arrested development.

Ponse has described the development of the gonads of the toad. The indifferent genital ridge at an early stage of development becomes divided into three segments: an anterior progonad, an intermediate mesogonad, and a posterior metagonad. These three segments develop separately both spatially and chronologically. The progonad differentiates very early into an incipient atypic ovary. This is the larval Bidder's organ, which soon degenerates almost completely. The mesogonad starts to develop at the time of metamorphosis and also forms an incipient ovary. It forms the major part of the definitive

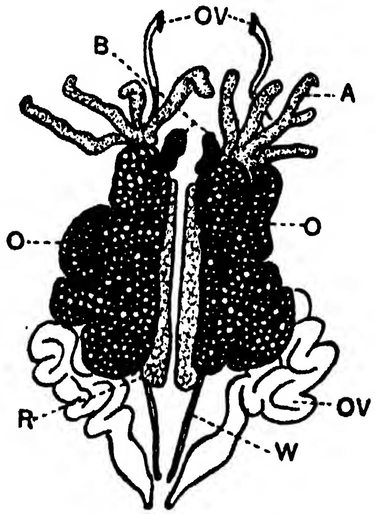


FIG. 2.

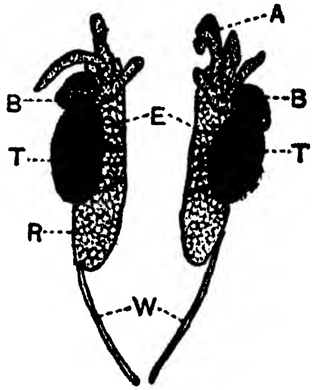


FIG. 3.

Reproductive organs of adult toads, showing Bidder's organ attached to the cephalic ends of the gonads. FIG. 2, female. FIG. 3, male. A, fat-body. B, Bidder's organ. E, vasa efferentia. O, ovary. OV, oviduct. R, kidney, T, testis. W, ureter.

Bidder's organ. The metagonad starts to develop after metamorphosis. It forms the definitive gonad and differentiates into an ovary or a testis as the case may be. It is clear that the early development of the gonads in both male and female toads is controlled by female-determining influences. These maintain their dominance in the female until sexual differentiation is completed. They control development in the male until metamorphosis is completed and first the progonad and then the mesogonad have differentiated into ovarian tissue. Then a switch over occurs and male-determining factors become dominant. The metagonad, which is still in the indifferent condition at the time of the switch over, then develops into a testis. This conclusion is substantiated by the occasional

occurrence of hermaphrodite toads which have a true ovary and testis as well as Bidder's organ. Several such cases have been described, and in all the ovarian tissue is situated between the Bidder's organ and the testis. The development of these can be explained on the assumption that the male-determining stimulus did not appear until a later stage of development than in the normal male. Differentiation begins at the anterior ends of the gonads and gradually extends in a posterior direction in all vertebrates, including the toad. The abnormally delayed appearance of the male-determining stimulus would result, therefore, in the anterior end of the metagonad, as well as the pro- and meso-gonad, differentiating into ovarian tissue. The delayed male-determining stimulus would be only in time to direct the differentiation of the more posterior region of the metagonad into testis. The resulting animal would have ovarian tissue interpolated between the Bidder's organ and the testis. This explanation of the development of normal male and hermaphrodite toads is similar to that of Pflügerian hermaphrodites among frogs. There is one difference between the frog and the toad in this respect. The male-determining stimulus in the frog not only directs the development of the undifferentiated gonad, but causes ovarian tissue, which has already differentiated, to transform into testis, while in the toad it appears to have no influence on ovarian tissue which has already differentiated. It is obvious that other hermaphrodites in which the ovary is anterior to the testis can be explained in a similar manner. Such a well-marked antero-posterior arrangement of the ovarian and testicular tissue could be expected to occur only in forms in which sexual differentiation proceeded sufficiently slowly for the anterior region of the germinal ridge to be distinctly more developed than the posterior region. This condition is not fulfilled in the mammals, in which the germinal ridge is shortened and differentiation proceeds so rapidly that the anterior end is little in advance of the posterior. Nevertheless, detailed examination of the structure of ovotestes of pigs indicates that the same explanation is applicable.

Intersex pigs, known to breeders as Wilgils, are of comparatively frequent occurrence, especially in certain strains. The sexual organs exhibit various grades of intersexuality and the animals are invariably sterile. They may be divided into two groups according to whether both ovarian and testicular elements are present or only testicular. The former group are true hermaphrodites, the latter are intersexes with abnormal maldescended testes, but no ovarian tissue. The hermaphrodites may have an ovary on one side and a testis on the other, or an ovotestis on one side accompanied by an ovary, a testis

or an ovotestis on the other side. One example has recorded with a separate ovary and testis on both sides. The ovotestes of pigs are remarkably constant in structure. The ovarian tissue in them always occurs as a cap on the ventro-antero-median surface of the testis (Fig. 4). This is its actual position, not necessarily its true morphological position. Since the unequal growth and rotation of the gonads during development render it extremely difficult to identify the part of the

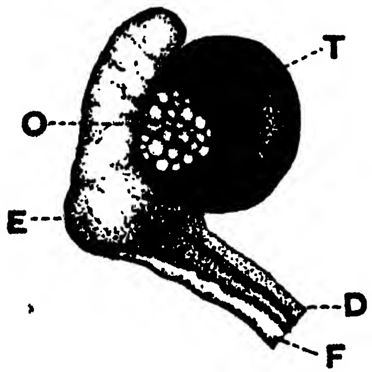


FIG. 4.

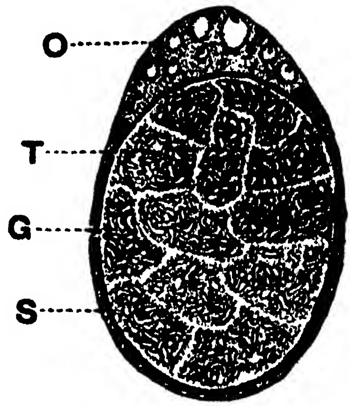


FIG. 5.

FIG. 4. Diagram of ovotestis and epididimis of a pig, showing the position of the ovarian tissue. FIG. 5. Diagrammatic section of an ovotestis, showing the position of the ovarian tissue and testicular cortex outside the tunica albuginea. D, vas deferens. E, epididimis. F, Fallopian tube. G, tunica albuginea. O, ovary. S, testicular cortex. T, testis.

embryonic gonads from which it originated, it is virtually impossible to ascertain its true morphological relations. The statement of some authors that the ovarian tissue occurs at the anterior ends of the ovotestes, therefore, is misleading. The ovarian tissue varies in size from a very minute patch to a mass almost as large as the testicular tissue. It is significant that the ovarian tissue invariably lies outside the tunica albuginea of the testis, to which it is closely attached (Fig. 5).

A large number of ovotestes of pigs have been described by Pick, Baker, Krediet, and a number of other authors. It can be seen from these descriptions that they all conform to a single type. The following detailed description of an example in the author's possession, therefore, may be taken as typical of pig ovotestes in general.

The ovotestis of this pig was situated on the right side. The major part of it was testis, which consisted of sterile spermatic tubules and large quantities of interstitial cells. It

was in fact typical of maldescended testes and was surrounded by a thick fibrous tunica albuginea. Outside the tunica albuginea, and completely surrounding the testis, except at the hilum, was a cortical zone of tissue (Fig. 5). The greater part of this cortex was composed of spermatic tissue, similar to that within the tunica albuginea, with sterile spermatic tubules and masses of interstitial cells. The spermatic tubules in this cortex opened by distinct pores on the surface of the gonad, apparently into the peritoneal cavity. The spermatic portion of the cortex was surrounded by a very thin secondary tunica albuginea, which was perforated by the peritoneal openings of the spermatic tubules. A small patch of the cortical zone at one pole of the gonad consisted of typical ovarian tissue. It contained numerous small oocytes, in primordial follicles, scattered in the stroma. The ovarian tissue was covered on the outside by a cubical epithelium, similar to the peritoneal investment of a normal ovary, but this was absent from the spermatic portions of the cortex. Around the circumference of the patch the ovarian tissue gradually merged with the spermatic tissue of the cortex, thus forming a zone which was intermediate in character between the two. This intermediate zone contained spermatic tubules in which a number of oocytes occurred, normal primordial follicles and oocytes in follicles which were modified to resemble spermatic tubules. The tissue between these was intermediate in character between the dense ovarian stroma and the loose inter-tubular tissue of the testis, and it contained a few interstitial cells. The left gonad was similar in structure except that there was no ovarian tissue in it.

The structure of this ovotestis can be interpreted from the developmental point of view. The embryonic mammalian gonad, at the time when sexual differentiation normally takes place, is ovate in form, the germinal ridge having become shortened and its region of attachment to the mesonephros constricted. It consists of an epithelial mass of medullary cords, containing primordial germ-cells, in the centre. The medullary cords are surrounded by a thin layer of connective tissue, the primitive tunica albuginea, which separates them from the germinal epithelium. At this stage the germinal epithelium is quiescent and is not proliferating. The differentiation of the gonads into ovaries or testes then begins. The testes differentiate positively before the ovaries, so that, for a brief period after the testes have differentiated, the ovaries are still in the indifferent condition and can be distinguished only by comparison with testes of the same stage of development. The medullary cords in the testes develop into the spermatic tubules, the primitive tunica albuginea thickens and persists as the definitive tunica of the testis, and the germinal

epithelium becomes so thin that it is scarcely possible to distinguish it. The development of the ovaries is essentially different. The germinal epithelium covering them thickens and soon exhibits a renewed activity which results in the proliferation of the cortical cords. The growth of the cortical cords breaks up the primitive tunica albuginea and gradually compresses the medullary cords, which degenerate partially, into the centre of the gonad. This proliferation of the cortical cords is followed, at least in some mammals, by a third proliferation of epithelial cords, which takes place shortly before puberty. A thin secondary tunica albuginea is formed beneath the germinal epithelium, as soon as the cortical proliferation is completed, and persists as the tunica albuginea of the ovary.

It may be assumed from these data that the male-determining stimulus normally becomes operative in male embryos almost immediately after the completion of the medullary proliferation. In the normal female it does not become operative at all during the period when the gonads are in process of differentiation into ovaries. It is immaterial whether there is a definite female-determining stimulus as distinct from the male-determining stimulus, or whether female differentiation takes place in the absence of an inhibitory action exerted by the male-determining stimulus. It is clear that on the basis of these assumptions delay in the normal time of appearance of the male-determining stimulus would result in the gonad beginning to differentiate in the female direction. The germinal epithelium would begin to proliferate cortical cords. These would form a peripheral zone around the gonad, outside the primitive tunica albuginea, as in the very young ovary. This zone would be better developed anteriorly and ventrally since this region is always slightly in advance of the posterior and lateral regions of the gonad. A stage would then be reached when the cortical zone at the antero-ventral end of the gonad would have differentiated definitely into ovarian tissue, while the rest of the zone would be still in the indifferent condition of epithelial cords. If the delayed male stimulus became operative at this stage it would direct the development of the medullary cords into spermatic tubules and cause the primitive tunica albuginea to thicken and persist. Presumably it would not affect the already differentiated ovarian tissue, but would transform the undifferentiated cortical cords into spermatic tubules. The resulting gonad would exhibit, in fact, the exact structure of the typical ovotestis of the pig which has been described. The testis proper of such a gonad would represent the medullary cords, and the thick tunica albuginea surrounding it would be the persistent primitive tunica. The cortical zone would represent the incipient formation of cortical

cords. The portion of this zone in the neighbourhood of the ovarian tissue which is intermediate in character between the ovarian and spermatic tissues could be explained as originating from cortical cords which were beginning to differentiate into ovarian tissue at the time when the male stimulus appeared. The spermatic tubules of the testicular portion of the cortical zone would correspond to the cortical cords and their communications with the peritoneal surface of the gonad might represent their original connection with the germinal epithelium from which they were being proliferated at the time of the appearance of the male stimulus. Finally the constant position of the ovarian tissue at one pole of the ovotestis would follow from the fact that the antero-ventral surface of the embryonic gonad normally exhibits a stage of development in advance of the remainder.

It is interesting to note that a stage resembling this hypothetical condition of the embryonic ovotestis of the pig, at the time of appearance of the delayed male stimulus, actually occurs in the normal development of the testis of the mouse. Immediately after the testis has differentiated in the mouse the germinal epithelium proliferates a thin zone of tissue outside the tunica albuginea. This zone of tissue is homologous to the cortical proliferation of the ovary, but it is a transitory and functionless structure. It soon stops developing and gradually disappears without differentiating further. Humphrey observed a similar condition in the embryonic testes of Amblystoma. He found that a transient ovarian cortex was formed outside the testis in many cases. It was usually limited to the ventral surface, but sometimes almost surrounded the testis. These observed conditions of the embryonic testes of mice and Amblystoma only differ from the hypothetical condition required to account for the development of pig ovotestes, in that the cortical tissue degenerates in them while it persists in the pig.

It is clear that the development of the ovotestes of pigs can be accounted for by the assumption of abnormally late appearance of the male-determining stimulus. A condition like that of the left gonad of the pig described or of the testes of intersexual pigs in which the tunica albuginea is surrounded by a cortical zone of spermatic tissue, without any trace of ovarian tissue, obviously belongs to the same category. Presumably the male stimulus in them appeared slightly earlier, before any portion of the cortex had differentiated into an ovary, and so entirely transformed the incipient cortex into testicular tissue. The hypothesis, therefore, brings into line the various grades of intersexuality and hermaphroditism in pigs and allows of their being arranged in a series representing successive degrees

of delay in the appearance of the male-determining stimulus during development. The extreme of such a series is provided by the hermaphrodite pig in which the ovarian and testicular portions of both gonads had become separate so that it possessed two ovaries and two testes.

This explanation of the development of the ovotestes of pigs simply involves the assumption of a time factor, similar to that contained in Goldschmidt's hypothesis. Crew suggested this, but his application of the hypothesis led him to conclude that intersexes, with only spermatic tissue, and true hermaphrodites were different, in that the former were due to an abnormality in the time of differentiation of the gonads and the latter to an abnormality in the mode of differentiation of the gonads. The detailed histology of the hermaphrodite pig described does not support this interpretation and shows that it is only necessary to assume abnormality in the time of appearance of the male-determining stimulus to account for both true and pseudo-hermaphrodites. The genetical data of hermaphrodite pigs is insufficient, however, to admit of further application of the hypothesis. It is only known at present that intersexual and hermaphrodite pigs occur comparatively frequently in some strains while they are extremely rare in others.

It is a remarkable fact that in some pigs an ovary, a testis, or an ovotestis on one side may be accompanied by any one of these on the other side. This unilateral arrangement is not explained by the hypothesis advanced, since it is difficult to assume any very pronounced difference in the stage of development of the two gonads at any given time. Several authors have attempted to show that one side or the other has, in this respect, a greater tendency towards one sex, but the number of examples of hermaphrodite pigs, in which the sex of the two gonads differs, is insufficient to provide a significant result. It must be admitted that this problem is insoluble at present.

The following should be consulted for references to the literature referred to in this paper and for further information: Brambell, *The Development of Sex in Vertebrates* (Sidgwick & Jackson, London, 1930, in press). Crew, *The Genetics of Sexuality in Animals* (Cambridge University Press, 1927). Goldschmidt, *The Mechanism and Physiology of Sex Determination* (Methuen & Co., London, 1923).

POPULAR SCIENCE

THE OYSTER-CATCHER

BY THE REV. EDWARD A. ARMSTRONG

IF I had to describe the oyster-catcher to someone not very familiar with birds I would say, Look out for a tail-less magpie with a long red nose, as if its potations had been deep rather than discreet. Putting it more politely, the oyster-catcher is a good-sized black-and-white bird with a red or orange-red bill, red eye, and flesh-pink legs to be seen round our coasts at all seasons. He is rather a swaggering fellow with a Micawberish way with him and his eccentric tastes are in keeping with his eccentric garb. You never know what you may find an oyster-catcher up to.

Until recently those who wished to be facetious were wont to say that the oyster-catcher was thus named because he does not eat oysters, but Dr. J. M. Dewar has now shown that the bird can and does eat these dainties. In America this habit has been frequently observed, but here he seldom gets the opportunity to indulge such epicurean tastes. So far as this country is concerned either of the other two popular names, "mussel-picker" or "sea-pie," would be more descriptive, the one referring to the food, the other to the plumage of the bird.

Many are the artifices of the oyster-catcher in securing his prey. He will prowl about the mussel-banks prising open the shells by the dorsal or ventral surface according to whichever method is most convenient, or probe in the sand or mud for the unfortunate mollusc. Even first-year birds, before their bills are fully developed, are able to open mussels. Occasionally, oyster-catchers have been seen to fly up with the shells and drop them in order to devour the contents at their leisure.

Prof. Newton, quoting Mr. Harting, says that the bird deals with limpets by laying its head sideways on the ground, grasping the limpet's shell close to the rock between its mandibles, using these as scissors to cut off the creature. Another description is that given by Robert Gray in *Birds of the West of Scotland*. "I have many times studied the habits of this shy bird, when

lying concealed on the shore waiting a shot at passing wild ducks or other birds I was in quest of. I recollect seeing about thirty in a flock pitch upon a shelving rock from which the waves had just receded, and commence an attack upon the limpets, which were very numerous. Being within three or four yards of them, I could distinctly perceive their movements, and could not help being struck with their dexterity in overturning the shells and scooping out their contents. Sometimes a bird would run forward to a limpet and bend down its head sideways, as if in a listening attitude, then it passed on to another and another, repeating the scrutiny apparently to see if the shell was at all raised from the rock, until it found one ready for treatment, which it immediately put in force by thrusting its thin-pointed bill suddenly between the edge of the limpet and its point of attachment, and turning it neatly over; one foot was then placed on the object, and the animal taken out as cleanly as if done with a knife or other sharp instrument."

In Holland this bird has become addicted to preying on other birds' eggs, sucking them like a crow. This is but one of its many eccentricities.

Individual oyster-catchers vary immensely in habits and disposition, as every bird-photographer knows. Some are extremely wary, while others are confiding and comparatively tame. Mr. Richard Kearton relates how, after photographing a pair without difficulty, he thought to have some fun out of a friend who had spent hours fruitlessly with another pair by sending him a photograph of these birds. The eggs were chipping, the time when birds are boldest at the nest, yet, although he tried long and hard, he failed to get a picture and had to confess that the laugh was not with him.

The nest, which usually contains three eggs, is normally near the shore on shingle or turf, sometimes on sand-hills or among rocks; but individual birds may do the most strange things. Nests have been found on a railway track and in a wood, on a high post, in the stump of a tree, and on the ridge-pole of a house. I have even photographed the bird at its nest on a wall!

We had arrived on the island of Texel, off the Dutch coast, and having been washed out of our little tent, a worthy farmer took compassion on us and allowed us to camp in his barn. The winds blew and the rain rained and we were unable to go far afield in search of birds. It was then that we were shown the oyster-catcher's nest. The bird had laid its eggs in a scrape on top of the "tuinwal"—a kind of turf wall, found, we were told, nowhere else in Holland. Instead of being rather difficult to see, as when it nests on the shingle, this extraordinary bird could be spotted from a distance of a quarter of a mile. From the door of our barn we could see it on the nest. We prepared

THE OYSTER-CATCHER.

to photograph it. My camera was set up on the wall a few feet away, disguised as a clod of earth (in other words, looking like nothing on earth) with a long thread attached to the shutter. I went off and watched until the bird returned. Then I crept up laboriously to the wall, hugging the ground close to its base and having to cross a ditch on the way. (When, during a lecture, I described this incident, how I had to jettison binoculars, shoes and stockings, and so on, to negotiate the ditch, a certain Professor who was in the Chair remarked what a useful euphemism "binoculars" was for nether garments!) The photograph was hopelessly fogged, as, indeed, were many of my Texel pictures, owing to faulty slides. But those who are hard up have to manage with what apparatus they can get. My whole outfit cost £2 second-hand—and, like the boy's second-hand trousers at 1*s*. 6*d*., "proved a disappointment."

The next attempt was made from a hiding tent beside the wall, but the material flapped so much in the high wind that the bird would not come back. It was then that I had an idea. In our barn was a colossal basket, a "hooi mand" used for carrying hay. We took it out, put the hiding tent over it, and watched. Very soon the oyster-catcher returned to her eggs. Then I got into the basket. I was very cramped, and my attention was much distracted by roving spiders whose one aim seemed to be to creep down my neck; but I photographed that bird. Having gained an inch we took a span, and tried the experiment of hoisting the basket on the wall and sitting inside with our legs hanging down outside covered with a ground-sheet. The oyster-catcher by this time was apparently reconciled to anything, and not only let me take photographs, but even when I whistled and talked and kicked my legs she only craned her black neck and gazed with mild surprise in her ruddy eyes. Sitting astride the wall, I watched her turning her eggs—edging them over with the point of her orange bill.

At a pool on the same island I once observed two oyster-catchers playing what looked like a game of hide-and-seek about a patch of turf which stood up from the surrounding mud. One would walk round after the other, which, when it had put the turf between it and its friend, would loiter casually about. Eventually they walked down to the water's edge and marched past, one with head bent low and altogether subdued appearance, the other about five feet in the rear with the most grandiloquent drum-major's step and bearing.

Mr. Kearton observed something very similar on this island. His account runs: "For two or three days in succession my daughter and I watched three birds going through the most curious antics at the same time in the afternoon and at precisely the same spot on each occasion. They marched in

solemn procession for a hundred yards or more, the two foremost members of the trio with heads bowed low and in silence, whilst the third brought up the rear with head erect and talking loudly and volubly all the time. As soon as a given spot had been reached the procession broke up in something akin to the hilarious din of a tavern joke." [1]

This remarkable behaviour would be very difficult to understand but that something analogous is to be observed in some other species. Compare the above with the description given by W. H. Hudson in *The Naturalist in La Plata* of the spur-winged lapwing: "If a person watches any two birds for some time—for they live in pairs—he will see another lapwing, one of a neighbouring couple, rise up and fly to them, leaving his own mate to guard their chosen ground; and instead of resenting this visit as an unwarranted intrusion on their domain, as they would certainly resent the approach of almost any other bird, they welcome it with notes and signs of pleasure. Advancing to the visitor, they place themselves behind it; then all three keeping step begin a rapid march, uttering resonant drumming notes in time with their movements, the notes of the pair behind being emitted in a stream, like a drum-roll, while the leader utters loud single notes at regular intervals. The march ceases, the leader elevates his wings, and stands erect and motionless, still uttering loud notes; while the other two, with puffed-out plumage, and standing exactly abreast, stoop forward and downward until the tips of their beaks touch the ground, and, sinking their rhythmical voices to a murmur, remain for some time in this position. The performance is then over, and the visitor goes back to his own ground and mate, to receive a visitor himself later on."

Mr. Hudson regards this behaviour as a manifestation of what he calls "a universal joyous instinct," but this does not go far towards explaining it. To account for anything by "instinct" is often nothing more than a confession of ignorance, and moreover, psychologists find no reason for postulating any such "joyous instinct." No, we must seek elsewhere for the elucidation of the mystery.

Mr. Selous, in *Bird Watching*, puts forward a much more plausible theory. He points out that there is the same sort of resemblance between the performance of the spur-winged lapwing and that of the oyster-catcher as there is between the perfect comb of the hive-bee and the crude comb of the humble-bee. The observer may see, for instance, two (oyster-catchers) standing side by side with their heads bent forwards and downwards, as the two lapwings bend theirs, though here the length of the brilliant, orange-red bills, the tips of which, also, almost

[1] *Wonders of Wild Nature*, p. 94.

touch the ground, make the angle of inclination a lesser one. In this attitude they both of them utter a long, continuous, piping note, of a very powerful and penetrative quality, sometimes swaying their heads from side to side as though in ecstasy at their own performance, and seeming to listen intently in a manner suggestive of a musical connoisseur. The third bird, who is obviously the female, either stands or walks at a short distance from the two pipers, who will frequently follow and press upon her, and then, though the march is not quite so formal and regular, it yet bears for a few moments a considerable resemblance to that of the spur-winged lapwing as described and figured in Mr. Hudson's work.

Mr. Selous further records that sometimes one of the pipers will fly at the other. When they begin to pipe vigorously their interest, he considers, becomes abstracted into this. Such he calls the "formalisation of actions once purposive." He thinks that antics at first performed with an object, for instance, courting displays, at times become eventually the object itself—separated from their original meaning and purpose. Social ceremonial displays have been evolved, he believes, from sexual displays. This explanation seems to be entirely satisfactory. It is to be noted that this performance of the oyster-catcher has been observed out of the breeding season; as late, indeed, as September. Prof. J. S. Huxley's conclusion, after studying the birds, is in general agreement. He suggests that the antics, piping, trilling, and dancing, "can play a number of different rôles in the bird's emotional life."[1]

Our friend the Texel farmer told us that the oyster-catcher is a bigamist. How far this is true I do not know, but these birds certainly seem rather casual in domestic affairs. At Blakeney two birds have laid in the same nest for several successive years. It would seem that oyster-catchers like to be odd in everything they do.

It is a curious fact that a bird so different from the oyster-catcher as is the black ani, or cuckoo of the West Indies, should also go through a performance bearing some resemblance to that of our own sea-pie, but that several females should also be reputed to lay in the same nest. Mr. Nicholl, in *Three Voyages of a Naturalist*, writes that on Grand Cayman "several individuals are usually seen together, and they roam over the pasture land in follow-my-leader style, uttering a monotonous bubbling cry."[2]

It is well known that the oyster-catcher can swim. A flock has been seen to alight on the water far from land apparently to feed on herring fry,[3] and Saxby mentions seeing a bird

[1] *Op. cit.*, p. 90. [2] *Op. cit.*, p. 148.
[3] Gray: *Birds of the West of Scotland*, p. 269.

jump into a pool from a rock and swim about looking for food.[1] It is, perhaps, not so generally known that this adaptable bird is, when occasion demands, an expert diver. R. L. Patterson, in *Birds, Fishes, and Cetacea of Belfast Lough*, gives an interesting account of an oyster-catcher's aquatic prowess. His account is as follows: "On the birds coming up over the ebb, I shot an oyster-catcher out of a small flock. It was only winged; and, strange to say, it dived when we went to catch it. It remained a long time under, and moved a rather considerable distance from the spot where it went down. After a little, on our second approach, it dived again close to us; and, as the water was calm, shallow, and clear, we could see it all the time it was below. Its principal effort this time seemed to be to remain under water; and it certainly did not use its wings to aid its progress through the water. For fully a minute an occasional easy back-stroke of the paddles was sufficient to keep us almost above it; till, finally, when it could stay under no longer, it slowly rose to the surface, and on emerging I caught it by the head and put an end to its sufferings."[2] Wilson, in his *American Ornithology*, also mentions the oyster-catcher's diving powers.

In the Hebrides the oyster-catcher is known as S. Bride's bird. There is a curious legend that the Saviour was once pursued by his enemies along the sea-shore and that these birds came to His aid and concealed Him with sea-weed until the danger was past.

I think it will be apparent from what has been already said that what we might call, for want of a better word, the psychology of a bird can be of the greatest interest. The adaptable, eccentric oyster-catcher alone presents a whole series of problems. The study of bird-psychology (if I may be permitted to use such a barbarous expression) has a wider importance, as may even throw light on our own mental make-up. Certain instincts are common to man and his lowly cousins and may be studied in a purer form uncomplicated by thought and reflection in some of the animals. The study of a bird may help us to plumb some of the mysteries of our own being. Only patient observation of the living creature can serve this purpose.

In the past, naturalists have been too often content with studying the dead bird and the old biology might have been more appropriately called Thanatology—the study of the dead and not the living. We have a natural bias to "murder to dissect" rather than to "come forth" and bring with us "a heart that watches and receives." I am inclined to believe that Monsieur Bergson is right when he declares that the human intellect has an innate incapacity for grasping Life. We are

[1] Saxby: *Birds of Shetland*, p. 172.
[2] *Op. cit.*, p. 173.

like children who snatch at soap-bubbles, destroying the attractive object whenever we touch it. Man has often made the attempt to study life, but has nearly always been fobbed off with something else. Too often this has been the case with ornithology; we start off to study a bird—and we study a tissue. It is a pity that we cannot grow out of that ancient superstition of regarding a dead thing as essentially similar to a living thing. We have not yet learnt the lesson of the *Phædo*:

"But how shall we bury you?" said Crito to Socrates.

"As you please," he answered, "only you must catch me first, and not let me escape you." And then he looked at us with a smile and said, "My friends, I cannot convince Crito that I am the Socrates who has been conversing with you, and arranging his arguments in order. He thinks that I am the body which he will presently see a corpse, and he asks how he is to bury me."

So, in catching flesh and feathers for the dissecting tray we let the bird escape us.

I am far from suggesting that the ornithologist should bury the scalpel and abandon the laboratory, but I do advocate with all the strength I can that he should regard the notebook, binoculars, and camera as at least as important and perhaps more so. The trouble is that people are as much deterred by its apparent ease as by its real difficulty from the study of bird behaviour; for the study of a creature as it goes about its daily round in full enjoyment of freedom *is* a difficult matter. How few are the naturalists who have devoted themselves to it—they could almost be counted on the fingers of one hand! Happily there is now an increasing band of such workers, accurate observers and painstaking field-naturalists.

It is to be feared that it is due to the influence of what I may be allowed to call the old biology that any yokel with a gun thinks that he is doing a service to science if he kills any strange bird he may come across. The best modern science, no longer thinking bones more important than behaviour, is out to fight this idea and to protect the goose that lays the golden eggs—the bird, in other words, which reveals secrets to those who, like Gilbert White, care to study its "life and conversation." It is more blessed to preserve than to destroy, and a bird in the bush is worth two in the hand. If there were a few passenger pigeons in the bushes and Labrador ducks on the high seas we might study both structure and behaviour instead of being able, in the case of these birds, to study structure only inadequately and behaviour not at all.

NOTES

A Critical and Historical Study of the Pectic Substances of Plants.
(A. G. Norman, B.Sc., Ph.D.)

The pectic substances have received but scanty treatment by authors of modern textbooks on plant biochemistry, the recent literature on the subject being confined practically solely to original papers. A well-informed review of the subject is therefore to be welcomed.

Much of the earlier work in this field appears on the surface to be confused and contradictory, but this is mainly due to the complete absence of any uniformity in nomenclature and to the lack of sufficient knowledge of carbohydrate chemistry at that time. In her historical survey, which is made in a most detailed and complete manner, the author[1] interprets and reconciles the many differences of nomenclature in the earlier researches, thereby bringing out their quality and greatly enhancing their value. In one respect, however, the nomenclature adopted by the author is at variance with that usually adopted. The American Chemical Society in 1925 issued a report on this question, with well-reasoned arguments for the adoption of certain terms. For the water-insoluble modification, present in association with cellulose in plant tissues, giving rise to the soluble form on hydrolysis or ripening and senescence, they proposed the use of the term "protopectin." The author in this and in her own work employs "pectose" in the same sense. This is to be regretted since it involves the use of a suffix reserved for the simple sugars. "Protopectin," implying as it does the precursor of pectin, is surely more correct.

A more serious criticism, however, is that this publication is by no means up to date, and from internal evidence should have appeared in 1926 instead of at the close of 1929. A casual examination of the very lengthy bibliography, added as an Appendix, revealed only five references to papers pub-

[1] M. H. Branfoot (M. H. Carré). [Pp. x + 154. Plates 10.] Department of Scientific and Industrial Research. Food Investigation Board. Special Report No. 33. (London: His Majesty's Stationery 1929. Price 3*s*. 6*d*. net.)

lished after 1925, and only one after 1926. This is distinctly unfortunate in an authoritative publication of this type, and must detract considerably from its value to persons commencing research in this and in allied fields. The very recent work of F. Ehrlich and his school, to mention only one example, is of considerable importance, most certainly meriting attention, since much of it involves modification of certain statements as to the nature of pectin in the plant.

Considerable space is devoted to the detailed work of the author and her associates for the Food Investigation Board on the ripening and senescence of stored fruits. The changes in quantity and distribution of the individual pectic substances have been followed under various conditions, so that it is possible to form a complete picture of this aspect of their metabolism. The histological details which are given should be of considerable value to botanists, to whom the presence of pectin in tissues has often been a difficulty owing to its general staining reactions. By differential methods of extraction, it is, however, possible accurately to localise these substances.

The biological decomposition of pectin is discussed with particular reference to the retting of flax, and the bacterial rots of fruits and roots. This is a field of work which would repay further investigation, since our knowledge of it is but patchy.

The final chapter contains a good review of the work on fruit jellies and the jellying power of pectin. The various factors involved in jellying, the quantities of acid, sugar, pectin, etc., might with advantage be controlled more in practice in the process of jam making, so that the time of boiling might be made as short as possible, and the product of a uniform high grade.

To sum up, this publication should be of great value as a source of reference to the historical aspect of this subject, since the author has handled and sifted the material with no little ability. It is, however, uneven in that the more recent work is not dealt with so fully, nor stated so clearly. One is left rather in doubt at times as to what is the present situation, and at others, annoyed by statements of fact that are not too generally accepted.

Letters from Rome: Two Reviews Reviewed (R. R.)

A short notice of this book is given on page 721. Several descriptive reviews of it have already been published in the lay press of South Africa and elsewhere, but recently two critical reviews have appeared, which I wish to review in

their turn without giving the names of the writers. One review was published in the *Tropical Diseases Bulletin* of November 1929, and the other one in *The Telegraph*, Amsterdam, November 16, 1929. Both these reviews appear to be by medical men, although I am not sure.

Probably no modern scientific discovery has been so falsified either from ignorance or by deliberate intention than the fundamental discovery regarding malaria and mosquitoes, which is of such importance throughout the tropics. Among these falsifications possibly the ingenious inventions of the late Professor G. B. Grassi of Rome rank as the most brilliant. He commenced his "work" on July 15, 1898, as he himself said, just as I was compelled to end my own labours; and having heard of my results, regarding which Manson and I had already published a series of illustrated articles, he determined to make the same discovery for himself "indipendentemente da Ross." Needless to say he succeeded in a wonderful manner, as his own writings testify. His first "studies" were certainly not brilliant, and were indeed based upon a spurious method which no tyro in genuine science would ever have attempted. These "studies" led to spurious results in two cases out of three, and to the ridiculous claim which he subsequently advanced. No one acquainted either with genuine science or with the subject of malaria would ever have accepted such rubbish, but Professor G. B. G. knew his public of alleged experts and of text-book compilers, whom he gulled completely. While he was making his first bogus adventure, but before he had attained any results at all, Dr. Charles wrote me the letters (transcribed with Charles's consent) which I have given in full. My object in reprinting them was to have copies for the instruction of those who do not understand the tricks which may be performed in similar cases, and because the original copies were exhausted long ago.

The duty of medical men is to treat the sick and injured, and they are not compelled in any way to undertake original research even into the causes or treatment of diseases; but of course they may do so if they like, and if they possess sufficient knowledge and leisure for the purpose. The result is that they do not always understand the niceties of questions of priority. Grassi understood medical men completely, and played upon this ignorance of theirs. For instance, there is a point which he frequently dwelt upon, but which is so absurd that I did not even mention it in my book. It is an obvious scientific axiom that the credit for ascertaining the life-history of any group of organisms belongs to the man who ascertains the life-history of any one species of that group.

Clearly the man who ascertains that another species of the same group has the same life-history, and does so by the same methods, has little claim to original discovery. Grassi always endeavoured to ignore this axiom, and to suggest that the discovery of the life-history of the human parasites was something quite distinct from my earlier description of the life-history of a malaria parasite of birds in mosquitoes. Of course it was this event, and my published description of my methods and technique, which enabled Grassi and his colleagues to obtain most of their results ; and the letters of Charles demonstrate conclusively that these gentlemen were fully acquainted with my work when they began their own investigations in Rome in 1898. Surrounded with books and with plenty of material close at hand, they were able to follow the matter without the numerous hindrances which hampered me and which ultimately forced me to give up the work for more than a year just as I was endeavouring to extend my observations to the parasites of human malaria. With all their advantages, and though their work was done some months later than mine, they found little new. To anyone acquainted with the subject, it is amusing to see how invariably and quickly G. B. G. would bring out in public as his own any little point which I had been working at, perhaps for years.

I see that both my reviewers alluded to seem to have been "bamboozled" by Grassi's clever articles, doubtless meant for the express purpose of deceiving those who were not fully instructed in such matters. For persons like these I remain merely the man who ascertained the life-history of an unimportant parasite of unimportant sparrows in India ; while the Great Grassi ascertained the life-history of the parasites of the "Lords of Creation," that annually kill millions of them in tropical countries.

It does not say much for either of these writers that they both seem to think that a man who endeavours by deceptions to acquire the credit amongst fools of another man's work, is to be seriously blamed at all. Many such cases have occurred, and there is no law against lying. Yet such a deception is morally equivalent to the theft of a watch or of a sum of money. Again, only a small percentage of medical men have ever succeeded in making scientific discoveries of any importance at all, and few of them are therefore the subjects of such thefts. Naturally, a man who has nothing is indifferent to theft. Personally I think that anyone who attempts to steal priority by fraud is exactly in the same position as a man who attempts to steal money or anything else in the same way. Both these writers adopt the contrary point of view, but I believe that their opinions on the point

are of no value, and that all victims of similar thefts would be right to expose them as clearly as possible, if only for the honour of genuine science in any branch where they occur.

The writer in the *Tropical Diseases Bulletin* has, however, not only adopted a wrong moral point of view in this matter (like many other alleged workers at medical subjects), but has been also good enough to add some personal remarks about myself. These remarks are *apropos* of nothing at all in connection with the subject, and it is difficult to understand why the Editor allowed them to appear, while they are in fact not true. The remarks merely suggest that the writer is trying to crab Sir James Barr's fund; but medical men are never too logical, and seldom seem to understand exact proprieties. The *Bulletin* has done much excellent work in keeping its readers in touch by descriptive reviews with the enormous number of papers which appear in the press in Britain and abroad; but when it indulges in *critical* reviews, we will probably ask what qualifications the writers of them have for giving critical reviews at all. Of course, everyone is entitled to his opinion on any subject; but this does not admit that all opinions on every subject are of the slightest value unless the writers are known to have done successful work in the branch of science to which they are paying attention.

A very small proportion of the world's population takes any interest whatever in science; a minute proportion ever try to do any original scientific investigation; an infinitesimal proportion ever succeed in making scientific discoveries of any real importance. Yet the whole progress of science has depended upon the labours, often very severe, of this infinitesimal proportion. It is therefore, I argue, advisable to protect them from fraudulent attempts to rob them even of the little credit which they sometimes obtain if they are lucky.

Several simple writers have begged me to leave the ghost of Grassi alone—why, I cannot say. One exposes cheats for a warning to other men, and they always deserve it. My own respect for the medical profession has certainly not been increased by my experiences of it.

Notes and News

The New Year Honours list included the names of the following men of science: *K.C.M.G.:* Lieut.-Col. Andrew Balfour, Director of the London School of Hygiene and Tropical Medicine. *Knights:* Prof. T. P. Nunn, Principal of the London Day Training College; Mr. A. Page, Past President of the Institute of Electrical Engineers; Prof. T. Zammit, Curator of the Museum, Malta. *C.B.E.:* Prof. J. S. S. Brame, Pro-

fessor of Chemistry and Metallurgy, Royal Naval College, Greenwich. *O.B.E.:* Mr. J. Jones, formerly Curator of the Botanic Gardens, Dominica; Mr. W. A. S. Lamborn, Entomologist, Nyasaland Protectorate; Dr. E. S. Russell, Director of Fishery Investigations, Ministry of Agriculture and Fisheries. *M.B.E.:* Mr. J. Aikman, Assistant, Royal Botanic Gardens, Kew; and Mr. A. A. Gomme, Librarian, Patent Office.

We have noted with regret the announcement of the death of the following men well known in scientific circles during the past quarter: Prof. H. L. Callendar, F.R.S.; Prof. C. Chilton, biologist, of Christchurch, N.Z.; Major P. G. Craigie, founder of British agricultural statistics; Dr. F. W. Dootson, lecturer in chemistry, Cambridge University; Sir S. Eardley-Wilmot, forester; Dr. S. Z. de Ferranti, electrical engineer; Admiral Sir H. B. Jackson, F.R.S.; Major P. A. MacMahon, F.R.S.; Dr. W. Maybach, of internal combustion engine fame; Dr. J. C. Melvill, botanist; Sir Archibald Reid, K.B.E.; Dr. Samuel Rideal, chemist; Sir George Thane, anatomist; Dr. H. W. T. Wager, F.R.S., botanist; Prof. T. B. Wood, F.R.S., agriculturist, Cambridge.

The meeting of the British Association this year will be held at Bristol from September 3 to 10 under the direction of Prof. F. O. Bower, who succeeds Sir Thomas Holland as President. Prof. F. J. M. Stratton, Professor of Astrophysics, University of Cambridge, has been elected General Secretary in succession to Dr. F. E. Smith. The Sectional Presidents will be as follows: A (Mathematical and Physical Sciences): Dr. F. E. Smith. B (Chemistry): Prof. G. T. Morgan. C (Geology): Prof. O. T. Jones. D (Zoology): Dr. W. T. Calman. E (Geography): Prof. P. M. Roxby. G (Engineering): Sir Ernest Moir. H (Anthropology): Dr. H. S. Harrison. I (Physiology): Prof. H. S. Raper. K (Botany): Dr. A. W. Hill. L (Education): Rt. Hon. Lord Eustace Percy, P.C. M (Agriculture): Dr. P. J. du Toit.

Prof. S. Chapman has been elected President of the Mathematical Society; Mr. R. G. K. Lempfert, President of the Royal Meteorological Society, and Prof. R. Rugglesgates, President of the Royal Microscopical Society.

Dr. J. S. Plaskett has been awarded the Gold Medal of the Royal Astronomical Society for his work on stellar radial velocities.

Prof. A. J. Dempster was awarded the prize of 1,000 dollars given at the annual meeting of the American Association for the Advancement of Science for the most notable paper read at the meeting. Prof. Dempster's paper was concerned with the wave characteristics of the proton.

Arrangements for the Fifth International Botanical Con-

gress to be held at Cambridge from August 16 to 23 are now well advanced. The rooms of the Linnean Society, Burlington House, Piccadilly, will be open to members of the Congress on August 14 and 15. On Friday, August 15, a reception will be held at the Imperial Institute, and the official opening will take place in Cambridge on the evening of August 16. Abstracts of papers to be read before the various sections are required to be in the hands of the Section Recorder by May 1. Many interesting discussions and excursions have been arranged, and the meeting should be most successful. Subscriptions (£1) should be paid to Dr. A. B. Rendle, British Museum (Natural History), S.W.7, at once. Other communications to the Hon. Secretary, International Botanical Conference, Botany School, Cambridge.

The Ninth International Horticultural Congress will be held in London from August 7 to 15 (immediately before the Botanical Congress) on the premises of the Royal Horticultural Society, Vincent Square, S.W.1. Another full programme of papers, discussions, and excursions has been arranged, and there will be a Government reception of the members of the Congress on August 8.

One of the late Lord Haldane's keenest interests was adult education, and with this object in view he took the greatest possible interest in the activities of the British Institute of Adult Education. On retiring from the office of President of the Institute in 1927 he approved a scheme for the creation of a Haldane Trust Fund to perpetuate the work. The committee of the Haldane Memorial Trust now appeals for a sum of £100,000 to be devoted (*a*) to endow the Institute, (*b*) to provide scholarships to enable adult students to take approved courses of study, (*c*) to provide tutors, and (*d*) to promote the interest of adult education in any way of which the Trustees consider Lord Haldane would have approved. The offices of the Trust are at 39, Bedford Square, W.C.1.

The Progress of Biological Control of Prickly-pear in Australia, by Alan P. Dodd, published by the Commonwealth of Australia Prickly-pear Board, contains an excellent account of the prickly-pear problem in Queensland and New South Wales, and of the very encouraging results which have rewarded the efforts of the Board appointed for its eradication. The worst species—the *Opuntia inermis*—was brought to Scone, N.S.W., in a flower-pot about 1839. From Scone plants or cuttings were taken to the pastoral areas and grown as hedges round homesteads. By 1870 the dangerous character of the growth had become obvious; in 1893 it was reported in N.S.W. that many thousands of pounds had been spent in attempts at control. In 1900 the area infested was about 10 million

acres, and in 1920 60 millions. Since 1925 the measures taken against the plant have at least checked, if not stopped, the yearly increase. The inland pastoral areas chiefly affected are worth only about £1 per acre, and this makes it economically impracticable to use chemical or mechanical methods of eradication, *e.g.* by spraying with poisons containing arsenic pentoxide. Control and eradication by biological methods depend on the rate of increase and spread of the insects or organisms employed. After the most searching tests, directed chiefly to discover whether the insects will attack other plants in the continent, four varieties have been established: the wild cochineal, *Dactylopius tomentosus*; the prickly-pear spider, *Tetranychus opuntiae*; the plant-sucking bug, *Chelinidea tabulata*; and the moth-borer, *Cactoblastis cactorum.* The cochineal finds its chief use in completing the destruction of plants attacked by its other enemies. Of these the most important is the *Catoblastis*, and it is stated that the introduction of this insect has completely changed the situation. The moth, which lives only a few days, has a wing expanse rather more than an inch. The female lays on an average about one hundred eggs, which take from three to six weeks to develop. The caterpillars live in the pear segments, eating out the interior of the pear joints. They develop in four to six weeks in summer or in six months in winter. The cocoon stage has a duration of three to six weeks. In general, there are two generations a year, in Central Queensland sometimes three. The original and only introduction into Australia consisted of about 2,750 caterpillars imported in May 1925. The second generation, in February to March 1926, yielded 2,540,000 eggs, 2·25 millions of which were liberated in selected localities. From September 1927 to March 1929 300 million eggs were released, and the numbers now present must reach many thousands of millions. The attack on the pear plant is rapid; for example, at Dulaccia, Queensland, the area of destruction increased from 50 to 1,000 acres in four months, and it is estimated that at least 30,000 acres of prickly-pear were destroyed by the insect in the period May 1928 to May 1929.

The Bureau of Standards Journal of Research, **3**, 5, November 1929, contains a most important paper by W. W. Coblentz and R. Stair on the "Transparency of Various Window Glasses to Ultra-violet Radiation." In the first place, it must be noted that the region of the spectrum which has a special healing value in preventing rickets—the "antirachitic rays" or "vital rays"—is only about 50Å to 100Å in width, its maximum activity being about 3020Å and the performance of a glass can be judged by its behaviour to rays of this wave-length. Some distributors of "ultra-violet transmitting

window glass " include 3200Å, or even longer waves in the range of therapeutic activity, thus obtaining a higher transmission figure than the facts warrant. Ordinary window glass, opaque to 3020Å, transmits from 2 to 20 per cent. at 3200Å, and if these rays (which are five to ten times as intense as those at 3020Å) had a special healing value there would be no need for special " ultra-violet " glasses ; the least green-tinted (as viewed edgewise) common glass would do quite as well. The ultra-violet transmitting glasses have not been on the market for long, and are rapidly improving, so that it is important to notice that the samples used for the experiments described in the paper were marketed before February 1, 1929. Bearing this point in mind, it may be noted that nearly all the glasses investigated solarise, *i.e.* undergo a photochemical change on exposure to sunlight, which much reduces their transparency to ultra-violet radiation. Thus the average transmission (λ = 3020Å) of vitaglass is 48 per cent. when new, and 23 per cent. after prolonged exposure to ultra-violet radiation from the sun as shown by experiments. The greater part of this change occurs in the first few days (fourteen, say), and after six weeks (it depends of course on the amount of sunlight) there is very little further change. Helioglass (Vioray) passes 58 per cent. when new and 40 per cent. when completely solarised. Corex-A (Corning-G 980A) appears to undergo no change when exposed to solar radiation, the figure for old and new glass being 89 per cent. Quartz glass gives 92 per cent. before and after exposure to the sun (quartzlite glass is to be classed with common window glass as far as ultra-violet transmission is concerned). The transparency of most glasses decreases rapidly with thickness. The figures given applied to glass 2·3 mm. thick ; " double thickness glass " (0·25 inch or 6 mm.), after solarisation, transmits only from 3 to 5 per cent. The glass must be kept quite clean, for very little contamination will reduce the transmission by 10 per cent., while if really dirty this 10 per cent. becomes 60–70 per cent. Ultra-violet glasses are of no use in north windows and of little use in winter. During the summer exposure to direct sunlight out of doors at noon is the best way to obtain the benefits of the ultra-violet radiation from the sun. The paper is long and very detailed, and the foregoing remarks give quite a very inadequate account of its scope.

The Safety in Mines Research Board is publishing a series of booklets under the general title *What every Mining Man should Know*, which sufficiently explains the object of the publication. No. 1, *Some Problems of Research*, and No. 2, *Gas and Flame* (H.M. Stationery Office, Kingsway, W.C.2, price 6*d*. and 3*d*. respectively), have already been issued, and serve to show how well suited they are to inform the working

miner of the scientific principles underlying modern developments in his trade. Booklet No. 1 in particular gives an excellent account of the aims and progress of mining research; both are illustrated by many excellent photographs. Not the least important aspect of mining research is that concerned with the prevention of coal-dust explosions. Coal dusts vary greatly in their degree of inflammability, but there has not existed hitherto any laboratory test of inflammability capable of replacing satisfactorily the laborious and costly method of repeated trials in large-scale explosion galleries. The difficulties involved in producing a simple test were discussed in *Safety in Mines Research Board Paper, No.* 31, published a year or two ago. Such a test has now been evolved, and is described in *Paper No.* 56 by A. L. Godbert and R. V. Wheeler (H.M. Stationery Office, price 6*d.* net). The method of test is to measure the inflammability by ascertaining the proportion of inert dust necessary to suppress inflammation when small quantities are blown by oxygen through a heated tube. It has been proved to give results comparable with the explosion gallery trials. A refinement of the same test was used to study the effect of the fineness and chemical composition of coal dust on its inflammability. *Safety in Mines Research Board Papers* 33 and 48 had shown that the inflammability of a coal dust ran roughly with its volatile matter content, but there were notable exceptions to the general rule. The present paper proves that there is a much closer relationship between the inflammability of a coal dust and the reactivity of its ulmin compounds, as demonstrated by their relative capacity for absorbing oxygen. Occluded gases were found not to affect materially the inflammability of the dusts.

Sir Richard Gregory gave an interesting account of Weather Cycles in his presidential address to the Royal Meteorological Society last January. The cycles which the Press have made most familiar to the general public are the Cold and Warm Periods enumerated by Buchan in 1869 as a result of observations of weather conditions in Scotland for the preceding ten years. There is, according to Sir Richard, no scientific evidence that they have any relation to temperature conditions in the south of England. Brückner's thirty-five-year cycle (1890) rests on a firmer basis. It was based on an examination of European weather records (so far as they were available) since A.D. 1020. The period of the cycle has varied from twenty to fifty years, and it is, of course, quite useless for weather forecasting purposes. It is true, however, that *on the average* agricultural conditions are better in the years which fall in the warm dry half of the cycle than in those which occur in the cold and wet part. The rainfall of Great Britain

shows a marked fifty-year period. Groups of wet years occurred *c.* 1770, 1821–9, 1871–80, and 1922–8, so that we should now be entering a dry period. The eleven-year cycle of maximum sunspot activity has no visible effect on weather in Western Europe (Brooks), but the rainfall records for certain parts of equatorial Africa follow this cycle in a really remarkable manner.

The *Annual Report of the Director of the Bureau of Standards, Washington*, for the year ending June 30, 1929, contains a brief account of a very large number of investigations in progress or recently completed at the Bureau. Many of these are of general scientific interest.. A comparison of the absolute and international ampere is in progress; one international henry has been shown to be $1{\cdot}00053 \pm {\cdot}00003$ absolute henry. A standard of electrical resistance of much greater constancy than any hitherto made has been obtained by annealing manganin wire at 600° C. in carbon monoxide at a greatly reduced pressure and then effectually sealing the containing case. The Waidner-Burgess absolute standard of light has been realised by the use of a hollow enclosure of fused thoria in a bath of molten platinum contained in a crucible of fused thoria. This source gives 58·9 candles per sq. cm. and is apparently both constant and reproducible. The experimental work on the gravitation constant G has been finished, the mean of the eleven values (out of sixteen) so far computed is $6{\cdot}668 \times 10^{-8}$ c.g. units. Apparatus is being designed to redetermine the coefficient of expansion of the thermometric gases between 0° C. and 100° C., and so to provide new data for calculating the interval of temperature between the ice-point and the absolute zero. The designs include a thermometer with no "dead" space (or "emergent stem"), and sensitive monometers to allow measurement to be taken at low pressures. The piezo-oscillators used for the Bureau's frequency standard have been modified, and are constant to within 1 part in 200,000; experiments have been made to determine the best specification of a standard "white," and others on the very difficult problem of X-ray dosage are in progress. A vast amount of commercial work is done; the staff of the Bureau numbers 1,072, the yearly expenditure is over 2·5 million dollars, and there is no first-aid station, either for accidents or for the supervision of those working with radium, with X-rays, or on furnace operations!

The Report of the Building Research Board for 1928, published last December (H.M. Stationery Office, price 3*s.* 6*d.* net), summarises a vast amount of research of first importance to the architect and builder. Of general interest is the account of the work undertaken to find a suitable stone with which to replace the decayed Anston stonework of the Houses of Parliament.

In 1926 the Office of Works recommended the use of Stancliffe stone (a siliceous sandstone) for this purpose. At first the Research Board approved this choice, but this approval was withdrawn when it was discovered that " the use of dissimilar materials in proximity in a building may seriously impair the weathering properties of good materials, and that, in particular, even good sandstones are liable to excessive decay when used in conjunction with limestone." Laboratory experiments showed that Stancliffe sandstone is particularly liable to disintegration by the salts (sodium sulphate and magnesium sulphate) drawn into it when it is used in contact with Anston limestone. Fortunately, while this investigation was in progress, the proposal to use Stancliffe stone was abandoned on æsthetic grounds. H.M. Office of Works then decided to use either Ketton or Clipsham limestones. These were investigated by the Board, and both were considered to be suitable, Clipsham being perhaps rather better than Ketton. Another series of experiments were made to select a suitable mortar, since, as is well known, unsuitable mortar leads to disfiguration and excessive decay of stonework.

Included in the section headed *Intelligence and Special Investigations* is a brief report on a proprietary surface waterproofing compound consisting of aluminium soap and fats in a paraffin oil medium. This material " was applied to red wire-cut facing bricks and gave a very satisfactory reduction in the flow of water into and through the specimens." It was considered " that this material is an excellent example of its class . . . it should be realised, however, that such surface-applied preparations do not last indefinitely." The strong winds and heavy rain which have prevailed this winter have enabled water to penetrate many exterior walls hitherto regarded as damp-proof, and the Director of Research is likely to receive many enquiries this summer for a suitable waterproofing material.

Reading between the lines in the Chairman's introduction, it would appear that the industrial interests which the Board is mainly concerned to benefit are not yet giving the whole-hearted assistance essential to the full success of the work, although interest is quickening.

Volume XXI of the *Collected Researches of the National Physical Laboratory* is entirely devoted to a reprint of papers on electrical subjects, mostly by Dr. Dye, Mr. Hartshorn, Dr. Smith-Rose, or Dr. Hollingworth. It includes a description of the Standard Mutual Inductance presented to the Imperial Japanese Government and Dye's classical paper on the " Piezo-electric Quartz Resonator."

ESSAY-REVIEWS

THE KINETICS OF CHEMICAL CHANGE IN GASEOUS SYSTEMS. By Alfred W. Porter, D.Sc., F.R.S., Emeritus Professor of Physics in the University of London. Being a review of a volume under the same title by C. N. Hinshelwood, M.A., F.R.S. Second Edition. [Pp. 266.] (Oxford University Press, 1929.)

In 1864 Guldberg and Waage put forward a theory of the rate of chemical change in liquids based upon the probabilities of collisions between the reacting molecules. In 1859 Clerk Maxwell had read a paper before the British Association showing that " so many of the properties of matter especially when in the gaseous form can be deduced from the hypothesis that their minute parts are in rapid motion, the velocity increasing with the temperature, that the precise nature of this motion becomes a subject of rational curiosity."

On the results of this inquiry the modern approach to chemical statics and kinetics is made. In an unbalanced change we may expect a relation $dn/dt = -kn^r$ where n is the concentration and r is the number of molecules that must collide simultaneously in order that a reaction may occur. The factor k is the reaction constant. Thus in a bimolecular reaction $r = 2$. Now Maxwell's theorems enable the number of collisions in unit time to be calculated—at any rate in the case of a perfect gas. If $\bar{u}$ is the mean velocity of a molecule, A, its cross-sectional area, n the number per unit volume, then each sweeps out a volume $4Au$ in unit time, all the molecules in which will be touched by the molecule in question. Thus the number of contacts in unit time are $4Aun$. All the rest have similarly been making contacts and the total number of contacts in unit time (neglecting the difference between n and $n - 1$ and taking care not to reckon a collision twice over) is of the order $\frac{1}{2} \cdot 4A\bar{u}n^2$. This simple calculation is right dimensionally, but the numerical factor requires to be changed when allowances are made for niceties of behaviour: the final result is $\sqrt{2} \cdot 4Au \cdot n^2$. Now A and $\bar{u}$ and n can be obtained (by aid of Maxwell) from experiments on viscosity, etc. For HI at 556° A, $\bar{u} = 3{\cdot}3 \times 10^4$ cms/sec, $4A = \pi\ (3{\cdot}5 \times 10^{-8}\text{ cms})^2$, and if the concentration is 1 gr. mol. per litre, $n = 6{\cdot}1 \times 10^{20}$ per cc. Hence in one millionth of a

second each molecule will have collided with about 100,000 others and the total number of collisions in this time will have been of the order of 10^{11}.

It is perfectly clear, therefore, that these collisions cannot all be effective in producing disruptive change of the HI molecule. The same remark applies to other substances as well. The world would have been a much livelier place while it lasted if every collision had been effective, though it would probably have run down into a stationary state by the present time. This indicates a difficulty which had to be faced in the endeavour to calculate the rate at which chemical change should proceed ; and it is the problem with which the volume under review is primarily concerned.

Each reaction has to be specifically considered. Thus for the reaction $2NO + O_2 = 2NO_2$ the speed is exceedingly great at temperatures at which the union of hydrogen and oxygen to form water is immeasurably slow, even though the numbers of encounters may be comparable with one another. It is clear that to be effective the encounters must be between molecules in an exceptional state of some kind, and it is the business of chemical philosophy to find out what this exceptional character is.

This problem is now an old one and many attempts have been made to solve it. The particular view which Mr. Hinshelwood adopts and develops is that (although there may be many other causes) the predominant condition for the colliding molecules to react is that each must possess energy in excess of a value depending upon the particular kind of molecule under consideration. The number of molecules in random motion that have energy of translation of at least a given amount E is easily calculated from Maxwell's principles ; it can be written

$$\frac{N_e}{N} = \frac{\text{Number with energy E or over}}{\text{Total number}} = e^{-\frac{E}{RT}}$$

where e is the base of natural logarithms, R is the gas constant and T the absolute temperature. The same kind of equation is true whatever the type of energy, provided that it can be expressed as the sum of three quadratic terms. The picture that can be formed is that, *e.g.* in the case of a head-on collision both molecules are brought instantaneously to rest while their energy is spent in producing such internal changes in the molecules that they are brought into a reacting state. Such molecules are said to be "activated," and it is assumed that only activated molecules can take part in the chemical changes which result.

is the formula which Mr. Hinshelwood tests for a

variety both of straightforward and of more complicated reactions. From the equations already given we have for a bimolecular decomposition

$$-\frac{dn}{dt} = \sqrt{2}\cdot 4A\cdot \bar{u}e^{-\frac{E}{RT}}\cdot n^2. \qquad \text{(I)}$$

so that the velocity constant is the coefficient of n^2. This is combined with Arrhenius' equation, which can be obtained most directly by differentiating log k, treating $\bar{u}$ and E as independent of temperature. This differentiation gives

(II)

The details of the test are as follows:

(i) k is determined experimentally at various temperatures.
(ii) Thence E/RT is derived from equation II.
(iii) The value of E/RT is inserted in (I) and k is calculated.
(iv) The calculated and experimental values of k are compared.

These values of k are found to agree as closely as could be expected—they are at least of the same order of magnitude instead of being utterly discordant. The conclusion drawn is that $e^{-\frac{E}{RT}}$ is the right kind of factor to represent the retarding influence present in a chemical change. Physically it means that the molecule remains inert until a certain energy E is imparted to it. Its atoms are now more loosely held together, and when other kinds of molecules, also activated, are in the field there can be an interchange of atoms, and when energy is withdrawn, fresh types of molecules are formed—these are the products of the reaction.

The reviewer does not think that the book properly emphasises the need for constancy of E, as regards temperature in the test itself and the importance of it as an experimental quantity. This constancy is what would be expected if E does in reality represent the energy change of a transformation inside the molecule. It may be also pointed out that if Arrhenius' equation is written $(d\log k)/d\left(\frac{1}{T}\right) = -\frac{E}{R}$ it can then be seen by direct inspection of the rectilinear character of the experimentally derived curves of log k against 1/T (on p. 42) that E/R is, in reality, a constant.

A student may notice that a certain number of the

molecules will have energy considerably greater than E and may possibly inquire what happens to the excess when activation has been brought about. Presumably the molecule will leave the site of the accident with the remaining energy or perhaps it may excite further internal changes which may not be relevant to the particular chemical change and which may therefore go unrecognised. It would be useful to point out to him that the number that possess energy of twice the value E (or more) is only $e^{-\frac{E}{RT}}$ times the number that have the energy E (or more) and we have seen that this ratio is almost vanishingly small. This small group with its great possessions is therefore of negligible importance.

The examination of the experimental phenomena which the author describes is surely the most complete that has yet been made in a single volume. The examination is not without its difficulties, but Mr. Hinshelwood carries it out with great fairness to conflicting views. Few reactions are as simple as they are imagined to be ; and when the need arises for introducing the conception of sequences or chain reactions, the multiplication of constants makes it difficult to come to a decision as to the reality of the chains proposed. Moreover, many professedly homogeneous reactions are in reality heterogeneous—at least in part, because the walls of the vessels are found to exert an influence.

It would take a volume to discuss this volume thoroughly. The reviewer will take one case only. Mr. Hinshelwood is convinced, in spite of much controversy, of the reality of unimolecular reactions—that is, those for which the decomposition follows the law $-\frac{dn}{dt} = kn$. Rather curiously he does not even mention the only cases in which this law, in its simplicity, applies, viz. :—the decomposition of radium and its congeners. (Perhaps because some of these bodies are solids and not gases.) The transformations of radium, etc., have all the expected qualities of unimolecular reactions. They proceed without respect to external conditions—"Ohne Hast, ohne Rest"—each molecule behaving just as it would if no other molecule were present. It is noteworthy, however, that although outside stimulus is of no avail—even collisions effect no change—the molecules do not all break up at once. The molecules cannot all be alike, but each one is self-determined —each is master in his own house. It must be remembered that the molecule in this case is an atom—it is much more firmly held together than the atoms are with each other in a polyatomic molecule. What determines disintegration of radium is not known. If it is old age, then old and young

must be mixed together in random proportions, for the average rate of decay is wonderfully constant.

It may be argued that this is only a special variety of unimolecular reaction—viz., one in which the heat of activation is zero.

When we examine cases which are apparently unimolecular, but in which the energy of activation is not zero, it is necessary to inquire from where the molecules get this essential stimulus. Perrin and W. C. Lewis attributed it to the surrounding equilibrium radiation; but this supposition has broken down under experimental test. Lindemann has assumed that it is derived from *previous* collisions, an interval of time elapsing between reception and expenditure of the energy, many of the activated molecules becoming spontaneously de-activated in this interval. If this be so, the apparent unimolecular character would need a definite adjustment of the rates of activation and spontaneous deactivation for which no adequate explanation has yet been given. In other cases chain reactions are speculated with. It is very questionable whether any of these cases can be *logically* embraced under the term unimolecular merely because on the whole the decomposition takes place at a rate independent of the initial pressure. In the reviewer's opinion it is wiser in the interests of logical nomenclature to classify them separately under the head of complex reactions. It will be understood that this is an expression of opinion on nomenclature and is not a criticism of experimental facts.

A very few slips occur. Molecular heat is used in two very different senses on page 14; "unimolecular" should be "bimolecular" near the top of page 94; "power" should be "factor" on the second line from bottom of page 100. The particular way in which the solidus is used is to be deprecated; some of the formulæ are susceptible of several interpretations. Some of the printing on the diagrams (page 42) is much too small.

Finally: this is a second edition. Its value is proved by the speedy call for it. The subject is very much alive. Chapters III and V have been entirely rewritten and a new chapter on Chain Reactions has been added.

The author has very wisely kept to gas reactions: the question of liquid reactions is for a future generation.

FABULOUS POETRY AND SATIRE. By R. L. Mégroz. Being a review of **Fables and Satires** by Sir Ronald Ross, K.C.B., K.C.M.G., F.R.S. [Pp. iii + 71.] (London: Harrison & Sons. Price 7s. 6d. net.)

When Matthew Arnold said that poetry is a criticism of life he might well have been describing satire, except that the

most effective satire is usually adverse criticism of life (while the best criticism—contrary to popular belief—is intelligent appreciation). The test of the excellence of satire is the force of its attack upon ugly things rather than the justice of its praise of the beautiful. Nevertheless, there is the anomaly that great satire requires a poet's power of expression; the word must be potent, that is, beautiful, even in the cause of hatred. The satire will live by reason of its expressiveness; and the justice of the jibe, the wisdom of the argument intended originally, may have but an ephemeral concern for posterity. So long as there was original power, the immediate cause of the display of it can easily be replaced by the reader with a general one. Therein is satire like poetry, for the magic of the word transfers the particular into the universal meaning.

Look at Pope's lines on Addison. We may or may not recognise their justice as applied to Pope's erstwhile friend, and we may or may not detest the malignity of Pope's feeling; but the satirist put such a force into his words, poured such energy out of his frail frame into the searing phrases, that they brand for all time the critical vice of those who—

> Damn with faint praise, attend with civil leer,
> And, without sneering, teach the rest to sneer;
> Willing to wound, and yet afraid to strike,
> Just hint a fault, and hesitate dislike;
> Alike, reserved to blame, or to commend,
> A timorous foe, and a suspicious friend. . . .

If this is to attain the highest rank in satire, it is not necessary to demonstrate, except by example, that satire must be a kind of self-immolation of the poetic mind. The poet's gift of words must be there, but his gift of love and praise must be submerged by his power of hating.

Granting all the purely personal resentments which usually awaken the satirist to his best efforts, it remains true that satire may be considered as the expression of an intense perception of the flaws in society. There is, in other words, a unifying vision behind the personal feeling. The mind of the satirist may be attracted in one of two main directions when he has come to feel that life is a sort of confidence trick played on the idealist. In one direction is perceived a breaking apart of moral and material forces in human society; in the other, a similar alienation between material and æsthetic forces. If these terms are a little vague yet, the distinct tendencies they imply can be illustrated by Shakespeare's sonnet LXVI, which is the cry of the outraged moralist, exposing life as a state in which he beholds "desert a beggar born,"

> "And needy nothing trimm'd in jollity, etc.,"

while the æsthetic aspect engages the poet's attention in Shelley's "Hymn to Intellectual Beauty," which contrasts what things are with what they might be if only they were "consecrated" by the spirit of beauty.

To blame a satirist for having too much of, or rather too constantly, the spirit of poetry in him for effective hatred seems a rather irrelevant occupation, but we have to do something of the sort after reading Sir Ronald Ross's extraordinary *Fables and Satires*. He uses the conventional symbols of non-human creatures, as Æsop and La Fontaine did, to point the moral of his satires, but his imagination is continually being carried away by sudden glimpses of the beautiful and the sublime. In a curious satire on "Another League of Nations," which seems to be weakened by some confusion of the argument, he thinks of Jove casting away his thunderbolts on hearing how the creatures of earth, air, and water have all resolved to live in peace and amity for evermore. Whether the succeeding argument can rightly be applied to the European League of Nations, or whether it rather too obviously ignores the possibilities of human civilisation, we need not discuss now; but it is clear that the poet readily forgets his satire to pursue the vision of immortal Jove:

> When he beheld th' Enthusiasm,
> Jove flung his Thunders in the Chasm
> Of that Eternal Fire that burns
> Before his Feet—whose Smoke upturns
> And mingles with the Wraiths of Snow
> That wind-blown o'er Olympus go
> For ever—that eternal Fire
> Where all the shrinking Pasts expire,
> Mountains and Suns and Oceans—aye,
> Beauty and Love and Friendship die.
> So in that Fire of Fate he hurl'd
> His Thunderbolts. . . .
>
> And high th' exultant Flame outshone
> From Pindus unto Pelion.
> Only his Eagle at his right hand
> Deign'd not t'obey the God's command,
> But his red eye ranged like the Sun
> Through Thunderclouds when day is done.

Fortunately (one is tempted to say!), Jove soon had to get a fresh supply of thunderbolts to quieten the tumult on the earth—fortunately, because it brings back the poet in place of a half-hearted satirist:

> And laughing Jove reach'd forth an arm
> And took new, living Lightnings, warm
> And limber, from the fuming Urn
> Wherein the Destinies are born. . . .

Take the delightful fable of Ariel and the Hippopotamus. This is dedicated to Rural Magnates (nearly all the fables are dedicated to human types—the Reformer, the Public, Kings, Critics, and so on). Ariel, loitering on his journey, meets the Hippo, who asks him for the latest news. When he hears that great Jove has the gout again—

" Why," said the Hippopotamus,
" That ain't no call to make a fuss ;
I've had the same and am no wuss."

Then Ariel

" 'Tis said that Cytherea, queen
Of beauty, weds to-day at e'en
The sooty Vulcan hump'd and mean."
" There," said the Hippopotamus,
" That party I will not discuss,
She might have me and do no wuss."

The witty gaiety of the caricature of conceited provincialism is maintained to the end, and yet I venture to say that if the opening lines had been less good, or not there at all, the satire would be nearer perfection, having a unity of effect which now it lacks. For what one remembers best is the poetic opening :

Fine Ariel, serf to Prospero,
Sped on the Great Meridian
For jetty pearls from Andaman
To make a chaplet to declare
The beauty of Miranda's hair.
When at the desert African,
Out of his master's ken, and slow,
Lag'd on his errand, loth to go ;
For sweltering Sol, with leaden beam,
Made stagnant all the windy stream
And suck'd from earth a stifling steam.
There idling still, the lazy Sprite
Beheld below, beneath his flight,
The Lord of Rivers, blackly bright,
Who, planted in a marshy bed,
On mighty rushes munching fed
And sigh'd for more the more he sped. . . .

As if to illustrate this tendency of the poet to brush the satire to one side and chase the flying beauty, there is one piece in this volume, " Alastor " (it appeared among the " Apologues " in the volume entitled *Philosophies*), which is only very slightly satirical, but bears triumphant testimony to the writer's genius for pure poetry. To follow so closely on the heels of Shelley and not to be shamed is what only one other English poet has shown himself capable of doing. He was Francis Thompson, but where Thompson exaggerated Shelley's easy freedoms, Ross restrains,

imposing a classical austerity of form upon the molten rushing dream and the wild music.

The three most severely satirical pieces in *Fables and Satires* deal with comparatively impersonal themes. This is probably characteristic of the writer's genius, occupied always with the abstract truth more than with a personal resentment. These pieces are aimed at the teaching of Greek and Latin, the teaching of English (perhaps one should say the old-fashioned teaching), and the speaking of poetry on the stage. "Our Stage," indeed, is exceptionally sarcastic in its humour. On the stage, after all, says the poet, "our Shakespeare's art survives":

> But only for our children, wives
> And friends we wish to give a seat to—
> A benefit but not a treat to.
> His verse the actors redispose
> By weak'ning rhythm, rhyme and close,
> To make it more like sense and prose.
> And mar the march of Marlowe's line,
> Lest fools complain it is too fine.
> For th' audience brings such wisdom wi' them
> They feel that no one talks in rhythm . . .

and the angry note is sustained until the conclusion:

> The pointless tale and punning jest
> Rouse ardour in each simple breast,
> Till thund'ring joy breaks up and down
> The foolish faces of the town;
> Tho' all can see the silly revel
> Is made to please the lowest level,
> And strangers ask what folly it is
> That so delights the simple British.
> Nor blame the authors or the actors—
> Who writes for fools fears no detractors.
> They have to seek the highest wage,
> It is our audience damns our stage.

(I shudder to think what might be the consequence of the satirist paying attention to the well-named "Talkies"!)

That exoneration of the people doing their jobs as best they can to earn a living in an unsatisfactory, not to say lunatic, economic system, is characteristic of the writer's quick sympathies, not only with human beings but with other creatures. (A piece longer than most in this volume is on a horse dying in the street—it might be a friendless, outcast human so far as the poet's feeling is concerned.)

The scornful play with imperfect or improvised rhymes is a stylistic effect noticeable in others of these satires, and it reminds us how close they come at times to the satirical Bishop Butler. The manner invites the comparison, and the comparison at

once shows that something more than mere manner is involved. I am thinking especially of the passage in which Butler describes the language of Hudibras. That " Babylonish dialect ":

'Twas English cut on Greek and Latin,
Like fustian heretofore on satin;
It had an odd promiscuous tone,
As if H' had talk'd three parts in one,
Which made some think, when he did gabble,
Th' had heard three labourers of Babel,
Or Cerberus himself pronounce
A leash of languages at once.

The argument of " Our Pronunciation of Greek and Latin " enables us to compare it with Butler still more easily. In this piece Ross is tilting a sharp lance at the dragon of misguided pedagogy, describing, with his usual sympathetic understanding, " the little wretched scholars "

With inky thumbs and irksome collars,

who are expected to learn

a tongue
Which no one ever said or sung;
Where Sizar raised his haughty head
And " vinai, vaidi, vaisai " said;
Or eke the ancient Greek to know
According to Oxford-atte-Bowe,
In which all common sense renounced
Each syllable is mispronounced.
(Forgive, I pray, the Gallic rhyme;
For better I've no taste nor time).

His anger is because of wasted beauty; because

The thund'ring Iliad is become
A text to teach a rule of thumb;

and so—

Achilles lectures on the article,
And Hector teaches the Greek particle;
But what they did or why they did it
Irks neither those who read nor edit.

If the theme returns, as I think it must, to the satirist's preference for the poet's proper rôle, " The Poet's Retirement " (dedicated, with misgivings, to Urania) may be taken as perhaps the best example of the artistic duality of effect noticeable in many of the satires. It is the story of the poet's conversion—a conversion that much resembles abduction—by Science from his dalliance, as of a second Paris, with Poetry, Fine-Art, and Music. Again the light touch of humour is felt, too. The

poet was just answering the three "slipper'd Maidens" that he loved them all so well he could not choose :

> "Alas!" I cried, but checked the word,
> For close behind a footstep heard
> Compel'd me turn; when lo! that Maid,
> Dress'd in black velvet, who bewray'd
> Plump Popes and Pastors once to fear,
> Came up and took me by the ear.
> "Is this the way?" she cried. "You waste
> Time should be spent in huddling haste
> To harry Ignorance to her den,
> Or pink fat Folly with the pen.
> Small, unobservèd things to use,
> Each with its little mite of news,
> To build that sheer hypothesis
> Whose base on righteous Reason is,
> Whose point among the Stars. . . ."

"The Ascent of Parnassus" (Thalia having commanded the poet to write a satire) is another aspect of his attitude to Athene, now identified with Science, and an indication of the need to harmonise the conflicting ambitions of genius, for the Muse of Satire tells him :

> Athene I, and I the Muse—
> One; and one minister may use,
> Tho' humble.

The essential alliance, as well as the difference of function, of poetry and satire has been already suggested, but it is of uncommon biographical interest to see how the poet has found his own reconciliation with diverse employs. The acquainted reader will not forget, moreover, the sonnet to "Thought" and the one to "Science" in *Philosophies*. Both tell of the conflict of opposing interests. For a time the poet had to deny that the Muse could also be Athene :

> Spirit of Thought, not thine the songs that flow!
> To fill with love or lull Idalian hours. . . .

But achievement in science was followed, or—so prompt was the return to art—accompanied, by service of the Muse again.

These reflections arise naturally from reading a volume so rich in personal as well as artistic interest, but they might be regarded as diverting attention from the feast actually offered to the reader in *Fables and Satires*. I have been so intent on tracing peculiarities which help to make this volume unlike any other, that few of the observations relating to the sheer pleasure in store for a reader have yet found a space. It will be making

the best amends perhaps to quote an exceptionally brief satire in full :

THE TOAD AND THE FAYS

(*Dedicated to Philosophers*)

There sat a Toad upon a lawn
Lost in a dream of fancy ;
His right foot in a Rose was set,
His left upon a Violet,
His paunch upon a Pansy.
Some merry Elfins passing by
At sight of him were sore affrighted,
And would have fled ; until he said,
" My little dears, if you knew why
I look to heaven thus and sigh,
I think that you would be delighted.
The Stars rise up and fall, the Stars
Do shine in pools and stilly places,
The Lilies blink on sandy bars,
The Midges move in flickering mazes ;
But I profoundly pore upon,
And reason, think, and cogitate,
And marvel, muse, and meditate,
Why had the ancient Mastodon,
So few sad hairs upon his pate ? "

A trifle ? But it is perfect in its own sphere. The pleasure of seeing wisdom feast upon laughter is one offered on almost every page of *Fables and Satires*. Over and over again I have been reminded of the youthful Keats's definition of Poetry as " Might half slumb'ring on its own right arm."

REVIEWS

MATHEMATICS

An Introduction to the Geometry of N Dimensions. By D. M. SOMERVILLE, M.A., D.Sc., F.N.Z. Inst. [Pp. ix + 196, with 60 figures.] (London: Methuen & Co., Ltd., 1929. Price 10*s*. net.)

THE book gives a useful introduction to the subject of n-dimensional geometry. The writing is lucid, and the presentation of the subject such as to render the book of real interest to the student who approaches the subject for the first time. The method of the author is to consider first the case of well-known results in 3-dimensional geometry and to pass thence to their extensions in n-dimensional regions, giving in addition certain unexpected results for which analogy would be entirely unreliable.

In the first few chapters he explains some fundamental ideas. Thus he begins with the axioms of incidence with reference to the point, straight line, and plane, and uses them to obtain all the elements of space of 3 dimensions. He extends these ideas in particular to space of 4 dimensions, and thus leads on to space of n dimensions, giving some illustrations from enumerative geometry.

From the ideas in Euclidean geometry of parallel lines and planes he develops those of direction and orientation, following as before with an extension to 4-dimensional, and finally to n-dimensional space. Thus he introduces a new idea of the "degree of parallelism," dealing eventually with the "parallelotope"—the analogue in n dimensions of the parallelogram (in 2) and the parallelopiped (in 3).

Next he considers the problem of perpendicularity. Thus in 3 dimensions: "If a straight line is perpendicular to two intersecting straight lines at their point of intersection, it is perpendicular to every line in their plane." So in n dimensions: "Through any point O of a straight line l there is a unique $(n-1)$-flat which is normal to l. And at any point O in a given $(n-1)$-flat there is a unique line which is perpendicular to every line of the $(n-1)$-flat which passes through O."

There follows a discussion of the distances and angles between flat spaces. The general conception of projection in n-dimensional geometry is propounded; the author considers briefly the case of parallel projection, and as a special case of this, orthogonal projection, with an application to the orthotope (a special form of parallelotope).

Chapter V deals with the analytical and projective aspect of the subject. Thus a point is represented by the ratios of $(n+1)$ numbers or co-ordinates $x_0, x_1, \ldots x_n$. The equations of a line, plane, . . . , $(p-1)$-flat in n-dimensional space are discussed. The principle of duality is enunciated, and having defined a "variety of order r" as represented by a homogeneous equation of degree r in point co-ordinates x_ν, $(\nu = 0, 1, \ldots, n)$, he considers in detail the quadric variety for which $r = 2$.

The subject is next approached from the analytical and metrical aspect, and the author shows how the system of projective co-ordinates can be made metrical. Special systems are defined corresponding to trilinear, areal, and rectangular cartesian co-ordinates—the latter being the simplest

system available. The generalisations of some of the well-known analytical expressions (*e.g.* direction cosines of a line) are given, the term "general cartesian co-ordinates" being applied to the generalised form of oblique axes of reference. Finally, some applications of Plücker's co-ordinates to linear spaces are considered.

The remaining chapters deal with polytopes, the regular polytopes being treated in detail. Some of the elementary ideas in topology are presented, and a special chapter is devoted to Euler's Theorem and its relation to the angle-sums of a polytope.

SYBIL D. JERVIS.

The Theory of Determinants, Matrices, and Invariants. By H. W. TURNBULL, M.A. [Pp. xiv + 388.] (London: Blackie & Son, Ltd., 1928. Price 25*s*.)

PROF. TURNBULL'S book fills a considerable gap in English mathematical literature. No book of similar intention has appeared since Salmon's *Higher Algebra*; no book of the same scope could have appeared until now. At the date of Salmon's treatise the theory of invariants was making rapid progress in this country through the work of Cayley, Sylvester, and their followers. Salmon aimed at putting into the hands of students a book which, starting from elementary ideas, should make accessible the latest work in the subject.

Since then the centre of algebraic research has shifted, and the English student finds the field both vastly more extensive and less accessible than before. To attempt, in a book not much longer than Salmon's, to sketch even the outlines of this vaster field is heroic. Yet this is what Prof. Turnbull has done, and with a great measure of success.

More than a third of his space is devoted to determinants and matrices; but although his treatment of these is remarkably thorough, he evidently regards this part of the book as subsidiary to his main theme, which is the modern theory of invariants, and he is interested in developing general theorems rather than in obtaining the more special results which often figure largely in an account of determinants. Then, in the remainder of his limited space, he makes a wide survey of modern results and methods in the theory of algebraic invariants. This cannot, of course, be accomplished without severe condensation nor without the sacrifice of matter that usually finds a place in a treatise on this subject. No attempt is made, for example, to include a detailed study of canonical forms and a classification of their invariants. But the student who succeeds in mastering what is, compared with many that are put before him, a small book, will be in a position to read widely and with an understanding of the most recent developments. The examples that are provided with all the chapters will make his task

Prof. Turnbull acknowledges his debt to Weitzenböck and Study, but though he expounds some of the results of these authors, he has departed widely from their methods, and has re-created the whole subject in his own way. For this reason his work should prove of interest even to the expert algebraist who is already acquainted with most of the results. It is a pity, however, that he has not been able to find more room for geometrical applications, which are almost absent. This exclusion is, as the preface makes clear, deliberate, but will be unfortunate if it alienates a class of readers who might otherwise find the book of especial interest.

R. C. J. H.

Der vierdimensionale Raum. By DR. ROLAND WEITZENBÖCK. viii + 142.] (Braunschweig: Friedr. Vieweg & Sohn, 1929. 9 M., Bound 10.50 M.)

PROF. WEITZENBÖCK, well known to mathematicians for his work on the theory of algebraic invariants, writes here in a lighter vein. Books, other

than geometrical treatises, on multidimensional worlds have in recent years concerned themselves mainly with relativity, but a generation ago the fourth dimension was the happy hunting ground of mystics and romancers. Books like those by Abbot and Hinton popularised the concept while still encouraging speculation as to the "reality" of the higher dimensions, and the type of mind that finds analogy a convincing argument hastened to establish new heavens and new earths in regions so arguably close yet so conveniently unattainable.

Since then physics has adopted the fourth dimension, and has twisted space out of all recognition, but it has adopted also the austere view of the geometer, for whom the term "real space" has no significance, and all logically possible geometries deserve equal respect. As a result, it is now less easy to write a popular book on four-dimensional space. There is a greater wealth of material, but it contains less fun and more philosophy. Dr. Weitzenböck accepts the philosophy and disapproves mildly of the fun, but by the use of an historical method he succeeds in giving us both in good measure.

From a sketch of the more easily comprehensible results of four-dimensional geometry he passes on to consider the influence of the concept of higher dimensions upon modern scientific thought, and touches lightly upon relativity. These duties done he turns to entertain us with an account of the speculations of the romantics, from Mr. H. G. Wells, who found the fourth dimension an admirable field for yarn spinning (perhaps because the three-dimensional knots can be untied so easily), to religious mystics like Ouspensky, and Bible searchers like W. A. Granville who, in Paul's ecstatic phrase "breadth and length and depth and height," can see a revelation of the four-dimensionality of God.

The book concludes with a Bibliography unusually polyglot and astonishingly long. Of this medley of human ingenuity, perversity, and error, Dr. Weitzenböck gives us the essence in his few but crowded pages.

R. C. J. H.

Leçons sur les Équations Linéaires aux Différences Finies. By N. E. Norland. [Pp. vi + 152.] (Paris: Gauthier-Villars, 1929. Collection de Monographies sur la Théorie des Fonctions. Price 50 fcs.)

Finite differences and the allied formulæ of interpolation are no new mathematical ventures, and the best-known names in their literature are Newton, Stirling, Gauss, Lagrange. On the other hand, modern treatment of an old set of problems has changed difference equations from a commonplace exercise into a study which demands considerable preliminary equipment. Here, as elsewhere in mathematics, a generation pays in the ignorance of the many for the knowledge of the few: for a method of solving the simplest difference equations is now in the armoury of comparatively few young mathematicians. Boole's Finite Differences has dropped out of use and copies of it are scarce, while, until some five years ago, the one relevant modern book in the libraries was Wallenberg and Guldberg, *Theorie der Linearen Differenzgleichungen* (Leipzig and Berlin, 1911).

The dearth of good books has now definitely passed. In 1924 the theory of differences, interpolation, and many other matters affecting numerical work were excellently treated in book form by Whittaker and Robinson in their *Calculus of Observations*. In the same year Nörlund published his *Differenzrechnung* as a volume in the German "Yellow backs," or, to be formal, *Die Grundlehren der Mathematischen Wissenschaften*. Two years later came Nörlund's first Borel tract on Séries d'Interpolation, and the present volume on linear difference equations completes the work. Having these four books, the mathematician is provided with a full measure of information on differences and their uses both in computation and in theory.

The book before us is concerned with the possibility of finding solutions of difference equations and with the function character of the solutions found. The algebra is inevitably heavy upon occasion, but the devices used to obtain complete solutions in series and to prove the convergence of the series obtained are well worth study. Nörlund's own method is to use series of inverse factorials, a powerful weapon largely perfected by his own researches, while Laplace's transformation gives rise to integral solutions which may also be handled by the aid of such series. A full account is given of the method of successive approximations, developed by R. D. Carmichael, and of Birkhoff's arresting solution by means of matrices. This represents a distinct advance on the author's earlier *Differenzrechnung*, where the matter was dismissed in some eight pages. On the other hand, the more advanced viewpoint here adopted has forced Poincaré's theorem, relating to a somewhat particular type of equation, into a mere footnote reference. This is a pity; for, though particular in its scope, it is perhaps the most strikingly neat result in the whole theory.

The high standard of the Borel tracts is upheld by this its latest number. In one respect it falls short of the author's Interpolation volume. When this appeared one hoped that its bibliography, informative without being overwhelming, would form a regular feature of the series: the hope is not realised. W. L. F.

Algebraic Geometry and Theta Functions. By A. B. COBLE. [Pp. vii + 282.] (New York: American Mathematical Society Colloquium Publications, Volume X, 1929. Price $3.00).

THE present volume is an amplification of a set of Colloquium Lectures delivered by Coble at Amherst in September 1928. It is an ordered collection of material dealing with problems which lead up to the unfinished discussion of the tritangent planes of a space sextic of genus four. The problem of finding a geometrical configuration, in terms of whose elements each of the 120 tritangent planes can be rationally determined, is as yet unsolved. Coble's endeavour is to put before the researcher solved problems of a similar type, together with an account of the particular theories which have been useful in their solution. The work is admirably carried out and has been aided not a little by the clear type, careful setting, and general arrangement of the book.

The introductory Chapters I and II (pp. 1-112) give an outline account of those parts of Algebraic Geometry (Cremona transformations, residuals, special systems of co-ordinates, etc.) and of generalised theta function theory which are necessary to the understanding of the later chapters.

In the theta-function chapter in particular, many important theorems are merely quoted, a page reference being given in each case to some standard work where the proof may be found. Chapters II, IV, and V set out certain

respectively.

itangent plane

sextic lies on a quadric cone, takes the matter to the point where investigations have failed, and shows the reader possible lines of attack for the solution of the general problem.

The book is not, of course, a textbook: as a work of guidance and reference for those engaged in research along the particular line of combining theta-function theory with algebraic geometry, and as a summary for other geometers of what has been achieved in this direction, the book seems to be an excellent one. There is a list of references, including some eighty numbers, at the end of the volume. One feature of this might be worth the consideration of all writers of monographs: each book or paper is cross-referenced with the section of the text to which it applies. W. L. F.

Solid Space-Algebra: the Systems of Hamilton and Grassmann combined. By Sir RONALD ROSS, K.C.B., K.C.M.G., F.R.S. [Pp. 70.] (London: Harrison & Sons. Price 10s. 6d.)

THE author has set himself the task of presenting a real algebra of space by an attempt to combine the systems of Hamilton and Grassmann. He claims that his conception gives a dimensional significance to the Hamiltonian products and includes the system of Grassmann while rejecting the convention of the nullity of the products of parallel vectors. He warns us that "there is a special danger in multiplication: a writer elects to express something by the juxtaposition of two letters and straightway calls it multiplication. But multiplication (of integers) is continued addition and we really cannot admit that it is merely juxtaposition."

In dealing with the multiplication of two vectors, Hamilton felt himself free to assign meanings to his fundamental products $i\,i$, $i\,j$, etc., and it is the brilliance of his well-known interpretation which makes his work elegant and productive. The author regards this interpretation as a confession of mere symbolism and expresses doubt regarding the soundness of the equation $i\,j = k$, when i, j, and k are vectors. Taking x, y, z as the edges of a unit cube he puts $z/y = i$, $x/z = j$, $y/x = k$, and writes Hamilton's equation in the form $I\,i = x$, $I\,j = y$, $I\,k = z$, where I is a dimensional index and he regards i, j, k as the ratio of vectors.

Grassmann retained the idea of dimensions and introduced two types of products corresponding to scalar and vector products. The author is at variance with Grassmann in his hypothesis that "the products of parallel vectors are zero," but he does not make it clear to which of the products he refers. He regards Grassmann's "combinatory multiplication" as nothing but symbolism or mere juxtaposition.

The author wishes to retain the idea of the multiplication of vectors as a type of continued product and hence regards the product of two vectors as the parallelogram formed by taking the vectors as adjacent sides. In a proposition, called the Triune proposition, he sets out to *prove* that a change in the order of the symbols introduces a change in the sign of their product. This really amounts to attempting to prove the usual convention regarding the sign to be attributed to an area. The author remarks "that the proof of the Triune proposition is not very satisfactory . . . and both Hamilton and Grassmann attempt very little proof of it." It will soon be evident to the reader that this Triune proposition is not a very secure foundation on which to build a solid space-algebra whose first essential, according to the author, is soundness.

JOHN MARSHALL.

The Universe Around Us. By SIR JAMES JEANS, M.A., D.Sc., LL.D., F.R.S. [Pp. x + 352, with 24 plates.] (Cambridge: At the University Press, 1929. Price 12s. 6d. net.)

"THE present book contains a brief account, written in simple language, of the methods and results of modern astronomical research. My ideal, perhaps never wholly attainable, has been that of making the entire book intelligible to readers with no special scientific knowledge." This extract from the preface to Sir James Jeans's latest book gives a true and accurate summary of the pages which follow. In an interesting introduction emphasis is laid on the fact that the study of Astronomy, often called the oldest of the sciences, is still only in its infancy. The first chapter is devoted to an able and concise survey of our present knowledge of the varied bodies which comprise the Universe, ranging from our near neighbours of the Solar System to the extra-galactic nebulæ 140,000,000 light years away. Then the

author turns from the infinitely great to the infinitely small, and describes in considerable detail the latest theories of the construction of the atom. These are found later of importance in the interpretation of astronomical results with reference to the problems of cosmogony and evolution to which the remainder of the book is devoted. What are the stars and what causes them to shine? What is their relation to the various forms of nebulæ? What changes are taking place in the Universe? Is life a rare phenomenon or the reverse? What was the origin of the whole mighty system, how long will it last, and what will be its end? These are some of the questions to which Sir James Jeans applies his comprehensive and accurate knowledge, his choice phraseology and literary style, his well-chosen illustrations and apt metaphors. The emptiness of space is well demonstrated even to the most unimaginative—six specks of dust inside the vastness of Waterloo station represent the extent to which space is crowded with stars; six wasps flying around in the same station represent the extent to which the carbon atom is crowded with electrons; while a single breath from the lungs of a fly is able to fill a large cathedral with air of density equal to that of matter in the outer regions of the nebulæ. But despite his great gifts of clear exposition, the author would doubtless be the first to admit that some parts of his argument must needs mystify the general reader for whom the book is ostensibly intended. The cosmologies of Einstein and De Sitter, however they be explained, cannot but leave such a one dazed and bewildered, while to be told on page 106 that the electron has a definite diameter of 4×10^{-13} cms., and again on page 134 that it takes up the whole of space is apt to engender a similar feeling. But such apparent confusion is unavoidable when an attempt is made to explain modern theoretical physics, and, while no one could have discharged the difficult task better than Sir James Jeans, the general reader will probably enjoy the book not a whit the less because he is very occasionally taken out of his depth. The numerous beautiful plates which illustrate the book greatly enhance its value. Of special interest are the series of Nebular Configurations photographed with the 100-inch telescope of the Mount Wilson Observatory.

R. W. W.

PHYSICS

Experimental Hydrostatics and Mechanics: For School Certificate Students. By E. Nightingale, M.Sc. (Vict.), A.R.C.O. [Pp. xi + 244, with 20 illustrations and 118 diagrams.] (London: G. Bell & Sons, Ltd., 1929. Price 4*s.* 6*d.*)

Here we have a good example of the modern text-book on Elementary Science. Mr. Nightingale has borne in mind, when writing this book, that many boys will read a course in Science up to the age of 16 years (1st School Examination Standard), and then cease to receive instruction. For this reason the text-book for boys up to the age of 16 years must be such as to inculcate the outlook of the man of general education, and not that of the specialist; the books to be read at this stage of the educational life must provide a broad view of the subject, indicating clearly the points of contact which the Science makes with everyday life.

The book under notice is full of interest, because it never gets far away from man and his ordinary activities; but nevertheless it is scientifically sound. Excellent biographical notes are to be found throughout the book, and the number of first-rate pictures and clear line diagrams is surprising. There is a more than adequate supply of problems—both numerical and practical—at the end of each chapter.

Mr. Nightingale thinks that Newton's Laws of Motion, if met at the beginning of a course of Mechanics, are apt to appear meaningless; he has

therefore developed the subject without reference to them, and finally at the end of the book he states the laws as a conclusion of the work, thus completely reversing the traditional course of events. There is much to be said for this novel procedure.

The author is to be congratulated on the selection of subject-matter, and on the method of treatment, and the publishers are to be commended on the form in which they have produced the book.

V. T. Saunders.

The Theory and Practice of Radiology. By B. J. Leggett, M.R.C.S., L.R.C.P., A.M.I.E.E. In three volumes. [Pp.: Vol. I, 250, with 187 figures; Vol. II, 318, with 191 figures; Vol. III, 560, with 534 figures.] (London: Chapman & Hall, 1928. Price 18*s*., 25*s*., and 42*s*. respectively.)

The three volumes under review deal with the physics and electrotechnics of X-rays, practical (medical) radiology being reserved for a fourth volume, which is in preparation. The author confines himself to X-radiology, and radium is not referred to.

Vol. I deals with electricity and magnetism with special reference to the requirements for an understanding of the construction and operation of X-ray apparatus. Insofar as this has been the object of the author, his purpose is well achieved. As a text-book of electricity and magnetism, however, the book is insufficient to meet the requirements of a candidate for a radiological qualification. From this point of view a little amplification of the earlier portion of this volume would add greatly to its value. Vol. II is concerned with the special physics and measurement of X-radiation. In this volume a wealth of important and interesting information is gathered together and presented in a very readable form. This volume should prove of very great value to the Medical Radiologist, for whom the perusal of advanced works and papers on physics—hitherto the only available source of such information—is apt to be a formidable undertaking.

Vol. III deals with X-ray apparatus and technology. There is an excellent chapter on the production and measurement of high vacua, and in another chapter the X-ray tube is very comprehensively dealt with. The principal types of X-ray installation and accessory apparatus are described in detail. The work is well printed, and the illustrations are numerous and good throughout.

W. M. Levitt.

Einführung in die Wellenmechanik. By L. de Broglie. Translated by Rudolf Peierls. [Pp. iv + 219, with 14 figures.] (Leipzig: Akademische Verlagsgesellschaft m.b.H., 1929. Price 11 RM., Bound 13.80 RM.)

The appearance of a treatise on wave mechanics by M. L. de Broglie is an event of importance in the history of the literature of this subject.

The work possesses the character of a text-book, and is written in the lucid manner we have come to associate with French writers.

In the introductory chapter the basic ideas of the theory are stated, and a brief discussion of the various physical interpretations is given. The remarkable difficulties revealed by the new theories are strikingly brought out, and the reader will appreciate at the outset that the last word has by no means been written on this subject.

The early chapters contain accounts of the old mechanical theories, of the basis of wave mechanics and of wave propagation in general. The discussion of the last of these contains a treatment of the representation of

wave trains and groups, and is especially useful and important for a proper understanding of the theory. An account of a number of experiments which support the new views is given, and the Bohr-Heisenberg uncertainty relations are deduced and discussed in some detail.

The important examples of the oscillator, rotator, and the hydrogen atom are worked out towards the end of the book, and in the last chapter we have a short introduction to the Heisenberg matrices.

The book is thus an account of what may be properly called wave mechanics, being concerned principally with the development of the ideas put forward by the author and Schrödinger. It is remarkable for its lucid discussion of the various interpretations of the views which have arisen under the stimulus of the new departures in physics.

H. T. FLINT.

Science and the Unseen World. Swarthmore Lecture by ARTHUR STANLEY EDDINGTON, F.R.S. [Pp. 56; cr. 8vo.] (London: George Allen & Unwin, Ltd. Price, cloth, 2*s*. 6*d*.; paper, 1*s*. 6*d*. net.)

PROF. EDDINGTON'S Swarthmore Lecture to the Society of Friends is extremely difficult to review because, in spite of its title, the science which deals with the unseen world is barely as much as mentioned. Throughout this lecture, as in his *Nature of the Physical World*, the author makes statements about science which are only true of physical or other sciences in their metrical aspect. He limits physics to a well-defined domain, and then because it turns out to be a world of shadows, *i.e.* symbols, and does not include anything to account for inner convictions, he resorts to a background of consciousness for reality, into which we have direct insight by means of mind—" the first and most direct thing in our experience." (Most philosophers would disagree!) "Consciousness can alone determine the validity of its own convictions" (p. 46)—which is true enough. But we discover optical illusions by further and more critical use of the same eyes, and this applies to consciousness also. All the epoch-making researches of the psycho-analysts, which have exposed the extreme unreliability of taking conscious convictions at their face value, are ignored. In fact, if we accept Prof. Eddington's hypothesis that the real "world-stuff" is of the nature of consciousness (or sub-consciousness, more strictly), we are confronted by the statement of the greatest living authority on the unseen world, viz. Prof. Sigmund Freud, that the inner nature of the Unconscious "is just as unknown to us as the reality of the external world, and it is just as imperfectly reported to us through the data of consciousness as is the external world through the indications of our sensory organs."

The fact remains that the data entering consciousness, either from the external world or from the Unconscious, are the only data that are truly "given," and all else involves inference. That in making inferences and in subsequent attempts at their verification we should adopt, equally in the case of the seen and unseen worlds, the careful, critical, and impartial methods of science would seem too obvious to question. But to religious people generally such procedure is unacceptable (for unconscious reasons well known to psycho-analysts), and it is disappointing to find that Prof. Eddington would like to think that scientific method is applicable only to a limited domain of experience. Thus we get such truly amazing statements as (p. 38): "Truth and untruth belong to the realm of significance and values."

The broad scientific outlook on the world seen and unseen is a very recent outcome of evolution, and far later and higher and more difficult to sustain than the religious outlook. As yet it is practically unknown except to a very few (even among scientists), and until it is, such lectures as this will continue to meet with an enthusiastic reception.

There are, as we have now learned to expect from Prof. Eddington, many passages of great charm and brilliance, but it must be added that there are also many of very great obscurity. All of which amounts to saying that, as a religious address, it is first-rate.

G. B. BROWN.

CHEMISTRY

Volumetric Analysis. By I. M. KOLTHOFF and H. MENZEL. Translated by N. H. FURMAN. Revised and Enlarged. Vol. II, Practical Volumetric Analysis. [Pp. xiv + 552.] (London: Chapman & Hall, 1929. Price 25*s*. net.)

MOST chemists will feel inclined to agree with Prof. Kolthoff's remarks in his Preface that volumetric analysis is "the most interesting and versatile branch, on the whole, of analytical chemistry," and a careful and authoritative compilation, such as the present work, is of value to all workers on chemical problems, whether in the university or the works laboratory.

This volume is intended especially as a sequel to Vol. I, *Theoretical Principles of Volumetric Analysis*, published some time ago, but it will be almost equally of value whether used in conjunction with that volume or not. The author has not attempted the impossible task of including all methods of titration for all substances, but has limited himself to those that have an actual practical significance, and that have shown themselves to be reliable, without however going exhaustively into the special methods of applied chemistry which are dealt with in special works of reference.

The essential object kept in view has been to examine and explain the fundamentals of the subject, such as the limits of accuracy of the usual measuring vessels, the purification and tests of purity of the standards, the influence of trace of impurities, and so on. The translator remarks that the distinguished author has tested practically every method of consequence, and has contributed much valuable new information, so that the work may be regarded as an extension of and addition to Beckurt's *Methoden der Massanalyse*, published in 1913.

The work is arranged in the usual manner, *e.g.* Calibration of measuring vessels, Preparation of standard solutions, followed by chapters dealing in detail with Alkalimetry, Acidimetry, Displacement reactions, Quantitative precipitations, Oxidations, Reduction methods, Iodometry, Titrations with potassium iodate, bromate, and bichromate, ceric sulphate, and titanous salts.

It will be seen therefore that the subject is very fully and adequately treated, and the book itself should serve as a "Standard Solution" for many of the problems met with almost daily in the analytical laboratory, and authors, translator, publishers, and printers merit the warm thanks of those—and their number is legion—to whom the book will be of value.

F. A. MASON.

Practical Chemistry for Senior Forms. By ALWYN PICKLES, M.Sc. [Pp. xv + 224.] (London: G. Bell & Sons, Ltd., 1929. Price 4*s*. 6*d*.)

GOOD books on practical chemistry suitable for candidates preparing for the Higher Certificate and University Scholarships are comparatively rare, and most teachers are in the habit of getting their own course together from various sources. Mr. Pickles has constructed an unusually attractive course, and we think he was quite right to publish it. The selection of experiments has been carefully made, and the instructions are such as the young student will find most clear and helpful. The best part of the book is the first, which is composed of inorganic preparations, estimations, and problems. The second part consists of exercises in physical chemistry;

these are satisfactory as far as they go, but we should like to have seen the net cast wider and a little more originality shown—physical chemistry is not made nearly as attractive as it should be, in schools. The third and fourth parts deal with organic preparations (where we notice *Friedel-Craft* in error for Friedel-Crafts), while Part V is devoted to that useful but (in schools) very much-neglected operation, gas analysis. Altogether, the book is certainly one to buy.

H.

A School Certificate Chemistry. By G. H. J. ADLAM, M.A., B.Sc. [Pp. x + 334.] (London: John Murray, 1929. Price 4*s.* 6*d.*)

As one of the leading figures in the Science Masters' Association, Mr. Adlam is in an excellent position to keep his finger on the pulse of school chemistry. It is clear from his preface that he believes the general tendency is to develop a peculiar "schoolmaster's chemistry," inclined to get more and more remote from the real thing. We are not altogether sure that this tendency is very marked as yet, for our own experience is that the Association is, as a whole, extremely keen to keep abreast of modern work and views. Still, Mr. Adlam has done useful service in writing this little book, in which School Certificate chemistry is presented with clarity, sound judgment, and a very profound insight into the capacity of the human young. We recommend all science teachers to examine a copy; many will adopt it, and those who do not will at least derive a not inconsiderable benefit from reading it. The publishers have done their part well.

H.

Everyday Chemistry. By PROF. J. R. PARTINGTON, M.B.E., D.Sc. [Pp. viii + 667.) (London: Macmillan & Co., 1929. Price 7*s.* 6*d.*)

HERE is an admirable book, which none but Prof. Partington could have written. Packed full of facts—all relevant and all interesting—it is nevertheless so skilfully written that the trees never obscure the view of the wood. The book as a whole is divided into three parts, which may be obtained separately (though to do so would be a pity). The first describes Chemical History and Theory from the earliest times to the formulation of the Periodic Law and the theories of atomic structure. This is, in our opinion, the best section of the book; many professional historians of chemistry could learn a great deal from it, and Prof. Partington's shrewd insight into chemical philosophy makes his treatment of the subject particularly valuable. The second part is entitled *Some Non-metallic Elements and their Important Compounds*; it is concerned with elementary chemistry such as forms the bulk of matriculation and school certificate courses. It seems to us that, while the author is as sound and as illuminating here as in the first part, he finds some difficulty in bringing himself down to the level of the average boy or girl, who has some three periods a week in which to study chemistry. The bright pupils would benefit enormously, and a capable teacher will find Prof. Partington a most stimulating guide; but the reviewer himself would be delighted to know that the rank and file of lower Fifth Forms could appreciate the book at its true worth. The third part gives a fascinating account of metals and some common organic compounds. It makes excellent reading, all the more enjoyable on account of the good and numerous illustrations. We congratulate the author and publishers on having made a first-rate contribution to elementary science teaching in this country; the author for drawing so freely and so wisely on his own wide stock of knowledge, and the publishers for presenting us, for 7*s.* 6*d.*, with what is almost comparable in bulk to one of the popular "omnibus" volumes.

H.

Das kolloide Silber. By J. Voigt. [Pp. 165.] (Leipzig: Akademische Verlagsgesellschaft m.b.H., 1929. Price 10M. Bound 12M.)

The present work forms Vol. VIII of *Kolloidforschung in Einzeldarstellungen*, the series of monographs edited by the late Prof. R. Zsigmondy. The subject, though it has never loomed as large as colloidal gold, has a respectable and interesting history: Faraday investigated silver sols and films, Carey Lea's "allotropic silver" created some stir in its time, and the first dialysis of a metal sol was carried out by Muthmann on the product of reduction of silver citrate by hydrogen. It is gratifying to find that Muthmann's paper, which the author thinks "has not been appreciated nearly sufficiently," has been republished in English in 1925 (in *The Foundations of Colloid Chemistry*).

The reason why silver sols have not played a part at all comparable to that of gold sols is the difficulty of obtaining preparations with reproducible characteristics, and especially with uniform particles. In addition to this a great number of investigations have been carried out on protected sols, so that the results are difficult to interpret. Methods for producing with reasonable regularity sols containing particles of uniform size, or, at any rate, of uniform colour in the ultramicroscope have only recently been developed, largely by the author. All this is clearly and fully set forth in the chapters on methods of preparation, protection, physical properties, and coagulation of silver sols.

Colloidal silver was the first inorganic preparation to find extensive application in medicine, and the author accordingly devotes three chapters, headed "Medical and Biological," "Deleterious Effects," and "Pharmacological Examination of Silver Sols" to this aspect of the subject. It is outside the present reviewer's purview, but he cannot help feeling that this incompetence has its compensations, when he sees it stated by the author that close on 1,500 papers have been devoted to colloidal silver in German journals alone! Many of these are said not to give either the silver content or the nature and concentration of the protective colloid, so that conclusions drawn from them seem to rest on an insecure basis.

The book concludes with a bibliography covering 29 pages. It forms a worthy companion to the two volumes devoted to colloidal gold and its clinical applications.

E. H.

A Concise Summary of Elementary Organic Chemistry. By F. H. Constable, M.A., D.Sc., Ph.D., F.I.C. [Pp. xii + 149.] (London: Methuen & Co., Ltd., 1929. Price 4s. 6d.)

An idea of the purpose of this book may be given by the following quotation from the Preface: "The average student of organic chemistry has a number of excellent textbooks in which he can read the many interesting facts concerning the compounds of carbon; but when an examination has to be faced these facts are required in a much more concise form. The present book provides a concise summary of the elements of organic chemistry suitable for candidates taking examinations at an intermediate stage of their full university course in chemistry. . . ."

Such a summary as this takes away from the study of organic chemistry its fascination and interest, and leaves a mass of cold facts and equations. The book will certainly be of use to students who have to revise the subject for an examination, but it is to be hoped that it will be used as a supplement to, and not in place of, one of the ordinary textbooks of organic chemistry.

J. N. E. D.

Organic Syntheses. Vol. IX. An annual publication of satisfactory methods for the preparation of organic chemicals. JAMES B. CONANT, Editor-in-Chief. [Pp. vi + 108]. (New York: John Wiley & Sons; London: Chapman & Hall, 1929. Price 8*s*. 6*d*.)

THE scope of this book is indicated by the sub-title. The preparation of the following compounds is described: acid ammonium *o*-sulfobenzoate, *dl*-alanine, ammonium salt of aurin tricarboxylic acid, anisole, benzoyl piperidine, *p*-bromophenacyl bromide, *o*-bromotoluene, *n*-butyl carbamate-*n*-butyl *p*-toluenesulfonate, *n*-butyryl chloride, *o*-chlorobenzoyl chloride, cyanoacetamide, ethyl cinnamate, hydrocinnamic acid, iodobenzene, levulinic acid, *l*-menthone, mercury diphenyl, methylene bromide, monochloromethyl ether, β-naphthol phenylaminomethane, *o*-nitroaniline, nitrostyrene, pentamethylene bromide, γ-phenoxypropyl bromide, phloroglucinol, pyrrole *o*-sulfobenzoic anhydride, *ac*-tetrahydro-β-naphthylamine.

This volume maintains the high standard set by previous ones, and the methods given may be recommended when fairly large quantities of material are required. In order to ensure that the instructions can be readily followed, and that they will give the stated yield, each description is checked by independent chemists before publication. The subject index covers the nine volumes of the series.

J. N. E. D.

An Introduction to Modern Organic Chemistry. By L. A. COLES, B.Sc., A.I.C. [Pp. xv + 452.] (London: Longmans, Green & Co., 1929. Price 7*s*. 6*d*.)

THIS book should prove useful for schools; it is divided into three parts: I. An introductory study of some simple compounds; II. Aliphatic compounds; III. Aromatic compounds. A chapter on the development of organic chemistry is put at the end. There are a number of questions at the end of the chapters and the necessary details are given for the preparation of many of the compounds described,

The use of the word "modern" in the title of this book raises the question of the amount of recent work and theories dealing with the subject which should be found in an introductory work of this type. The present book is not remarkably up-to-date; for example: the old formula is given for urea (p. 232); the oxide formula given for glucose (p. 283) contains the five-membered ring, while more recent work indicates the presence of the six-membered ring; in the section dealing with isomeric oximes (p. 383), while the *syn* and *anti* forms are interchanged in the text, the old idea of the *cis* elimination of water is referred to, and no mention is made of recent work indicating *trans* elimination of acetic acid. In connection with valency and structure also, the electron might well be introduced at the first stage in the study of organic chemistry.

J. N. E. D.

Soap Films. By A. S. C. LAWRENCE. [Pp. xi + 141, with plates and figures.] (London: G. Bell & Sons, 1929. Price 12*s*. 6*d*.)

A BOOK on soap films emanating from what may be termed their ancestral home is sure to be received with interest and expectation by all. As stated by Sir William Bragg in the Foreword, Mr. Lawrence assisted Sir James Dewar for many years in his researches on soap films, and gained a remarkable knowledge of their technique, and in this book he presents an account of a subject of which, during the last few years, our knowledge has rapidly increased.

A word of warning should be given to our readers that some errors have unfortunately crept in. These are mainly in the first chapter, *i.e.* the

section dealing with the more exact physical definitions, but fortunately most are not of an obscure nature, *e.g.* surface tension cannot be equated to a pressure, nor can it be supposed to act normally to the plane of a plane film, whilst generally it is expressed in dynes per centimetre. After having read Chapter I rather cautiously the rest follows easily.

In Chapter II the author deals with the preparation of soap and the relative surface tensions of the sodium salts of the fatty acid series are given for various concentrations and temperatures, whilst soaps generally are considered from a chemical standpoint. Chapter III introduces the reader to the soap film and bubble in existence, and of special interest is the kinematographic record of the penetration of a bubble by a projectile, during which photographs were taken at the rate of 3,000/second. A study of the coalescence of several bubbles introduces the boundary channel investigated by Willard Gibbs, the existence of which is an important factor in the life of a soap film.

After a description of the methods of preparation and maintenance of the film, as practised at the Royal Institution, and the wave theory of the colours of thin films, the chapter concludes with a description of the general ageing of films and the abnormal development of the critical black fall. Mention of stratified films leads naturally to the consideration in Chapter IV of the black film, in connection with which it is interesting to recall the early methods of estimating the film thickness.

Chapter V is concerned with various physical, chemical, and colloid properties of soap films, amongst which is mentioned the work of Hatschek and Freundlich on viscosity. They both employed the Couette apparatus, in which the angular deflection of the suspended inner cylinder is theoretically proportional to the product of the angular velocity and the viscosity.

Everyone will be interested in Chapter VI, in which the scale of perception has been altered so that atoms and soap molecules may be visualised. A brief summary of some of the results of X-ray analysis is given, to which is added the complementary results of monomolecular layer measurements of Langmuir and Adam, which actually give the surface area occupied by a number of molecules; if the molecules are assumed to be perpendicular to the layer, this corresponds to the right sectional area and a further assumption as to density (which, by the way, may not be the liquid density but rather that for the crystalline state) enables the length to be calculated.

The composition and structure of the soap films forms the subject of the last chapter, which includes, also, a consideration of absorption in soap solutions and a critical discussion of the formation and theories of the constitution of the black film.

I have delayed until the end the expression of a feeling which arises as each of the plates is examined, viz. the praise which the publishers deserved; the plates are extremely good, and the whole publication maintains the high standard to which Messrs. Bell have accustomed us.

R. E. Gibbs.

Chemistry in the Home. By J. B. Firth, D.Sc., M.S.Sc., F.I.C., Senior Lecturer in Chemistry at University College, Nottingham. [Pp. 246, with 17 illustrations]. (London: Constable & Co., Ltd., 1929. Price 5s. net).

If a traveller gets tired of the type of literature usually favoured by those who journey by train, and wants a "popular" scientific book that will not impose too great a tax on his mental equipment, here is the very thing for him, not only in interest, but in size. Although the work is called "Chemistry in the Home," it contains more than a sprinkling of physics, but all is very

readable. One cannot but be struck by the numerous applications of fundamental principles to common daily life.

The book is divided into two parts, the first including such topics as the atmosphere, ventilation, domestic water supply, heating, fuels, lighting, soap, disinfectants, textile fabrics, etc., and the second the dietary, the occurrence, composition, preparation, and relative food values of some of the commoner foods.

It is necessary to criticise at least two points adversely. After discussing the heating effects of the sun's rays on metal objects, the author declares that, among the distinguishing features of radiation, is "the instantaneous character of the process." The context seems to make this refer to the radiation received from the sun, but this must be an unfortunate piece of wording, for the author would surely not apply the term "instantaneous" to the eight minutes actually required for the journey.

In another chapter we read of the work of "Lord Rayleigh and Professor Ramsey" on the inert gases. The late Professor would be identified more easily by the general public if he were referred to as "Sir William," by which title he was subsequently known. We admit, however, that the matter may be open to argument, and that there may be some excuse for this; but there can certainly be none for using an *e* in the final syllable of Ramsay's name.

H. S. T.

The Biochemistry of the Amino Acids. By H. H. Mitchell and T. S. Hamilton. An American Chemical Society Monograph. [Pp. 619.] (New York: The Chemical Catalog Company. 1929. Price $9.50.)

This publication, in common with others of its series, aims at "an appraisal of the extent of knowledge in the fields considered, as well as of its limitations." Historical treatment is reduced to a minimum: critical discussion of present positions is the object of the authors. Chapters 1–3, which are concerned with purely chemical matters of isolation, estimation, etc., include useful tables of chemical and physical properties. A chapter on proteolysis is followed by two dealing with the general aspects of amino-acid metabolism, and the last four chapters are devoted to special aspects of the physiology of amino acids, such as the origin and function of creatine in the animal body; the constitution and function of glutathione and of thyroxin; recent studies of the "specific dynamic action" of amino acids; and the nutritive value of proteins in the diet of man and other animals. A detailed review of the book, ranging as it does from the synthesis of methionine to the nutritive value of pork cracklings, is hardly feasible, but a word of praise is due for the easy literary style and the good perspective throughout the book, which make interesting reading out of what might easily have become a congested mass of information. In view of the generous space rightly given to the physiological significance of creatine it would have, perhaps, been wise to devote a few pages to its chemistry. The structure of creatine is not known with certainty, but it must be known before physiological theories can be trusted completely.

Every important reference is cited at the foot of the page where it first occurs, and all the citations are numbered in order of reference for each chapter—making them readily available. The index occupies thirty pages.

P. Eggleton.

Chart illustrating the History of Biochemistry and Physiology. 6½ ft. by 3 ft. Privately published. Obtainable from Dr. J. Needham, Biochemical Laboratory, Tennis Court Road, Cambridge, England.

This chart, which has been reproduced from the original by a lithographic process, gives a two-dimensional account of the historical development of

these and cognate sciences. The arrangement is that of a graph, the vertical scale being in years and running from 1450 to 1900, with ten-year intervals marked off by horizontal lines. The lives of individual investigators are represented by continuous vertical lines beginning at the date of birth and ending at the date of death. Beside each such line is the name of the investigator in capital letters with special signs indicating the University in which he worked. The vertical divisions group the investigators into anatomists, physiologists, chemists, biochemists, zoologists, and philosophers. Diagonal wavy or dotted lines indicate relationships, controversies, succession in professorial chairs, and in this way also the main streams of intellectual influence are shown. At the date of publication of an important book or memoir a thin horizontal line leaves the life-line of the author, leading to the title and a short description of the book. In addition to these descriptions there are quotations and notes interspersed throughout. To give a chronological perspective, the lives of men other than biologists are shown in a separate column—Galileo, Cervantes, Montaigne, for example. Certain wars are represented, the founding of certain associations such as the Royal Society, and the beginnings of scientific journals are noted. Before 1450 there is no regular time scale, but the achievements of classical antiquity, the Hellenistic age, and mediæval times are given brief reference. The chart may be said to give a wide and detailed survey of the history of biochemistry and physiology, but it has considerable interest for biologists in general, for before 1800 the fields of study were not clearly differentiated, and up to that date the chart is practically a history of general biology.

P. Eggleton.

GEOLOGY

Ore Deposits of Magmatic Origin: Their Genesis and Natural Classification. By Prof. Paul Niggli. (Translated by Dr. H. C. Boydell from the original German edition, revised and supplemented by Prof. Niggli and Dr. R. L. Parker, Zurich. [Cloth, octavo, 104 pages.] (London: Thomas Murby & Co. Price 9*s*. 6*d*.)

The old saying that "Ore is where you find it" has long become, to the mining geologist, the question "Why is ore where it is?" and a fundamental principle in the scientific search for unknown ore deposits is based on the genetic relationship of ore bodies to surrounding rocks.

There has been considerable progress during the last two decades in the study of the formerly obscure processes involved in the deposition of ore minerals, but there still remains a great deal to be learnt. By making *Ore Deposits of Magmatic Origin* by Paul Niggli, Professor of Mineralogy and Petrology, Zurich, available to English-speaking mining geologists, the translator, Dr. H. C. Boydell, formerly research associate at the Massachusetts Institute of Technology and now consulting mining engineer, and his helpers, have rendered a definite service to their fellow-workers in the application of geological principles to the mining industry.

Niggli, a well-known petrologist, showed in 1918 that ore deposition of magmatic origin could be considered as a phenomenon that *necessarily* accompanied the consolidation of the magma under certain conditions, and he then made his first attempt at a systematic treatment of the physico-chemical relations of magmatic solutions. In the book now under review he reclassifies ore deposits of magmatic origin on a rigidly genetic basis, and endeavours to show that the processes involved in the formation of ore minerals are those which also confront the petrologist in the study of igneous rocks.

cal chemistry of ore-generating magmas and in particular to the effects of varying temperature and

pressure, to the normal changes in composition of residual solutions, and the paragenesis of elements; Chapter II deals with the physical and geological factors and the chemical and mineralogical characteristics of ore deposits, leading to Niggli's new classification; and in his third and last chapter the author discusses the conditions of association of such deposits with respect to the earth's major structural units, and to petrological provinces.

The ultimate origin of metals in the magma has, wisely, not been attempted in this volume, but the author with marked success has accomplished his main object of showing that the formation of magmatic ore deposits is a process intimately related to, and in fact inseparable from, petrogenesis and hence that the study of this type of ore body is part of the wider problem of petrology.

This is a very inspiring and suggestive book that will be heartily welcomed by mining geologists, who have long deplored the fact that so many eminent mineralogists and petrologists have hitherto confined their researches mainly to the genesis of minerals of purely scientific interest.

WILLIAM R. JONES.

AGRICULTURE

Minerals in Pastures and their Relation to Animal Nutrition. By J. B. ORR, M.D., D.Sc. [Pp. xvi + 150.] (London: H. K. Lewis, 136 Gower Street, W.C.1. Price 10s. 6d. net.)

THE importance of the pastures of the British Empire can be gauged by McDougall's estimation that the value of the grassland products annually consumed in Britain is roughly £426,000,000, of which more than half is imported. Naturally enough, such a potential source of wealth has attracted much attention from research workers, chiefly botanists and soil chemists, who have succeeded of late years in effecting great improvements in cultivated pastures. It has always been recognised that the feeding value of grassland varies considerably, and it is now known that the chemical constitution of herbage has much to do with its nutritive value. More recently, however, it has become evident that foodstuffs of good composition as regards protein and fat-forming substances may be deficient in other bodies which are essential to animal nutrition, especially inorganic salts and certain organic compounds which are only required in small amount, but nevertheless must be present. Various types of malnutrition are now recognised as being in reality deficiency diseases caused by the lack of a sufficient supply of these essential small quantities. Such elements as phosphorus, calcium, iodine, etc., may, in certain soils, be either present in too small quantities or in forms unavailable for use by plants, with the result that the herbage growing thereon is liable to affect the health of animals to which it is fed. A considerable amount of work has been done in various quarters along these lines, but the literature is very scattered, and Dr. Orr has done good service to workers in this field by gathering together the available information.

In work on deficiency diseases in grazing animals most attention has been directed to the five elements calcium, phosphorus, sodium, potassium, and chlorine, and a considerable mass of analytical data is available. It is apparent that low percentage of any one element tends to be correlated with low percentages in others, as the small available quantity of any deficient element acts as a limiting factor for the utilisation of the rest. It is gradually becoming recognised that other ash constituents which are present in very small amounts are of equal value in animal nutrition, and that deficiency diseases may equally well be the result of their absence. Iodine, iron, sulphur, manganese, and various other elements may come in this category, and the list will probably be extended as time goes on.

The mineral content of pastures is influenced by various factors, the chief being the species of plants composing the herbage and the particular stage of growth, the climatic conditions and the nature of the soil. Where irrigation is necessary, the soil composition may gradually be altered by the addition of soil constituents, which eventually affect the mineral composition of the plant. There is evidence to show that deficiency diseases are most likely to occur where the animals are fed on old pastures to which no return in the form of artificial fertilisers has been made. Another predisposing cause is in attempts to improve native breeds of cattle by crossing with imported stock without, at the same time, improving the quality of the grazing pastures. Such evidence is forthcoming from every continent, and malnutrition is now regarded as the cause of a great variety of ills, including pica, brittle bone, stiff sickness (South Africa), goitre, coastal disease (Australia), bush sickness (New Zealand), creeps (United States), joint-ill and general debility.

In certain cases of deficiency direct administration of mineral salts is beneficial, salt, phosphorus, and calcium (as bone meal) being specially useful in this respect. Indirect administration can be effected by raising the mineral content of the pastures by the application of artificial fertilisers, which results in increasing the carrying capacity of the grassland, and also in increasing the rate of production of the animals grazing thereon.

From the evidence collected by Dr. Orr it is clear that the improvement of stock, both in quality and quantity, is intimately bound up with improvement in grazing lands, not only as regards the quantity of herbage produced, but also with respect to the mineral content of the grass. Deficiency in certain elements which may only be present in small quantities may reduce the feeding value of the herbage as much as deficiency in proteins, carbohydrates, and other substances more usually recognised as essential to nutrition. Experimental work along these lines is necessarily a long process, as it deals with slow-growing animals, but the indications are that many of the hypotheses put forward at the present time will be proved to be actual facts in the course of the next few years.

W. E. B.

ZOOLOGY

Man and Animals in the New Hebrides. By JOHN R. BAKER, M.A., D.Phil. [Pp. xiv + 200, with 43 figures.] (London: George Routledge & Sons, Ltd., 1929. Price 12*s*. 6*d*.)

THE author of the present work spent five months in 1922 and nearly a year in 1927 in the New Hebrides under the auspices of the Percy Sladen Trust. He and others have reported upon the collections and observations then made in a number of technical journals. In this book he brings together much of this material, and rounds it off in a manner more suitable for the non-scientific reader. This he has done successfully, and the result makes interesting reading. Among chapters dealing with general topics are two concerned with the more specific problems of "Depopulation" and "Intersexual Pigs."

The question of depopulation is one to which he has obviously given thought and which is sure to arouse interest. Other writers have tackled the same problem in a less critical and scientific spirit, and some of their theories should receive their death-blow as the result of the present observations. Whether or not his own theories will stand the test of time is yet to be shown. Considerable stress is laid on the sex ratio, which in Sakau reaches the astonishing figures of 159 males to 100 females. This may be a contributing cause, and, if so, it opens up the still more interesting question as to how such a ratio came into existence; it obviously cannot have been

operative since the first settlement of the islands, or there would be no population left. It is held to be a very important factor, and on page 69 graphs are given which at first sight appear to put the matter beyond all doubt. Closer investigation shows that the curves are for entirely imaginary conditions, based not upon observations, but upon suppositions. If one of the conditions or suppositions be not accepted, as we fear will happen in the present instance, the curves prove absolutely nothing. This method of restating theories in a spurious mathematical form will carry no weight with the thinking person to whom the book is undoubtedly addressed. We are sorry to have to call attention to this, but the remainder of the chapter is so interesting that it is a pity it should have been introduced.

The occurrence of hermaphroditism is comparatively rare in the mammals, and it is astonishing to learn that "intersex" pigs are not uncommon in the New Hebrides. Indeed, the author himself "dissected nine, and made notes on the external genitalia of ninety-one," and even then was hindered by the important part these animals play in the social life of the natives. It should be noted, however, that the term "intersex" is used for an animal that always possesses testes and tusks, but never ovary, Fallopian tube, uterus, nor vagina, so that the "intersexuality" consists, when present, to its most marked degree, in a simulation of the female external genital aperture. It will readily be seen that such animals present a number of interesting biological problems which the author deals with in an attractive manner.

C. H. O'D.

Homiothermism. The Origin of Warm-Blooded Vertebrates. By A. S. Pearse and F. G. Hall. [Pp. ix + 119, with 3 figures and 2 tables.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, 1928. Price 10*s*. net.)

It is a good thing when a book begins well and it is pleasant for the reviewer, for he has then something to comment on favourably at the outset. The book in question has this advantage, for the preface is excellent; it is well written, brief, and to the point. Unfortunately the rest of the book does not maintain the same high standard. It contains some platitudes and much that is not relevant. In tracing the origin of warm-blooded vertebrates there is no necessity, for instance, to go back quite so far in the history of the earth as the hypothetical nebula from which the solar system originated. Then a passage such as the following, though relevant, is hardly necessary in a scientific book: "Man alone among animals has become independent of temperature fluctuations by inventing clothes. In cool countries he endeavours to protect himself by keeping his skin covered. Only about 20 per cent. of his skin is normally exposed to air. He varies the amount of surface exposed and the thickness of this artificial covering with the climatic changes." The book contains little that the average first-year student of biology does not know. It might be useful, however, to people unacquainted with biology, who, for some obscure reason, wish to learn about the evolution of homiothermism in particular.

F. W. R. B.

Growth. By W. J. Robbins, S. Brody, A. G. Hogan, C. M. Jackson, and C. W. Greene. [Pp. xiii + 189, with frontispiece and 83 figures.] (New Haven: Yale University Press; London: Humphrey Milford, 1928. Price 14*s*. net.)

This little book consists of a series of five popular lectures on various aspects of growth by different members of the staff of the University of Missouri. It is, of course, impossible in the space of five lectures to do more than

outline the major aspects of a subject of such importance. The field covered is, however, remarkably large, and there is little overlapping of their respective fields by the authors. Each of the lectures is well constructed, well written, and well illustrated. The first introduces the subject by defining the nature of growth. The three succeeding lectures discuss the statistical, nutritional, and morphological aspects respectively. The final lecture deals with physiological factors, normal and pathological. It is perhaps a pity that the light thrown on various aspects of growth in recent times by the study of tissue culture is scarcely mentioned. This important mode of attack on the subject might well have formed the subject of a sixth lecture. The book as it is provides interesting and easy reading, and the production and appearance are attractive.

F. W. R. B.

Étude critique du Transformisme. By F. Carrel. [Pp. 84.] (Paris: Vigot Frères; Genève: Georg & Co., 1929. Price 15 frs.)

The central idea of this volume is that the evolution of species is unproven, and that we too readily forget that fact and overlook the difficulties involved in the hypothesis. We might on the other hand argue that the author too slightly glances at the difficulties without considering the other side of the question. In five pages only he discusses the weakness of palæontological evidence! he combats the idea that there is any real parallelism between individual and ancestral development—he may be right—and he suggests that though each species can vary within restricted limits as a result of environmental influences, nevertheless the only true type forms are those which originally were inherent in the protoplasm from which they developed. The particular form in each case he supposes to be determined by some physico-chemical mechanism operating in a colloidal protoplasm much as the characteristic form of a crystal develops in its particular crystalloid substance; in short, man was always man, and has not been evolved from any lower form.

It requires courage to put forth such a proposition in a few pages to-day, for the reader is apt to be impatient when very momentous questions are slightly treated, but the volume has some interest from the thoughts it arouses; its proposition recalls the names of some great men to our minds; we are reminded, for example, of Linnæus—"Just so many species are to be reckoned as there were forms created at the beginning"—it is pleasant to be reminded of so ardent a worker, though we may be convinced that if Linnæus had the facts before him which have accumulated since his day, he would not be found supporting the author's views.

β1.

A Laboratory Manual for Comparative Anatomy. By Malcolm E. Little and Rudolf T. Kempton. [Pp. xix + 286, with 19 plates.] (London: Macmillan & Co., 1928. Price 10s.)

The most striking feature in the present book is the arrangement of the subject-matter which is best explained by a quotation from the preface: "The authors have taken a compromise course between the 'systemic' and the 'systematic' treatment of vertebrate anatomy. There is no need to defend the modern method of teaching by systems, though there are two important objections. First, in large classes, the physical problem of handling the numerous specimens needed at one time; and second, the pedagogic problem of giving the student an idea of the animal as a group of interrelated systems. The attempt is made to overcome these objections by teaching the dogfish first as a type, and then proceeding to the other vertebrates in a systemic manner." ". . . Before studying the comparative

structure of the organs, the student has a thorough knowledge of the vertebrate as a functional unit, and while comparing one system thinks in terms of its relation to the others."

We think, after having tried both methods, although not on so large a scale as the authors, that the older method is the better, although the one proposed does lend itself rather better to certain types of examination tests. There is also another aspect not brought out in the above statements, and that is that, while the determination of the relationships of the various systems within one animal is a matter of accurate investigation and observation of facts, the comparisons of systems in a series of different animals involve not only this but a series of inferences, deductions, and in some cases the acceptance of certain hypotheses. Thus there is always the danger of the elementary student confusing facts with inferences and hypotheses, a training that does not lead to clarity of thought. The brachial artery in the frog is not so called because of its resemblance to a similarly named vessel in the diagram of the arterial system in a rabbit, but because of its distribution in the frog itself. Both sides of the matter need to be presented to the student, but they require different methods of presentation. The practical course is designed especially for the observation or "discovery" of facts, and the lecture course is the place where the inferences can be indicated, comparisons drawn, and the whole rounded out into a complete system of exposition. It is to be noted that this objection is met to a certain extent by treating the dogfish first as a whole, but at the same time it is not entirely overcome.

Apart from this fundamental point the book is good. The usual information about a series of animals chosen from teleosts, necturus, turtle, alligator, pigeon, rabbit, cat, is well and clearly put, save that in certain respects, *e.g.* the blood vascular system of Squalus, it needs revision. The plates given are outline drawings on which the student is to superpose details from his own observations.

C. H. O'D.

Laboratory and Field Ecology: The Responses of Animals as Indicators of Correct Working Methods. By Victor E. Shelford, Professor of Zoology, University of Illinois. [Pp. xii + 608, with 219 text-figures.] (London: Baillière, Tindall & Cox, 1929. Price 45*s.* net.)

From the biological point of view North America presents several noticeable differences from Western Europe. In the latter most of the larger areas of vegetation have reached a climax condition, whereas in the former many are in a state of flux. The hand of man also is bringing about marked and rapid alterations on a large scale. The changes in the vegetation are naturally accompanied by changes in the fauna. Where changes are thrust forward in such an obvious and striking manner, it is only to be expected that they should arouse considerable interest. In the United States the study of Ecology is widely followed, and the author of the present volume is one of its most eminent exponents. Prof. Shelford has contributed a number of valuable memoirs in this field and also a very useful book on *Animal Communities in Temperate North America*, which, unfortunately, is now difficult to obtain. The appearance of the present book has been awaited with considerable interest, and the reader will not be disappointed either in the subject-matter or its presentation.

The wide experience of the author in the field has given him a good grasp of the whole subject, and also enabled him to indicate the main lines of investigation now in progress and their interrelations. The first six chapters are devoted mainly, but not exclusively, to more or less general topics, so as to provide "a setting for its problems," and the succeeding

chapters also elaborate the general aspects of different types of problems. For the purpose of grasping this theoretical side of field ecology the book will well repay study, although this constitutes only a part of the whole work. The author sums up that "Communities are adjusted to climates; climax communities are climatic," but without in any way belittling the importance of climate one might venture the criticism that the statement as it stands omits or obscures one fundamental aspect of the subject, namely, the geological nature of the area which is a variable independent of climate. Assuming an identity of climatic conditions, it is obvious that the climax communities upon, say, a pre-Cambrian peneplain, sand dunes, alluvial clay, or limestone rocks, must be very different.

It is in the other portions, where the ways and means of observing and experimenting and of recording the observations and results with scientific accuracy are considered, that one realizes that the author is not only a theoretical ecologist but also a master of the practical side with a knowledge based on wide personal experience.

After dealing with the relations of animals to each of various factors in the environment there follow very instructive chapters upon such topics as: Control and Measurement of Temperature, of Moisture, of Air Pressure, of Evaporation, of Light, etc. These chapters will be of great assistance to anyone who wishes to conduct investigations along any of these lines. The various instruments used are described and their value and limitations pointed out, the methods of using them are discussed, and the directions and cautions given bespeak much practical experience. These and other topics are considered not only with a view to field study, but the controls in particular are aimed at simulating climatic conditions within the laboratory, and thereby conducting experiments in which the various factors are under the control of the investigator.

An appendix is provided which gives more explicit directions in the keeping of records and the setting up of typical equipments. In this connection also the question of cost is considered, and lists of the firms supplying various types of equipment are given. As might be expected in a book written in the United States, practically all the firms mentioned are in that country. This is obviously useful, since the book is assured of a wide sale there, and is not a serious drawback to scientists in Europe, since the descriptions of the instruments and their workings are so full that they or others giving similar results can easily be duplicated elsewhere.

Lastly, a bibliography of over 1,000 references is given, and this in itself is a valuable addition. This is admittedly incomplete, and is, of course, the easiest of criticism on the ground of omissions, but in view of the great utility of the book as a whole this is quite a minor point. The author has obviously read very widely, and so it is rather strange to find on page 60, where he deplores the fact that no satisfactory surveys of dams and canals and their effects on the biota have been made, that he makes no reference to the Suez Canal Expedition, nor to the work now being done in the Zuider Zee.

The book is well printed and illustrated and provided with useful diagraphs, and formulæ. The author is to be congratulated on producing so interesting, inspiring, and useful a work which can be commended to all interested in ecology or in laboratory experimentation.

C. H. O'D.

An Introduction to the Study of Bird Behaviour. By H. Eliot Howard. [Pp. xiii + 135, with 11 plates and 6 plans.] (Cambridge: At the University Press, 1929. Price £2 2s. net.)

The author of *Territory in Bird Life* has enhanced his reputation with this volume. Mr. Eliot Howard combines the power of accurate, sustained, and detailed observation with the faculty of seeing the wider reference of

what he observes and the result is that his theories are built on the bed-rock of carefully recorded facts. In this work we have Mr. Howard at his best, and the result is an important and stimulating contribution to ornithology.

When biologists realised the errors into which anthropomorphism could lead them and tried to avoid describing the behaviour of living creatures as if they were defective human beings a considerable advance was made. But then there came on the field the term " instinct," as efficient a word to conceal his ignorance as man ever invented. The value of Mr. Howard's work lies in this, that it is an effort to understand the behaviour of the bird and to describe it in non-anthropomorphic terms. He regards the bird and its environment as a complex of reactions, and makes a determined attempt to sort out the influences which are extrinsic and intrinsic and to show how reciprocal they are.

Using as data a remarkable series of detailed observations of Buntings and other species, the writer divides the birds' behaviour during the breeding season into four phases. In the first phase " the female does not appear as a situational item, the male is occupied with his territory." The second phase is marked off by the arrival of the female in a condition to stimulate and be stimulated. Coition, nest-building, and the laying of eggs mark the third phase, and the fourth concerns incubation and care of young.

Instead of speaking of these changes in behaviour as being due to the operation of instincts, sexual, aggressive, parental and so forth, the author holds " there are not a number of separate forces, but one neurally linked pattern of reactions which with appropriate intrinsic excitation comes functionally into being." Mr. Howard shows how delicate is the reaction of the bird to its environment and what an important part the pro-œstrum, or period before the female is ready for conjugation, plays in securing that the young should hatch when there is an adequate food-supply. His point of view is shown in the following excerpt: " In a sense there are not two sexes but one sex, not two organisms but one organism, and the purpose of it all is to hatch out young at a certain time in a certain environment."

In his concluding chapter, " An Approach to a Mind Story," the writer considers whether a bird's life may be described in purely behaviouristic terms. Is the life-history nothing but the account of external stimuli acting upon inherited structural constitution? Mr. Howard's conclusion is that there is a mind-story as well as a life-story. There is not only cognitive reference but prospective reference—implying prospective imagery. A bird is not always the instrument of blind reaction. In birds one finds reference which is distinctively mental, and it appears to be the case " that space impressions are revived in the form of imagery whenever internal excitation, with which they were initially correlated, recurs, and that they contribute to guidance of action." " Revival in the form of imagery is the keystone on which the edifice of territory is reared."

In this work the writer has advanced ornithological science a distinct step, and whether his views are accepted or not, he has at any rate made it possible for the serious student of bird life to observe in a profitable, systematic way. Perhaps in a succeeding volume Mr. Howard will apply himself to the problems of bird migration. Such observations as those of the sky-lark with two territories should be of value in this connection. The writer of this review believes that migration may be explained without any of the far-fetched hypotheses which have been put forward, but as the operation on the grand scale of factors which guide the bird in its everyday routine—the impulse to seek congenial environment, recognition of territory, and the like.

The illustrations by Mr. G. E. Lodge are a valuable adornment and it is a pleasure to find artist and author collaborating. The volume is produced

according to the best traditions of the Cambridge University Press, but it is a pity that the price of such an interesting book should be such as to keep it out of the libraries of many of our young ornithologists.

E. A. ARMSTRONG.

Insect Singers: A Natural History of the Cicadas. By J. G. MYERS, Sc.D. [Pp. xix + 304, with 7 plates and 116 text figures.] (London: George Routledge & Sons, Ltd., 1929. Price 21*s.* net.)

THE family of Cicadas has been known and referred to in literature by man since classical times, yet up to the present day only few, even among entomologists, have more than a name acquaintance with the group. Dr. Myers's intentions of producing a "well-rounded natural history" and showing "what abundant new problems they offer to the general biologist" are fulfilled, and this book should do much to spread the knowledge of the Cicadas among biologists, philosophers, and psychologists. Unfortunately but a single species occurs in Great Britain.

Cicadas in Mythology, Art, and Literature is a most attractive introduction and it comprises two chapters. Next the author deals with the external and internal structure and sound organs. We suspect that for the student of morphology these chapters are rather dogmatic and not sufficiently sympathetic to the different views held. However, references are given to which he may refer. In a book such as this all that is necessary is to cover the ground with sound information, giving references to points still under discussion. The classification is dealt with ably until the author proposes a slightly modified grouping; this is passed over with merely a paragraph of remarks. Dr. Myers then discusses the evolution, early life-history, later life-history, and distribution. Then follow chapters on the general relations of the Cicadas with other organisms; with lower plants (parasitism and symbiosis); with vascular plants; with other invertebrates; the enemies of Cicadas; and the relations of Cicadas with man. The last chapter is very interesting, and it is pleasant to read that the group as a whole cannot be considered injurious. However, there is the possibility that some of them may be implicated in the transmission of the so-called "filterable virus" diseases. Chapters on Cicad psychology, their behaviour and song, together with short notes on methods of collecting, preservation, and study, bring the book to an end. A good bibliography and an adequate index are added.

This volume fills a gap and is an admirable natural history of the Cicadas. The chapter on their song has unusual qualities. We think, however, that the book is worthy of one or two really good coloured plates. To the general reader a most interesting and evenly balanced account of a little-known group of non-social insects; to the homopterist indispensable.

H. F. B.

Elementary Lessons on Insects. By JAMES G. NEEDHAM, Professor of Entomology, Cornell University, Ithaca. [Pp. viii + 210, with 72 figures.] (Springfield, Illinois, and Baltimore, Maryland: Charles C. Thomas; Agents, London: Baillière, Tindall & Cox, 1928. Price 9*s.* net.)

PROF. NEEDHAM in this volume claims to present an outline for a brief introductory course in entomology, using a new selection of material and some new plans for its use, but aiming to give a sound knowledge of the essentials of insect structure, development, and habits. He does this in an admirable way.

There are four parts—the first deals with what an insect is like, outside and inside, and how an insect grows up. The second consists of chapters

on the principal groups of insects and ends with a chapter on recognition characters of the groups. Injurious insects, both plant and animal pests, and their control form the third part, while chapters on collecting, preserving, rearing and aid in entomology comprise the fourth part. There is also a short index.

The method followed throughout is to start with a short descriptive account with a subsequent lesson. This latter is divided into two sections—the work programme and the laboratory programme. The one on work consists of getting together all that is necessary for the subsequent laboratory programme, such as field work for collecting the material, breeding certain insects, and making slides which are to be used, while the laboratory programme deals almost solely with observations to be made on the material. These two divisions might better have been described as preparation and examination of material. Prof. Needham rightly emphasises the importance of the students collecting, breeding, and making slides, and not expecting the teacher to produce, as if by magic, all the necessary things at the right moment. This is far too commonly the case. It means that a great deal of the time at the private disposal of the teacher is taken up or else the most easily obtained material is used; and, which perhaps is more important, a type of laboratory entomologist is encouraged, a type which is very good at laboratory work but deplorably ignorant of field work and its attendant difficulties of breeding and handling live insects.

The book is very clearly written and in simple language, except for a few sentences, such as " with a single lenticular cornea," which have no explanation. Naturally the material used consists of American insects and, if the book were used outside America, it would have to be modified. We notice with regret that only American works on entomology are quoted. The figures are not up to the standard of the letterpress. The lettering of the venation of the wings in Fig. 5 does not appear to be sufficiently explained. As a whole, however, the book is admirably written and students can here reach the essentials very readily. We should like to see an edition of this book suited for English students.

H. F. B.

The Principles of Systematic Entomology. By GORDON FLOYD FERRIS, Associate Professor of Zoology, Stanford University. Stanford University Publication, vol. 5, no. 3. [Pp. 169, with 11 figures.] (Stanford University Press, California; London: Oxford University Press, 1928. Price 12*s*. 6*d*. net.)

THIS publication has already been briefly noticed in " Recent Advances, Entomology " (SCIENCE PROGRESS, vol. xxiv, no. 93, p. 94).

The author, in his preface, states that there appears to be no single book to which one who may be interested in the problems of systematic entomology may turn for a discussion of the principles upon which such work is based, and a consideration of the methods by which those problems may be translated into practice. Bearing this in mind, he discusses the fundamental principles and the philosophical background of systematic entomology. " This volume is a frankly critical survey of the existing conditions in systematic entomology."

After discussing the contribution of the systematist to biology and the scope of systematic biology, Prof. Ferris proceeds to lay down eight principles of systematic entomology. These principles—the segregation of species and categories less than species, the morphological basis of systematic entomology, the preparation of material, entomological drafting, the description of species, classification, nomenclature, and the training of the systematist—are then dealt with in succeeding chapters. A good index completes the volume.

Some biologists seem to regard systematics as an occupation only fit for

dark basement rooms, for men who are content to examine necropoli of insects, and that to be a real scientist one must purport to ignore, and perhaps occasionally to condole with, men whose fate has led them to such an end. How absolutely erroneous this concept is can only be fully appreciated after reading this book. Certainly there are excuses for this idea—the accommodation of most museum systematists and the general trend of university courses to break away from the morphological side of biology. But it is to be hoped sincerely that such things are ephemeral, and already there is evidence that sooner or later systematics will reassume their position and make their influence felt in a fuller and much wider sphere. It is the nature of the species, not merely dead specimens, that is the centre of systematics. Prof. Ferris, after stating in his first principle that the segregation of species and their minor divisions is the first step in all systematic work, states that "for its accomplishment the systematist *may and should* employ every means that are available in order to arrive at a knowledge of the biological facts, whether those means be found in *morphological, anatomical, physiological, experimental, genetical, or even chemical studies.*" The italics are our own. Such, then, is the first principle. The difficulties are great. New forms usually are in small numbers, they are at once killed and put in a necropolis in order that a name may be given to them by which they can be referred to again. Perhaps it is naïve, but one cannot help wishing that a form should be given an identity name and short description in the first place and only after several years' study of its biology be confirmed as a new species. This method of a new species *nisi* would undoubtedly slow down the creation of new species, but this would not be deleterious to the advancement of science. One difficulty, however, which at present seems the most serious and even unsurmountable, is the fact that systematic entomological workers are a weird and awful combination of professional entomologists who have been trained, and dilettantes often with absolutely no idea of anything but amassing collections and affixing their own names after as many new species or forms as possible. Here is the rub. Prof. Ferris has written an admirable book that should be read by everyone interested in biology—it contains even a reprint of the international rules of zoological nomenclature—but will it ever reach these dilettantes? We do not think it will.

We recommend this book to all biologists; it could be used with advantage in all places where zoology is studied and taught, and we wish some philanthropist would buy enough copies to give every amateur entomologist.

H. F. B.

A Brief Course in Biology. By WALTER H. WELLHOUSE and GEORGE O. HENDRICKSON. [Pp. xii + 200, with 53 figures]. (London: Macmillan & Co., 1928. Price 7*s.* 6*d.* net.)

THIS book is for use in secondary schools, and contains a considerable amount of useful information, well selected and arranged in an attractive manner. Many of the illustrations are good, but those on pp. 67 and 134 are bad, the former particularly so. That on p. 107, while clear, suggests that in the frog the systematic arches pass backwards ventral to the heart. A certain feeling of doubt is introduced in places by the use of such phrases as "thought to contain," "said to consist," and so on, when the statements that follow are, so far as we know, facts. As well might one say two and two are "thought to be" four. The principal criticism, however, concerns the arrangement of the subject-matter. The first chapters deal with Algæ and Protozoa, whence we gradually ascend to the frog, which forms the principal type in the book. The authors have chosen this method of treatment deliberately since, as they say, life has developed from the simple to the complex. That this is a logical treatment is true, but that it is the best pedagogically we

do not believe, more particularly as the course is obviously intended for beginners. Apart from this the book is good, and will no doubt serve a useful purpose.

C. H. O'D.

Ants, Bees, and Wasps: A Record of Observations on the Habits of the Social Hymenoptera. By Sir JOHN LUBBOCK. New Edition, based on seventeenth. Edited and annotated by J. G. MYERS. [Pp. xix + 377, 31 figures, with 6 plates, 4 coloured by A. J. E. TERZI.] (London: Kegan Paul, Trench, Trübner & Co., Ltd.; New York: E. P. Dutton & Co., Inc. 1929. Price 10*s.* 6*d.*)

THE popularity of Sir John Lubbock's observations which has already demanded seventeen editions, together with newly acquired knowledge, have warranted this new annotated edition.

Dr. Myers begins with a short introductory note on Lubbock as an entomologist and comparative psychologist. The plan then adopted is to give the author's original preface and text without appendices, followed by one consecutive series of annotations and a short working bibliography. The five original coloured plates have been omitted and replaced by four coloured ones by A. J. E. Terzi.

The annotations consist as far as possible of quotations, which are well chosen, from modern writers on the subject and serve their purpose admirably in incorporating the views of such workers as Wheeler, Donisthorpe, and Forel.

Dr. Myers is to be congratulated on his able handling, in just over 100 pages, of the vast mass of new information. The whole book is a very desirable and welcome addition to the shelves of the general reader.

H. F. B.

MEDICINE

Letters from Rome on Certain Discoveries connected with Malaria. By T. EDMONSTON CHARLES, M.D., Q.H.P., and Addenda. Edited with a Preface and Remarks by SIR RONALD ROSS, K.C.B., K.C.M.G., F.R.S. [Pp. 78.] To be obtained from the Ross Institute, or from the Printers, for 5*s.*

THESE letters were first privately printed in 1900, and were considered as published next year—by consent of Dr. Edmonston Charles; but as only a few copies were ever produced and a still smaller number given away (none being sold) probably few copies have ever been seen by anyone, much less by the numerous ingenious compilers who are in the habit of writing medical textbooks and who often give quite inaccurate histories of discoveries.

Those who are interested in the honour of science would do well to examine these letters and the letters of Calandruccio, Koch, and Laveran, which are included in the volume. Probably no more impudent attempt was ever made to obtain credit for scientific work by means of spurious histories of discoveries, and those who are interested in medical science (which is not always my own case) will do well to record their opinion of similar occurrences. The letter from S. Calandruccio is an especially melancholy example of the mischief that can be caused by unscrupulous pretenders. Calandruccio was much depressed by the treatment that he had received and wrote numerous letters to me, but died shortly afterwards—whether in consequence of the treatment or not, I cannot say. There is apparently no known method, legal or moral, for putting a stop to such nefarious occurrences, which are a discredit to science. See also Lord Lister's statement in the book. Two of the men concerned are now dead and the third was probably misled by his collaborators.

R. R.

Lectures on Conditioned Reflexes. By I. P. PAVLOV, M.D. Translated from the Russian by W. HORSLEY GANTT, M.D., B.Sc., with the collaboration of G. VOLBORTH, M.D., and an Introduction by W. B. CANNON, M.D., S.D. [Pp. 414, with 9 photographs and figures.] (London: Martin Lawrence, Ltd., 1929. Price 18*s*. net.)

IN the series of experiments planned by Prof. Pavlov and carried out by him and his collaborators during the past twenty-five years and here described, we see the successful application of the objective method to the study of the mode of action of the cerebral cortex, in what are usually termed "psychical processes." This wonderful work demonstrates anew Pavlov's greatness of mind and clarity of thought. These same qualities are shown in his writings and lectures, in which, without the aid of any special psychological terms, using indeed only everyday words, he is able to describe and make clear these important results which will undoubtedly form the basis of future psychological study of the brain.

For most of us acquaintance with Pavlov's writings began with the reading of his book *The Work of the Digestive Glands*, which gave the account of a series of experiments into the processes of digestion. This work, which has great intrinsic interest, has played an important part in the development of the modern methods of study and treatment of digestive disorders. But probably few of those who enjoyed the book and were interested and amused by the observations that the rattling of pans or the whistling of a laboratory assistant could by association with the act of feeding become an effective stimulus of the secretion of the saliva and of other digestive juices, suspected that they were reading of physiological processes that as "conditioned reflexes" were to become the basis of these most fruitful investigations into the mode of action of the cerebral cortex. Yet it was from these observations that Pavlov was led to the experiments described in his books on these reflexes.

The first of these to be translated into English was *Conditioned Reflexes*, which consists of a series of lectures given by Pavlov in 1924 at the Military Hospital in Petrograd, in which he summed up the work done in the past twenty-five years in his laboratories. The Royal Society, which was largely responsible for this first complete translation made from the Russian into one of the more familiar European tongues, was fortunate enough to obtain as translator Dr. Anrep, a distinguished student and collaborator of Pavlov's, who since his arrival in this country has continued to work in the same field. The present volume is translated by Dr. Gantt, of the American Relief, and a worker in Pavlov's laboratory. It comprises a collection of lectures given by Pavlov on this subject from 1903 up to the lecture given in London at the Royal Society in 1928. For those who have read the former volume, with Pavlov's own full and systematised account of these famous researches, it is particularly fascinating to read in these lectures the account of the simple beginnings of this attempt to tackle, from the objective standpoint, the problem of the working of the mind, to follow the gradual evolution of the work, and to watch the solution of the great difficulties which presented themselves.

The book is full of interesting results. The method of watching the secretion of saliva gives for instance a means of studying an animal's ability to discriminate between different intensities of stimulation. It is found that a dog can differentiate such slight differences of intensity of sound, even when they follow one another at an interval of hours, as can hardly be distinguished by the human ear when they are repeated with a very short interval between them. Also after practice the ear analyser of a dog can differentiate, even after a lapse of twenty-four hours, between a metronome beat of 104 and 96. The human ear cannot distinguish between them without counting after an interval of even one minute.

Turning to a different aspect of the work there is a fascinating lecture given in 1927 entitled, "A Physiological Study of the Types of Nervous Systems, *i.e.* of Temperaments." Here it is claimed that the Greek philosopher Hippocrates, with his four groups of temperaments, came nearest to the truth as exemplified in these observations, for Pavlov fits his animals into the same four groups. There is the Choleric animal, very excitable, easily developing positive reflexes; and the Melancholic, marked out by the extreme ease of development of inhibitory reflexes; both of these, though at opposite ends of the scale, are unbalanced. In between come the well-balanced animals, which are again divided into two types, the Sanguine, eager, interested, but tending to become bored and actually to fall asleep if not continually stimulated, and the Phlegmatic, well-equilibrated individuals, but quiet and much less dependent upon outside interests. Both of these can develop either the positive or inhibitory types of reflexes, and are not easily disturbed in their mental balance. From such observations Pavlov concludes that temperament is mainly determined by the properties of the cerebral hemispheres.

In the final lecture he sums up by saying that the nervous system as a whole has for its chief function the maintenance of a dynamic equilibrium between the functional units within the self-contained system of the organism on the one hand and on the other between the organism as a whole and its environment. The most delicate adjustments belong to the hemispheres; in them alone takes place the establishment of new nervous connections, and in them are found the analysing functions. This method of study gives the possibility of showing "such a fragmentation of cortical functions . . . as could never be shown by any operative procedure. Another of its features is the astounding plasticity of its activity."

Here is the final sentence of his last lecture: "I trust I shall not be thought rash if I express a belief that experiments on the higher nervous activities of animals will yield not a few directional indications for education and self-education in man. I, at any rate, can say, looking back on these experiments, that for myself they have made clear many things both in myself and in others."

The book begins with a short but exceedingly interesting biographical sketch of the author, and is illustrated by some pleasant photographs and snapshots of Prof. Pavlov at work and at play.

W. C. CULLIS.

Protozoology: a Manual for Medical Men. By JOHN GORDON THOMSON, M.A., M.B., Ch.B., Director Department of Protozoology, London School of Hygiene and Tropical Medicine, and ANDREW ROBERTSON, M.B., Ch.B., Lecturer and Milner Research Fellow, Department of Protozoology, London School of Hygiene and Tropical Medicine. [Pp. xiii + 376, with 220 figures and 4 coloured plates.] (London: Baillière, Tindall & Cox, 1929. Price 30*s*.)

THE authors of this book set themselves a very difficult task. No doubt it is a less arduous undertaking to write a book on the medical protozoa than to tackle the whole of protozoology, but unless the book is to prove indigestible to medical men and to offend zoologists it is certainly more difficult. When further limits are set to the scope and attempt is made to present something which will both give a correct and sufficient view of the zoological side and also provide an adequate basis for clinical and preventive practice, while sparing medical readers superabundant zoological classification and argument, the problem becomes formidable indeed.

That the present authors should have conceived a book on the lines of that under review was to be expected of protozoologists not only devoting their attention to the laboratory study of this special branch, but also for

long engaged in teaching it to those for whom this book is written. That they should have succeeded so admirably was perhaps also to be expected by any who know how they approach their actual teaching. This book, like the authors' teaching, tempers academic knowledge by a hard-headed practicality. It never loses sight of its purpose, which is not to convey knowledge for its own sake but knowledge which will be an efficient working tool.

There are provided adequate descriptions of the parasitic protozoa, of their mode of life and life-histories, and the picture of parasitism is amplified by accounts of the pathology of the conditions produced by the interaction of the parasite and the tissues of the host.

The valuable chapter on technique, which has due regard to the limitations of out-of-the-way laboratories, together with those on common fallacies in blood examination and common objects in the fæces, and the beautiful drawings and coloured plates enhance the usefulness of the book.

An unusual feature is a list of derivations and definitions. For those who have "little Latin and less Greek" these are helpful. Those who are classically inclined will not like "*kinetikos*, to move," and "*basis*, to go."

The book is well produced and printed; perhaps too well, since the price is 30*s*.—the only matter in which the book is not well suited to those for whom it is intended.

J. F. C. Haslam.

MISCELLANEOUS

Four Years' Farming in East Anglia, 1923-1927. Being a detailed investigation of the costs and returns on twenty-six farms. By R. McG. Carslaw, M.A., with a Foreword by J. A. Venn, M.A. [Pp. x + 125.] (Cambridge: W. Heffer & Sons, 1929. Price 3*s*. net; cloth 5*s*.)

In the words of Mr. Venn, we have here "presented for the first time, it is believed, in this country a complete analysis of the results of four years' activities on what there is little reason to doubt are representative farms well distributed throughout the five million acres that comprise the Eastern Counties Province." A "full cost" inquiry, such as this is, necessarily entails a vast amount of clerical labour, and the results are not therefore available in their entirety for some years after the period under review has expired. Their value for the future is dependent, therefore, on the assumption that general farming conditions such as are taken account of will not change appreciably for some time. Thus there is little doubt that information of the kind now available would have been worth a very great deal to the country on the outbreak of war, and later when it became necessary to control the whole industry, including profits and wages. From this point of view, it is a little unfortunate that the farming industry is not at present in such a stable condition that the results of this inquiry will not need to be modified to take account of the changes in farming practice. The period under review has coincided with a time of depression; it has witnessed a gradual decrease in arable farming, while the enormous increase of the land growing sugar-beet has been one of the most rapid of changes that have taken place in farming history. Indeed, it is admitted that the results of a special sugar-beet enquiry, conducted in 1927, differ from the results of the comprehensive costings enquiry for the years 1924–7, and show that the latter is already to some extent out of date. This is to some extent a weakness of a method which requires so much patient investigation and which takes so long to accumulate results. Another important point is that the farming

conditions of East Anglia are assessed on the information yielded from a small sample of twenty-six farms. This makes it essential that the sample chosen should be sufficiently representative of conditions over the whole area. The author has laboured gallantly to ensure that this shall as far as possible be the case. There are difficulties, however. The clerical work necessary on the part of the farmers selected has confined the choice to such farms as were able, and willing, to undertake this work, while only farms that could claim to be run on commercial lines received attention. It is a great pity that a more representative selection could not have been made from farms of the more prosperous fen-land area. But the tendency was for farmers who were making the largest profits to withhold their co-operation. As a result, only one fen-land farm is represented, and the information in regard to one important crop, potatoes, is therefore inadequate. Half the acreage given over to this crop occurred on the one fen-land farm included, and owing to two successive attacks of "blight" on this farm, the profit-and-loss statement concerning this crop loses a great deal of value.

On the whole, however, the author is to be congratulated on the large measure of success he has achieved in describing the general conditions of this area. He found that with the "best" farms assets increased during the period under review, but with the "worst" farms assets decreased and overdrafts increased. This is shown in a series of comprehensive tables, and stresses the regrettable fact that the losses on the worst farms are partly due to an increase in the interest paid on overdrafts and loans. Summing up, the author states that capital sunk in farming enterprises in this area has yielded little more than what would have been obtained from gilt-edged securities. This is a little more hopeful than a statement in a former preliminary report that the yield was less than that realised from this eminently safe form of investment, but the position is obviously far from satisfactory, especially when it is borne in mind that no allowance is here made for the value of unpaid family labour. The method was to estimate the net farm profit (or loss), which varied from a profit of £2 9*s.* 4*d.* per acre of crops and grass on the best heavy soils to a loss of 13*s.* 10*d.* on the worst heavy soils, the light soils, best and worst, being intermediate, and showing a moderate profit. If from this is deducted interest at 5 per cent. on occupier's capital and the estimated value of unpaid work (on the basis of £200 per annum for managerial work and £2 per week for manual work), the profit on the "best" farms shrinks to 15*s.* 4*d.* per acre on the heavy soils and to 8*s.* 6*d.* on the light soils, while there is a heavy loss on the "worst" farms of £1 15*s.* 4*d.* per acre on the heavy soils and 14*s.* 3*d.* on the light soils. There is, therefore, good reason for the author's statement that "while some farmers have been realising heavy losses, others have been making good profits, and, in this connection, the type of soil appears to be one of the main determining factors." Allowance must be made, of course, for the fact that the more prosperous farmers are inadequately represented, while those whose financial conditions were not of the soundest were often only too pressing in their willingness to assist, in the hope that sympathy and, perhaps, succour would be forthcoming in official quarters. But there is little doubt that this volume adds to the general consensus of opinion that farmers to-day are not doing well. The author did not evidently consider it his duty to say where a remedy might be found for a state of affairs which must be a matter of grave concern to everyone, but he makes one or two interesting suggestions. For one thing, he notes strong indications that the supply of foreign beef available for British consumption is becoming more and more insecure, and that therefore the English farmer may be expected soon to come into his own again. Warning is given, however, by comparison with prices obtaining in Scottish and English markets, of the necessity to consider type and quality of produce and methods of marketing. In regard to milk, a suggestion which is of

interest to the consumer at the present time is that professional milk distributors appear to be getting more than their fair share of the profits realised from the industry, and could therefore well afford the extra penny per gallon necessary to convert the farmer's loss into profit, without increasing the price to the consumer. Sugar-beet is considered to be promising, in that experience in the growing of this crop has reduced materially the farm cash cost per acre, while the record acreage for 1929 augurs well for the future. Cereal farming, however, is still an uncertain proposition. "'Corn,' said Mr. Micawber, 'may be gentlemanly, but it is not remunerative,'" and that seemed to be true of the period under review, although prices since then have improved the position materially. The author ends on the hopeful note that while reorganisation in agriculture must be slow, it is untrue to say that farmers are not capable of moving with the times, and of adapting their organisation to new conditions.

J. WISHART.

The Subject Index to Periodicals. Issued by the LIBRARY ASSOCIATION. [Pp. ix + 278]. (London: The Library Association, 1929. Price £3 10s.)

THE vast accumulation of material on all subjects which pours in a never-ending stream from the modern printing press has made it absolutely imperative that guides and indexes of a variety of sorts should be supplied to help those in search of information. The scientist stands in particular need of these aids, since the great bulk of scientific work is now published in journals, many of which he may never see unless his attention is drawn to them. He wants to see conveniently gathered together all the articles relating to the particular subject in which he is interested, and quite rightly he looks to the librarian, if not actually to do the gathering, at least to show him where he should look for the matter he requires. Hence the great increase in recent years in the number of bibliographies of all kinds. That the Library Association's *Subject Index to Periodicals* should hold a prominent position among these is but fitting; the Association produces its annual volume, not for profit, but in the well-founded belief in its general usefulness to librarians and the ever-growing host of seekers after knowledge whom it is the librarian's privilege to serve.

The *Subject Index* was originally issued in 1916 in the form of class-lists with an annual consolidated volume. Experience has shown, however, that it is more convenient to have all the entries in one alphabetical sequence of subject-headings, and in this form in a single volume it now appears. Some 600 periodicals are indexed, of which quite a fair proportion deal with scientific subjects. The majority of these periodicals are naturally English or American, but they include also journals published in France, Belgium, Switzerland, Germany, Holland, Italy, and Finland. The total number of entries is in the neighbourhood of 23,000. As an example of the usefulness of the index the subject "Aeronautics" may be cited. Under this heading 85 entries will be found carefully classified under a series of alphabetically arranged sub-headings, and with a large number of cross-references.

The work is admirably edited and bears on every page the evidence of careful and patient work, both in the original compilation of the entries and in the subsequent proof-reading. The volume is well printed and produced in a style which reflects great credit upon the Library Association. It should be, and indeed probably is, upon the shelves of every reference library of repute, and the scientist should know that it is there to help him.

J. WILKS.

Catalogue of Lewis's Medical and Scientific Circulating Library. Part 1. Authors and Titles. Part 2. Classified Index of Subjects, with Names of Authors who have written upon them. New edition, revised to the end of 1927. [Pp. 576.] (London: H. K. Lewis & Co., Ltd., 1928. Price 15*s*. net; to subscribers, 7*s*. 6*d*. net.)

THE activities of the firm of Messrs. H. K. Lewis & Co., Ltd., as publishers of medical books are too well known to need mentioning; and there must be few medical men in London who have not at some time or other found it to their advantage to subscribe to Messrs. Lewis's Circulating Library. We doubt, however, whether the admirable catalogue of the Library is as widely known as it deserves to be. Its value is far greater than that of the usual list of the contents of a library, though the first part of the volume amounts to no more than this. The second part is devoted to a most useful classified catalogue of recent works dealing with a wide range of scientific subjects. The name of Lewis is of course primarily associated with medicine, but here will be found also long lists of books on Archæology, Astronomy, Engineering, Geography, Geology, Philosophy, Physics, Surveying, and a whole host of other matters. For particulars of the works listed under any one of these headings one has to refer back to Part 1 of the *Catalogue*, where all works are placed in a single alphabetical list; but the references are clear and simple, and the labour involved is very slight, since the two parts are bound in one volume. Moreover, the classified index gives the initials of authors wherever two or more writers of the same name have written on the same subject, the date of the work, and in many cases a word or two to indicate its nature. In these respects a considerable improvement has been effected as compared with earlier editions of the *Catalogue*.

No library, of course, contains all the books which every user of it hopes to find on its shelves, and nothing is easier than to compile a list of works which might be included, but are not. None the less one cannot help regretting that Messrs. Lewis's Library does not include foreign books unless they are translated. So much scientific work of all kinds is published in Germany, to take an obvious case, that one might reasonably expect to find, at any rate, the more important of such publications included. It is disconcerting also to find a long list of works headed "Botany" among which the name of Linnæus does not appear and to discover, on further examination, that the great classical scientific works of the past are generally missing. But these omissions are obviously due to deliberate policy, and do not prevent us from congratulating Messrs. Lewis upon the production of a most useful volume.

J. WILKS.

Speech and Hearing. By HARVEY FLETCHER, Ph.D. [Pp. xv + 331.] (London: Macmillan & Co., 1929. Price 21*s*. net.)

A LARGE amount of work has been done in the Research Laboratory of the Bell Telephone Company in classifying the various characteristics of speech, and the psychological aspects of hearing, particularly with regard to intelligibility on the telephone. This book may be regarded as a *compte rendu* of this research work, most of which was carried out under the direction of the late I. B. Crandall and the author. It gives many instances of the recent transference of the methods and results of alternating current technology to sound. At the outset it was necessary to invent a standard of loudness, and to classify the sounds of spoken "English," which were reduced to thirty-nine. In this connection English readers who do not frequent the "talkies" may be surprised to read that "the sounds in the words part, not and father were sufficiently similar to be designated by a single letter—a." A useful critique of theories of hearing is given which seems to establish the resonance theory, with certain modifications introduced by recent research.

E. G. R.

Science and Personality. By WILLIAM BROWN, M.A., M.D., D.Sc. [Pp. ix + 252.] (London: Oxford University Press, 1929. Price 12s. 6d. net.)

IN this book Dr. Brown has given us much more than its title suggests. It consists of a series of essays, some of which have been delivered as lectures, chiefly on different aspects of psychology. As a result, the book lacks continuity; not that the chapters have no connection with each other, but the general impression is one of a series of articles rather than a whole, developing gradually towards its conclusions. This does not detract from the value of the parts; they are all excellent.

The bulk of the book is occupied by an account of psychological fact and theory, including their medical applications. As one would expect from the author's experience and previous writings, this is clear and critical. It is as pleasant as it is unusual to find theories like those of Freud discussed without prejudice or uncritical credulity. The language is such as can be understood by the educated layman. Probably there is little in this part which will be new to the student of psychology, but it is important that among the fundamental instincts Dr. Brown finds a place for a "cognitive" instinct, an instinct which, as he says, has been neglected by contemporary psychologists.

The chapters discussing the physical, biological, and psychological sciences in their relations to each other and to personality are good, but a little too concise. It would have added clearness if the biological sciences in particular had been discussed at greater length, and especially the theory of emergent evolution. There are two chapters on psychical research. They deal mainly with personal experiences of the author, and are critical and unprejudiced, although, of necessity, inconclusive.

Philosophically the first and last chapters are the most important. They contain a discussion of personality, chiefly from the point of view of its relation to the universe, and a very interesting account of the author's views on religion and the religious attitude. They do partly remedy the lack of coherence between the parts of the book, which, as a whole, is a very fair and unbiased discussion of difficult and controversial questions.

F. GOLDBY.

The Trade of the Indian Ocean. By V. ANSTEY. The University Geographical Series. [Pp. xvi + 251.] (London: Longmans, Green & Co. Price 8s. 6d.)

THE purpose of this book is to present a summarised view of the trade, industry, and commerce of a large and important natural region, and thus provide a guide to further inquiry. The author, who is Assistant Lecturer at the London School of Economics, has produced this account in a true geographical spirit. The picture is based upon a background of official returns, physical and geological factors, political and economic considerations, whilst history too has not been ignored.

The work has been thoroughly carried out, and it is possible to approach, from the data provided, the questions of a utilitarian character such as the trade of the future or probable sources and results of competition in this region.

India and problems relating thereto will occupy a prominent position in the public mind in the immediate future. This book will enable many of these problems to be approached in a scientific manner. The ignorance of English people on matters concerning India has long been a reproach upon our interest and conscience. This work deserves careful study by interested students and all teachers of geography (against whom the charge of national ignorance must weigh heavily). We may even suggest that those upon whom the political aspect and future status of India depend might with profit read and learn from this volume.

May the reviewer call attention to one or two minor slips which have found their way into this edition?

The parity value of the gulden (p. 57) should read 12.107 to the pound sterling. It is unfortunate, too, that the author forgot Fig. 9 when he was making reference to Sarawak (p. 194), which he then mistakenly places in British North Borneo.

The figures (specially prepared) are numerous, and on the whole good, but some, such as Figure 5, one may suggest will require much interpretation. It may not be unwise to suggest that even advanced students may find more confusion than elucidation in such symbolic figures. The index and bibliography are further features which commend the book.

J. Eling Coleclough.

Proceedings of the Seventh International Congress of Photography. By T. Slater-Price and others. London, 1928. [Pp. xiv + 571.] (Cambridge: W. Heffer & Sons. Price 25*s*.)

Photographic materials are very complex systems and the demand for more knowledge about them appears to increase year by year. On the one hand, manufacturers require much more information about the fundamental chemical and physical mechanisms which operate when light acts on their products; on the other hand, users demand a much wider, and, at the same time, more accurate description of the sensitive qualities of the photographic materials which they wish to employ. The manufacture of silver halide "emulsions" in gelatin is still very largely empirical, but there is a gradually growing mass of precise knowledge about the conditions controlling sensitivity, and this is reflected in the ever-widening range of materials available for photographic purposes. The demands made by science and industry as well as art on photographic methods have caused a great mass of work to be carried out in devising new and improved methods of testing sensitive materials. The volume under review bears eloquent testimony to the great efforts which are being made to extend photographic science. About seventy papers were communicated to the Congress, and it will therefore be possible to select only a very few for special comment.

Eleven papers deal with subjects relating either to the sensitivity of silver halide emulsions or to methods of studying emulsions during manufacture. Three of these may be mentioned. F. C. Toy summarises much of the early work on the photo-conductance effect in silver halides and describes some of his own experiments in the same field. He goes far towards proving that the first action of light in forming the latest image in silver halide emulsions is the same as that which produces changes of electrical conductance in silver halides on illumination. For this reason a study of the photo-conductance effect in silver halides will probably clear up many of the mysteries about photographic sensitivity. Chemical analysis has not yet been made delicate enough to deal with the problem of the latent image and the opening of a new avenue of attack creates a lively feeling of expectation in those engaged in this field of research.

It is now recognised that the size of the silver halide particles in silver halide emulsions is an important factor and must be controlled during manufacture. So-called "turbidity" measurements have often been made on emulsions in order to estimate variations in average particle size. The optical properties of disperse systems are, however, very complicated; any attempt at bringing order amongst the somewhat chaotic ideas existing about this subject is therefore to be welcomed. B. H. Crawford, E. R. Davies, B. Farrow, and F. C. Toy have made an excellent beginning in this subject with the work described in a "Report on Turbidity, with special reference to its Application in the Photographic Industry." It may be noted

that the full understanding of the problems discussed in this paper must be of great importance in the Paints and Pigments industry.

The third paper to be mentioned in this group is "A Chemical Study of Desensitisers. Part I. An Account of Known Desensitisers," by F. M. Hamer.

Coming now to the behaviour of photographic materials towards light, and the appropriate methods of investigation, we find thirteen more papers. Two of these deal with a proposed international standard light source for the testing of negative materials. The proposals, to which the Congress agreed in principle, are founded on the report furnished by a special committee of the Optical Society of America on a Photographic Unit of Intensity, and this in its turn is based on the work of R. Davis and K. S. Gibson, of the American Bureau of Standards, who describe a two-cell filter by means of which the radiation from a lamp burning at a colour temperature of 2,360° K. may be converted approximately to Abbott's values for mean noon sunlight at Washington. General problems of sensitometry are dealt with extensively in six papers by S. E. Sheppard, L. A. Jones, and others from the Kodak laboratories, and in another paper by R. Luther, W. Seifert, and W. Forstmann. Methods of measuring the resolving power of photographic materials are described by O. Sandvik. An example of the use of fine grain developers in spectrography is given by E. Viterbi, who shows a very much enlarged spectrum photograph in which a clear separation is visible between the images of the lines 2625·79 Å. and 2625·60 Å. A. Hübl contributes a paper on "The Determination of the Colour Sensitivity of Photographic Plates."

The remaining papers cover a very wide range of subjects. Readers interested in the printing industry will find ten papers devoted to photo-engraving. Eighteen papers deal with photographic practice and the scientific applications of photography. Three other sections deal with colour photography, kinematography, and with such subjects as the bibliography of photography, record photography, the history of photography, etc.

S. O. R.

Photographic Emulsions. By E. J. Wall. [Pp. viii + 256, illustrated.] (London: Chapman & Hall, 1929. Price 21*s*. net.)

The manufacture of photographic sensitive materials is still very largely empirical. That this should be so nearly sixty years after the invention of the silver bromide-gelatin emulsion may be considered remarkable. The reason is, however, not very difficult to find. The action of light in forming the latent image is not yet fully understood, still less is known of the mechanism of the initiation of development in photographic materials; photographic sensitivity in itself, therefore, remains a mysterious property possessed in varying degree by different photographic emulsions and the emulsion maker is unable to *predict* the conditions necessary to achieve a given sensitive quality in his products, because nobody knows exactly why a given material behaves in a particular way. Mr. Wall must have recognised this when he wrote this little book. It is mainly a book of recipes; about 140 pages are devoted to describing various emulsion formulæ, chosen so as to cover most kinds of silver salt emulsions. The remainder of the book deals very briefly with the necessary equipment and operations involved in making and coating photographic sensitive materials. No attempt is made to discuss the problem of photographic sensitivity.

Mr. Wall was a writer who had great experience of photographic materials, and this book is a valuable addition to the literature of a very difficult subject.

S. O. R.

Overhead Power Lines. By Capt. W. Morecambe, R.E., B.Sc. (Eng.), A.M.I.E.E. (Pp. xi + 236, with 23 working tables, 18 working curves, and 73 other diagrams). (London: Chapman & Hall, 1929. Price 15s.)

In the Introduction the author says that his book is intended to present certain constructional data for overhead lines in an easily interpreted form, and to explain the elementary features of design. This he has certainly achieved with conspicuous success.

A comprehensive array of tables and curves, some deduced from practical measurements and others from theory, are employed to elucidate points of special importance; in fact, the book is a compendium of data particularly useful to the engineer engaged in designing and in arranging the erection of overhead power lines.

For the purpose of making a comparison between different systems certain costs are included, but these may very soon become out of date.

A number of theoretical formulæ are given without any attempt to explain their origin. This is probably the best procedure in a book mainly concerned with the application of the theory to practical examples.

Some very good sketches are inserted showing various types of overhead line equipment.

The problems considered are, for the most part, mechanical, and only a short account is given of the electrical aspect of the subject, this being confined to the first chapter. Then follows a discussion of sag and stress calculations, various conductor arrangements and clearances. Next the insulators and their supports are dealt with, and the principal types of poles in general use, including wood, iron, and ferro-concrete, are carefully studied. A brief comparison is made between lines of copper and other metals, with some notes about composite conductors like steel-cored aluminium. The book closes with a useful list of the most important safety precautions which are prescribed by the Postmaster-General and the Electricity Commissioners. The full safety requirements are printed as an Appendix.

Much of the information given is not easily obtained elsewhere, so that, with the recent developments in electrification schemes employing overhead lines, the book should make a wide appeal.

H. Monteagle Barlow.

The Growth of the World and its Inhabitants. By Prof. H. H. Swinnerton, D.Sc., A.R.C.S., F.Z.S., F.G.S., Head of the Department of Geology and Geography, University College, Nottingham. [Pp. 211, with 31 illustrations.] (London: Constable & Co., Ltd., 1929. Price 5s. net.)

This volume is uniform with "Chemistry in the Home" reviewed elsewhere, and is equally good. It is divided into two parts, the first of which opens with an excellent sketch of the gradual transition from mediæval to modern views of nature. In dealing with this, the author traces the development of the idea which occurred to Lyell "that perhaps there were forces and processes in Nature to-day, working quietly and slowly, which in course of time could do all the work which existing opinion ascribed to catastrophic agency." That "the surface of the earth, so far from being *terra firma*, is in a state of instability, rising here, sinking yonder," is illustrated by examples from Georgia, Stockholm, and Kent, the last being the well-known Reculver—a church on the edge of the cliff, though not less than a mile from the water in the days of King Henry VIII. A still more striking instance might have been given in the case of Dunwich, the Suffolk town which received royal charters from the early English sovereigns, but now lies under the sea.

The author goes on to deal with rock types, the natural storage of ores and minerals, the internal constitution of the world, and the light thrown upon this by its earlier history, the calendar of the later stages, the growth and destruction of mountains, the formation and dissection of plains and plateaux, and the structure and fate of continents.

Part II is entitled "The World of Living Things." It begins with life in its lowest forms and traces its development up to the emergence of mankind, concluding with two good chapters on the growth of intelligence and culture and *Homo sapiens*.

H. S. T.

An Introduction to the Study of Map Projections. By J. A. Steers, M.A., Fellow and Dean of St. Catherine's College, Cambridge; University Lecturer in Geography; with a Foreword by F. Debenham, M.A., O.B.E., Fellow of Gonville and Caius College, and Reader in Geography in the University of Cambridge. [Pp. xxiii + 204, with 87 illustrations.] (London: University of London Press, 1929. Second and revised edition. Price 8*s*. 6*d*. net.)

The first edition of this book was published in 1926, and was fully reviewed in these pages shortly afterwards.

The chief feature of the second edition, as distinct from the first, is that the Appendix has been considerably enlarged and Trigonometrical Tables have been added, by which the usefulness of the work has been increased.

H. S. T.

This Bondage: A Study of the "Migration" of Birds, Insects, and Aircraft, with some Reflections on "Evolution" and Relativity. By Commander Bernard Acworth, D.S.O., R.N. [Pp. xxiv + 229.] (London: John Murray; not dated. Price 7*s*. 6*d*. net.)

In this book the migration of birds and other ornithological phenomena are approached from a new angle. The work seems to have grown from a remark of the author at dinner one night "that he supposed everybody realised that, of all creatures, birds in free flight are the only living things that have never felt a breath of the wind that is blowing, no matter how fierce or tempestuous the wind may be. Strangely enough," continues the Preface, "the statement of this simple truth occasioned much surprise, interest and incredulity. . . . The simple fact of absence of wind-pressure on air-borne bodies, whether birds, insects, or machines, and the very general, but quite natural, misunderstanding of the lay public on this important matter gave rise in the author's mind to a long series of reflections of which this modest volume is the embodiment."

Commander Acworth has certainly produced an arresting and thought-provoking book. One of the interesting facts he brings to light—and illustrates by means of a diagram—is the helplessness of birds to return to their nests in a strong wind. They cannot alight at all if the velocity of the wind is greater than their own flight-speed, and this accounts for the exhausted birds one finds on the ground. A good deal more might be said, but this must suffice, for those interested will follow up the matter in the book itself.

It remains to add that the author scarcely follows scientific method when, by attaching epithets—presumably satirical—to two prominent ecclesiastics with whom he disagrees, he asks, "And what of our spiritual Lord, Dr. Barnes, F.R.S.?" and speaks of "the very Christian Dean of St. Paul's"—to select but two phrases from several.

H. S. T.

The Rhythms of Life and other Essays in Science. By D. F. Fraser-Harris, M.D., D.Sc., F.R.S.E. [Pp. vi + 185.) (London: George Routledge & Sons, Ltd., 1929. Price 5s. net.)

This book contains a series of excellent "popular" essays, some of which are reprinted from various journals. It is one of a new "Science for You" series, of which the title is self-explanatory. If one essay may be picked out of a feast of good things, the first, which gives its title to the book, is worthy of careful reading. It deals with the rhythms of life. "Why," asks the author, "should the human heart beat seventy-two times a minute and not seven or 700? Why should we breathe eighteen times and not eight or eighty times a minute?" We do not know, and this striking essay concludes with the statement: "Spontaneous rhythmicality is the great mystery of life, the central puzzle in biology; if we knew what rhythm really was, understood it 'all in all,' we should perhaps 'know what God and man is.'"

If points of printing may be criticised, it is somewhat annoying to find, in passages like that quoted above, that numbers are expressed sometimes in words and sometimes in figures, even in the same sentence—"seven or 700."

H. S. T.

The Modern Dance of Death. By Peyton Rous. [Pp. 51.] (Cambridge: At the University Press. 1929. Price 2s. 6d. net.)

How has modern medical practice affected our longevity? In what new ways may it yet tinker with our bodies to hold off disease, and to repair damages that are unavoidable? What risk is there that in our sometimes short-sighted treatments of diseases we are opening the door to worse troubles? With such heart-searchings did Dr. Rous entertain his colleagues in his Linacre lecture, reprinted under the above title. In mediæval times men clung to the pleasant, if fictitious, consolation that Death came in horrid form and at awkward times to the wicked, treating the just and poor (usually considered synonymous) with more consideration. Our view of Death's activities is more impersonal: we have learned that it is often possible to outwit him, and we find no longer that bitter satisfaction in contemplating his levelling activities.

Dr. Rous scans the International List of Causes of Death to see if the real dance of death has changed, how far the actions and reactions within the diseased organism have been altered since Linacre's day. He brings to light cases where our successful treatment of the rapidly fatal symptoms of a disease has revealed chronic symptoms previously unimportant because so few ever lived to display them. Is it conceivable that the insulin treatment of diabetics may ultimately fall into this category? Again, to what extent are we justified in regarding the tonsils, the vermiform appendix, and the gall bladder, for example, as superfluous, or nearly superfluous, organs? How far can we with safety remove even vital organs when diseased? Dr. Rous discusses the possibilities of transplantation and grafting, and of the administration of active principles, but concludes that "man's power to substitute for organs is so narrowly limited that the end may already be in sight." "Few genii," he says, "can be expected to emerge from the ancient physiological bottle."

Turning to microbial diseases Dr. Rous emphasises the ease with which many infections have been met when their method of propagation was once known. These methods, where we know them, are often ludicrously simple, and have been successful in the past only because, in matters of health, man has been an unthinking animal. Very simple measures defeated the yellow fever mosquito, once its habits were known.

To medical students and biologists this book is strongly recommended.

P. Eggleton.

The Matrix of the Mind. By FREDERICK WOOD-JONES and STANLEY D. PORTEUS. [Pp. 424.] (London: Edward Arnold & Co., 1929. Price 21s. net.)

THE primary object of this book is to indicate, side by side, the evolution of structure and function with regard to the nervous system, and their close interdependence in the process; the spinal cord, the brain, and the sympathetic nervous system are all included in its scope, and the volume constitutes an exceedingly suggestive study in comparative neurology.

Prof. Wood-Jones has contributed the major part of the work. By profession an anatomist, he is in reality far more than this, and is specially qualified by his tastes and habits of mind, no less than by his mental equipment, to undertake such a work, requiring, as it does, the close observation of a field naturalist and the specialised knowledge of an anatomist and physiologist, combined with a contemplative and fertile mind, which can sit in an easy chair and link up the significance of a great variety of observations.

The volume is teeming with original ideas, many of them very far-reaching in character; the presentation of anatomical facts regarding the nervous system which are already familiar to a considerable section of readers, is ever enlivened by the suggestions accompanying them; their inclusion is so clearly set forth as to increase the number of readers who can appreciate the study. Such a work, though it contains many undoubted facts, is inevitably often speculative in character, and on reading it one needs to maintain throughout a critical and cautious attitude, noting the facts and welcoming the ideas regarding them, but ever distinguishing between the two. The very clear diagrams and illustrations in the text are of great assistance throughout.

The second and lesser part of the volume is by Prof. Porteus, who is primarily a psychologist. There has been close co-operation between the two authors, but there is a good deal of overlapping in the scope of the two sections. It is never very easy to combine authorship unless the one influence predominates throughout: something vital in the writer of the first section is inevitably missed on passing to the work of a different type of mind; there is much that is interesting in the second part of this volume, but perhaps its individual work would be more fully appreciated if it stood alone.

Those of us who feel the maximum of indebtedness to the writer who gives us ideas and sets us thinking will feel extremely grateful to the authors of this volume.

Bio.

Greek Thought and the Origins of the Scientific Spirit. By LÉON ROBIN, Professor at the Faculty of Letters of the University of Paris. [Pp. xx + 409, with a Map.] (London: Kegan Paul, Trench, Trübner & Co., 1928. Price 21s. net.)

PROF. ROBIN'S work is one of the volumes in the series devoted to "The History of Civilization," edited by Mr. C. K. Ogden. In such a series the Greek contribution to civilization naturally receives due attention, and the book under review is only one of six or seven volumes dealing with that fascinating theme. To make the series self-contained it was obviously necessary to include a history of Greek philosophy, and Prof. Robin has carried out his task in a praiseworthy manner. Beginning with Homer and Hesiod he narrates the story of Greek thought right down to the end of Neo-Platonism, and his narrative is lucid, interesting, and in accordance with the views commonly accepted by historians of Greek philosophy. If it is rather conventional, it is at all events a safe guide, and offers sufficient guidance and stimulus for the further study of Greek thought from the best sources. But it must be confessed that it has not the originality and power of Prof. J. Burnet's *Early Greek Philosophy*, which easily and deservedly

remains the favourite history for English readers. For this, of course, Prof. Robin cannot be blamed. If any criticism is to be made at all, it is on rather different grounds. The emphasis laid, in the very title of the book, on "the scientific spirit" rather leads one to expect an adequate history of Greek science as well as of Greek philosophy. But in this respect one is disappointed. And it is somewhat disconcerting to find that among the projected "subject histories" to be included in the series, nothing is promised on the history of science, although the histories of Medicine, Music, and even Witchcraft have already been planned. Perhaps the omission is only temporary.

A. WOLF.

The Evolution of Scientific Thought from Newton to Einstein. By A. D'ABRO. [Pp. 544.] (New York: Boni & Liveright, 1927. Price $5.00.)

THE title of the volume is perhaps a little misleading, no doubt unintentionally. The main or central theme of the book is Relativity, what led up to it, and what follows from it. Part I deals with Pre-Relativity Physics (pp. 23–139); Part II is devoted to the Special Theory of Relativity (pp. 143–241); Part III explains the General Theory of Relativity (pp. 245–370); and Part IV is devoted to the Methodology of Science. Mr. D'Abro has evidently taken great pains to be lucid, and considering the inherent difficulty of his great theme he deserves to be congratulated on the great measure of success which he has achieved. His book may be said to have a place of its own midway between the host of popular books, which are too popular to be sufficiently accurate, and the severely technical treatises, which are too difficult to be even reasonably popular. The book requires hard thinking, of course, but those who are willing to pay this reasonable price will be rewarded by a very fair understanding of the great problems discussed, even if they are not expert mathematicians. The weakest part of the volume is Part IV. In view of the further developments of the theory of relativity since the publication of Mr. D'Abro's book, he would be well advised to omit Part IV altogether, and replace it by an account of relativity up to date.

A. WOLF.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Advanced Mathematics for Students of Physics and Engineering. By D. Humphrey, B.A., B.Sc., Head of the Department of Mathematics and Physics, the Polytechnic, London. London: Oxford University Press, 1929. (Pp. viii + 175.) Price 12*s*. 6*d*. net.

The Aims of Mathematical Physics. An Inaugural Lecture delivered before the University of Oxford on November 19, 1929. By E. A. Milne, Rouse Ball Professor of Mathematics in the University of Oxford. Oxford: at the Clarendon Press, 1929. (Pp. 27.) Price 2*s*. net.

Seven-Place Natural Trigonometrical Functions, together with many Miscellaneous Tables and Appendices on the Adjustment of the Engineer's Transit and Level, Area Computation, Vertical Curves, Simple Curves, and Determination of Latitude, Longitude, and Azimuth. By Howard Chapin Ives, C.E. New York: John Wiley & Sons; London: Chapman & Hall, 1929. (Pp. v + 231.) Price 12*s*. 6*d*. net.

The Great Mathematicians. By H. W. Turnbull, M.A., Regius Professor of Mathematics in the University of St. Andrews. London: Methuen & Co., 36 Essex Street, W.C. (Pp. viii + 128, with 19 diagrams.) Price 2*s*. 6*d*. net.

Cambridge Five-Figure Tables. By F. G. Hall, M.A., Head Master of the Grammar School, Ashton-in-Makerfield, Lancs., and E. K. Rideal, M.A., Humphrey Owen Jones Lecturer in Physical Chemistry to the University of Cambridge. Cambridge: at the University Press, 1929. (Pp. 96.) Price 3*s*. 6*d*. net.

Introduction to the Theory of Fourier's Series and Integrals. By H. S. Carslaw, Sc.D., LL.D., F.R.S.E., Professor of Mathematics in the University of Sydney. Third Edition. Revised and Enlarged. London: Macmillan & Co., St. Martin's Street, 1930. (Pp. xiii + 368.) Price 20*s*. net.

Gaseous Combustion at High Pressures. By William A. Bone, D.Sc., Ph.D., F.R.S., Dudley M. Newitt, Ph.D., D.I.C., and Donald T. A. Townend, Ph.D., D.I.C. London: Longmans, Green & Co., 1929. (Pp. xiii + 396, with 14 plates and diagrams in the text.) Price 42*s*. net.

Experimental Optics. By Albert F. Wagner, Professor of Physics and Electrical Engineering, United States Naval Academy Postgraduate School. New York: John Wiley & Sons; London: Chapman & Hall, 1929. (Pp. xii + 203, with 117 figures.) Price 16*s*. net.

The Electro-Magnetic Field. By Max Mason and Warren Weaver. Chicago, Illinois: at the University of Chicago Press; Cambridge, England: at the University Press. (Pp. xiii + 389.) Price 27*s*. net.

An Introductory Electricity and Magnetism. By C. W. Hansel, B.Sc., Senior Science Master at Bedford School, and P. Woodland, M.A., Assistant Science Master at Bedford School. London: William Heinemann, Ltd. (Pp. x + 286, with 43 figures.) Price 4*s.* 6*d.* net.

The National Physical Laboratory Collected Researches. Vol. XXI, 1929. London: His Majesty's Stationery Office. (Pp. 448.) Price £1 2*s.* 6*d.*

Handbuch der Experimentalphysik. Wien-Harms. Band 24. 2. Teil. Röntgenspektroskopie von Axel E. Lindh, Privatdozent an der Universität Lund. Leipzig: Akademische Verlagsgesellschaft m.b.H., 1930. (Pp. vii + 436, with 197 figures.) Price 40M. Bound, 42M.

Molecular Spectra and Molecular Structure. A General Discussion held by the Faraday Society. September 1929. (Pp. 611–954.) Price 15*s.* 6*d.* net.

Quantum Mechanics. By Edward U. Condon, Ph.D., Professor of Theoretical Physics, University of Minnesota, and Philip M. Morse, Ph.D., Porter Ogden Jacobus Fellow in Physics, Princeton University. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1929. (Pp. ix + 250.) Price 15*s.* net.

Considérations sur la Structure du Noyau de l'Atome. Par Charles Janet. Beauvais: Imprimerie Départementale de l'Oise, 26 rue de Malherbe, 1929. (Pp. 44.)

Magnetism. By Edmund C. Stoner, Ph.D., Research Fellow of Emmanuel College, Cambridge. With a General Preface by O. W. Richardson, F.R.S. London: Methuen & Co., 36 Essex Street, W.C. (Pp. vii + 117.) Price 2*s.* 6*d.* net.

Wall Diagram showing Range of Electro-Magnetic Waves. Prepared by Vivian T. Saunders, M.A. London: John Murray, 50 Albemarle Street, W.1. Price 4*s.* 6*d.* net. Mounted on strong white linen in stout cardboard cylinder, 8*s.* 6*d.* Mounted on linen, folded, 10*s.* 6*d.* net; mounted on linen, varnished, on rollers, 12*s.* 6*d.* net.

Outlines of Biochemistry. The Organic Chemistry and the Physico-Chemical Reactions of Biologically Important Compounds and Systems. By Ross Aiken Gortner, Professor of Agricultural Biochemistry in the University of Minnesota. New York: John Wiley & Sons; London: Chapman & Hall, 1929. (Pp. xv + 793.) Price 30s. net.

The Effects of Moisture on Chemical and Physical Changes. By J. W. Smith, B.Sc., Ph.D. University College, London. London: Longmans, Green & Co., 1929. (Pp. xii + 235, with 44 figures.) Price 15*s.* net.

Simple Research Problems in Chemistry. For Junior Students. By F. Sherwood Taylor, M.A., B.Sc. London: William Heinemann, 99 Great Russell Street, W.C.1. (Pp. vii + 100.) Price 4*s.* Answers only, 1*s.* 6*d.*

The Kinetics of Chemical Change in Gaseous Systems. By C. N. Hinshelwood, M.A., F.R.S., Fellow of Trinity College, Oxford. Second Edition. Oxford: at the Clarendon Press, 1929. (Pp. 266.) Price 12*s.* 6*d.* net.

Experimental Physical Chemistry. By Farrington Daniels, J. Howard Mathews and John Warren Williams. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1929. (Pp. xvi + 175, with 132 figures.) Price 17*s.* 6*d.* net.

Diatomaceous Earth. By Robert Calvert, Chief Chemist, van Shaack Bros., Chemical Works, Chicago, Illinois. New York: The Chemical Catalog Company, 419 Fourth Avenue at 29th Street, 1930. (Pp. 251, with 70 figures.) Price $5 net.

The Conductivity of Solutions and the Modern Dissociation Theory. By Cecil W. Davies, M.Sc., A.I.C., Senior Lecturer in Physical Chemistry, Battersea Polytechnic. London: Chapman & Hall, 1930. (Pp. viii + 204.) Price 15*s*. net.

Sulphuric Acid and its Manufacture. By H. A. Auden, M.Sc., D.Sc. London: Longmans, Green & Co., 1930. (Pp. vii + 227, with 50 figures.) Price 16*s*. net.

Handbook of the Geology of Great Britain. A Compilative Work. Edited by J. W. Evans, C.B.E., D.Sc., F.R.S., and C. J. Stubblefield, Ph.D. London: Thomas Murby & Co., 1 Fleet Lane, Ludgate Circus, E.C., 1929. (Pp. xii + 556.) Price 24*s*. net.

Dana's Manual of Mineralogy for the Student of Elementary Mineralogy, the Mining Engineer, the Geologist, the Prospector, the Collector, etc. By William E. Forel, Professor of Mineralogy in the Sheffield Scientific School of Yale University. Fourteenth Edition. New York: John Wiley & Sons; London: Chapman & Hall, 1929. (Pp. x + 476, with 360 figures.) Price 20*s*. net.

Mineralogy. An Introduction to the Scientific Study of Minerals. By Sir Henry A. Miers, M.A., D.Sc., F.R.S. Second Edition Revised by H. L. Bowman, M.A., D.Sc. London: Macmillan & Co., St. Martin's Street, 1929. (Pp. xx + 678, with 761 figures.) Price 30*s*. net.

A Textbook of Geology. Part I—Physical Geology, by Louis V. Pirsson; Part II—Historical Geology, by Charles Schuchert. New York: John Wiley & Sons; London: Chapman & Hall, 1929. (Pp. vii + 488, with 322 figures.) Price 18*s*. 6*d*. net.

The Physiographical Evolution of Britain. By L. J. Wills, Sc.D., F.G.S., Lecturer in Geology in the University of Birmingham. London: Edward Arnold & Co., 1929. (Pp. viii + 376, with 154 figures.) Price 21*s*. net.

Sydney University Reprints. Series VI (Geology and Geography), Vol. II, Nos. 1–11, 1929.

Methods in Geological Surveying. By Edward Greenly, D.Sc., V.P.G.S., and Howel Williams, M.A., D.Sc., F.G.S. London: Thomas Murby & Co., 1 Fleet Lane, E.C.4. New York: D. van Nostrand Co., 250 Fourth Avenue, 1930. (Pp. xvi + 420, with 81 figures and 3 plates.) Price 17*s*. 6*d*.

Pathologie der Pflanzenzelle. Teil I. Pathologie des Protoplasmas von Ernst Küster, Professor der Botanik an der Universität, Giessen. Protoplasma-Monographien, Band III. Berlin: Verlag von Gebrüder Borntraeger, W. 35 Schöneberger Ufer 12n. 1929. (Pp. viii + 200, with 36 figures.) Price 15 marks.

The Plant Rusts (uredinales). By Joseph C. Arthur, in Collaboration with F. D. Kern, C. R. Orton, F. D. Fromme, H. S. Jackson, E. B. Mains, and G. R. Bisby. New York: John Wiley & Sons; London: Chapman & Hall, 1929. (Pp. v + 446, with 186 figures.) Price 32*s*. 6*d*. net.

Gnetales. By the late H. H. W. Pearson, Sc. D., F.R.S., Harry Bolus Professor of Botany at the South African College, Cape Town, 1903–16. Cambridge: at the University Press, 1929. (Pp. 194, with 89 figures and 3 plates). Price 18*s*. net.

Agricultural Research in 1928. Royal Agricultural Society in England. London: John Murray, Albemarle Street, 1929. (Pp. vii + 193.) Price 1*s*. net.

Growth and Tropic Movements of Plants. By Sir Jagadis Chunder Bose, M.A., B.Sc., LL.D., F.R.S., C.S.I., C.I.E. London: Longmans, Green & Co., 1929. (Pp. xxix + 447, with 229 illustrations.) Price 21*s*. net.

Flowers and Flowering Plants. An Introduction to the Nature and Work of Flowers and the Classification of Flowering Plants. By Raymond J. Pool, Ph.D., Professor of Botany and Chairman, Department of Botany, University of Nebraska. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1929. (Pp. xx + 378, with 190 figures.) Price 17*s*. 6*d*. net.

Hydrogen-ion Concentration in Plant Cells and Tissues. By James Small, D.Sc., F.R.S.E., Professor of Botany in the Queen's University of Belfast. Protoplasma-Monographien, Volume II. Berlin: Verlag von Gebrüder Borntraeger, W.35. Schöneberger Ufer, 12*a*, 1929. (Pp. xii + 421, with 28 illustrations.) Price 30M.

Recent Advances in Plant Physiology. By E. C. Barton-Wright, M.Sc., Lecturer in Botany in the University of London, King's College. London: J. & A. Churchill, 40, Gloucester Place, Portman Square, 1930. (Pp. xii + 352, with 51 figures.) Price 12*s*. 6*d*. net.

Plant Biology. An Outline of the Principles underlying Plant Activity and Structure. By H. Godwin, M.A., Ph.D., Fellow of Clare College and Demonstrator in Botany in the University of Cambridge. Cambridge: at the University Press, 1930. (Pp. x + 265, with 67 figures.) Price 8*s*. 6*d*. net.

Locusts and Grasshoppers. A Handbook for their Study and Control. By B. P. Uvarov, Senior Assistant, Imperial Bureau of Entomology. London: The Imperial Bureau of Entomology, 41 Queen's Gate, S.W.7, 1928. (Pp. xiii + 352, with 9 plates and 118 figures.) Price 21*s*. net.

More Gleanings from Nature's Fields. By W. P. Pycraft, Assistant Keeper, Zoological Department, British Museum (Natural History). London: Methuen & Co., 36 Essex Street, W.C. (Pp. xiv + 203, with 39 illustrations.) Price 7*s*. 6*d*. net.

The Evolution of Earth and Man. By Lorande Loss Woodruff and eleven others. Edited, with a Preface, by George Alfred Baitsell, Professor of Biology in Yale University. New Haven: Yale University Press, 1929. (Pp. xv + 476, with 83 illustrations.) Price 22*s*. 6*d*. net.

Experiments in Bird Migration. By William Rowan, University of Alberta, Edmonton, Alberta. Boston: Printed for the Society from the William Brewster Fund, October 1929. Proceedings of the Boston Society of Natural History, Vol. 39, No. 5. (Pp. 151–208, plates 22–32.)

Agricultural Entomology. By D. H. Robinson, B.Sc., Head of the Biology Department, Harper Adams Agricultural College, Newport, Shropshire, and S. G. Jary, B.A., Advisory Entomologist, Southern Province, and Lecturer in Economic Entomology in the University of Reading. London: Duckworth, 3 Henrietta Street, W.C.2. (Pp. xi + 314, with 149 figures.) Price 15*s*. net.

The Oligochaeta. By J. Stephenson, M.B., D.Sc., Lecturer in Zoology in the University of Edinburgh. Oxford: at the Clarendon Press, 1930. (Pp. xvi + 978, with 242 figures.) Price 60*s*. net.

Embryology and Evolution. By G. R. de Beer. Oxford: at the Clarendon Press, 1930. (Pp. vii. + 116, with 7 illustrations.) Price 5*s*. net.

Our Face from Fish to Man. A Portrait Gallery of our Ancient Ancestors and Kinsfolk, together with a Concise History of our Best Features. By William K. Gregory. Professor of Vertebrate Palæontology, Columbia University. With a Foreword by William Beebe. New York: The Knickerbocker Press; London: G. P. Putnam's Sons, 1929. (Pp. xi + 295, with 118 figures.) Price 18*s*. net.

Textbook of Zoology. By H. G. Wells, B.Sc., F.Z.S., F.C.P., and A. M. Davies, D.Sc. Revised and Rewritten by J. T. Cunningham, M.A., and W. H. Leigh-Sharpe, M.Sc. Seventh Edition. London: University Tutorial Press, High Street, New Oxford Street, W.C., 1929. (Pp. viii + 509, with 227 figures.) Price 8*s*. 6*d*. net.

The Internal Secretions of the Ovary. By A. S. Parkes, M.A., Ph.D., D.Sc. London: Longmans, Green & Co., 1929. (Pp. xv + 242, with 69 illustrations.)

The Essentials of Human Embryology. By Gideon S. Dodds, Ph.D., Professor of Histology and Embryology, School of Medicine, West Virginia University. New York: John Wiley & Sons; London: Chapman & Hall, 1929. (Pp. vii + 316, with 182 figures.) Price 20*s*. net.

Annals of the Pickett-Thomson Research Laboratory. Vol. V. The Pathogenic Streptococci. Their Rôle in Human and Animal Disease (continued). London: Baillière, Tindall & Cox, 8 Henrietta Street, Covent Garden, W.C.2; Baltimore, U.S.A.: The Williams & Wilkins Company, 1929. (Pp. xi + 376, with 46 plates.) Price 42*s*. net per volume.

Laboratory Outlines in Comparative Physiology. By Charles Gardner Rogers, Ph.D., Sc.D., Professor of Comparative Physiology in Oberlin College. London: 6 Bouverie Street, E.C.4, 1929. (Pp. vii + 130.) Price 7*s*. 6*d*. net.

Devils, Drugs, and Doctors. The Story of the Science of Healing from Medicine-Man to Doctor. By Howard W. Haggard, M.D. London: William Heinemann, 1929. (Pp. xxii + 405, with 132 figures and 12 plates.)

Comparative Neurology. A Manual and Text for the Study of the Nervous System of Vertebrates. By James W. Papez, B.A., M.D., Cornell University, Ithica, New York. Illustrations by Mrs. Pearl Sowden Papez. New York: Thomas Y. Crowell Company. (Pp. xxv + 518, with 315 figures.) Price $6.00.

Microscopic Pharmacognosy. By William Mansfield, A.M., Phar.D., Ph.G., New York: John Wiley & Sons; London: Chapman & Hall, 1929. (Pp. x + 211, with illustrations.) Price 15*s*. net.

Belt Elevators. By H. Davies. The Association of Engineering and Shipbuilding Draughtsmen. Session 1929–30. London: The Draughtsman Publishing Co., 96 St. George's Square, S.W.1. (Pp. 71.) Price 3*s*. post free.

Logic for Use. An Introduction to the Voluntarist Theory of Knowledge. By Ferdinand Canning Scott Schiller, M.A., D.Sc. London: G. Bell & Sons, 1929. (Pp. viii + 469.) Price 16*s*. net.

Fables and Satires. By Sir Ronald Ross, K.C.B., K.C.M.G., F.R.S. London: Harrison & Sons, 44 St. Martin's Lane, W.C.2. (Pp. iii + 71.) Price 7*s*. 6*d*. net.

Uplift in Economics. A Plea for the Exclusion of Moral Implications from Economists and the Political Sciences. By P. Sergant Florence, Professor of Commerce in the University of Birmingham. London: Kegan Paul, Trench, Trübner & Co., Broadway House, Carter Lane, E.C., 1929. (Pp. 100.) Price 2*s.* 6*d.* net.

Modern Cosmologies. A Historical Sketch of Researches and Theories concerning the Structure of the Universe. By Hector Macpherson, M.A., Ph.D., F.R.S.E., F.R.A.S. London: Oxford University Press, 1929. (Pp. 131, with 12 plates.) Price 7*s.* 6*d.* net.

High-voltage Cables. By L. Emanueli, M.I.E.E., M.Amer.E.E. With an Introduction by Professor C. L. Fortescue, M.A., M.I.C.E., M.I.E.E. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1929. (Pp. 107, with 81 figures.) Price 1*s.* 6*d.* net.

The Identity Theory. By Blamey Stevens. Manchester: 38 Mauldeth Road, West Withington. (Pp. 32.) Price 1*s.*

Coal Carbonisation. By R. Wigginton, M.Sc., A.R.C.S., Lecturer in Fuel Technology in the University of Sheffield. London: Baillière, Tindall & Cox, 8 Henrietta Street, Covent Garden, W.C., 1929. (Pp. x + 287, with 47 figures.) Price 21*s.* net.

Météorologie du Relief Terrestre, Vents et Nuages. Par Albert Baldit. Ancien Chef du Service Météorologique du Groupe des Armées du Centre. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1929. (Pp. xii + 328, with 128 figures.) Price 70 fcs. net.

Bibliografio de Internacia Lingvo ellaboris kaj Komentaris. By P. E. Stojan, licencito en sciencoj. Genève: Bibliografia Servo de Universala Esperanto-Asocio, Tour de l'Ile, 1929. (Pp. 560.)

Photoelectric Cells, Their Properties, Use, and Applications. By Norman Robert Campbell and Dorothy Ritchie, Members of the Research Staff of the General Electric Company, Ltd., Wembley. London: Sir Isaac Pitman & Sons, Parker Street, Kingsway, W.C.2, 1929. (Pp. vii + 209.) Price 15*s.* net.

Report of the Building Research Board for the Year 1928. Department of Scientific and Industrial Research. London: Published under the Authority of His Majesty's Stationery Office, 1929. (Pp. viii + 141.) Price 3*s.* 6*d.* net.

Abstracts of Dissertation for the Degree of Doctor of Philosophy. Oxford: at the Clarendon Press, 1928. (Pp. vol. I, viii + 144; Vol. II, vii + 179.) Price 5*s.* net each.

Individual Understanding. A Layman's Approach to Practical Philosophy. By Emile Garcke. London: Electrical Press, Fisher Street, Southampton Row, 1929. (Pp. xi + 383.)

Leçons sur la Théorie des Tourillons. Par Henri Villat. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1930. (Pp. 300.) Price 65 fcs. net.

Wanderings in Wild Australia. By Sir Baldwin Spencer, K.C.M.G., F.R.S., M.A., Litt.D. London: Macmillan & Co., St. Martin's Street, 1928. (Pp. Vol. I, xxvii + 455, with 281 figures; Vol. II, xiv + 457–930, figures 282–573.) Price 42*s.* net.

Ethnologische Studien. Herausgegeben von Dr. Fritz Krause, Direktor des Museums für Völkerkunde zu Leipzig. Leipzig: Verlag der Asia Major, 1929. (Pp. 133.) Price 10M.

The Physiology of Oral Hygiene and Recent Research. With Special Reference to Accessory Food Factors and the Incidence of Dental Caries. By J. Sim Wallace, D.Sc., M.D., L.D.S., Lecturer on Preventive Dentistry, King's College Hospital. Second Edition. London: Baillière, Tindall & Cox, 7 Henrietta Street, Covent Garden, 1929. (Pp. vii + 228.) Price 10*s*. 6*d*. net.

Possession, Demoniacal and other among Primitive Races, in Antiquity, the Middle Ages, and Modern Times. By T. K. Oesterreich, Professor at the University of Tübingen. London: Kegan Paul, Trench, Trübner & Co., 68 Carter Lane, E.C., 1930. (Pp. xi + 400.) Price 21*s*. net.

Philosophy by Way of the Sciences. An Introductory Textbook. By Ray H. Dotterer, Ph.D., Professor of Psychology in Franklin and Marshall College. New York and London: The Macmillan Company, 1929. (Pp. xv + 469.) Price 10*s*. 6*d*. net.

Identity and Reality. By Emile Meyerson. Authorised Translation by Kate Loewenberg. London: George Allen & Unwin; New York: The Macmillan Company. (Pp. 495.) Price 16*s*. net.

Bacterial Metabolism. By Marjory Stephenson, M.A., Associate of Newnham College, Cambridge. London: Longmans, Green & Co., 1930. (Pp. xi + 320, with 34 figures.) Price 18*s*. net.

Novial Lexike, International Dictionary. By Otto Jespersen. London: George Allen & Unwin, Museum Street. (Pp. 251.) Price 3*s*. 6*d*. net.

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